This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

3-0A

Journal

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS:

ILLUSTRATED WITH ENGRAVINGS.

BY WILLIAM NICHOLSON.

VOL. I.

ASTOR LIBRARY
NEW-YORK

LONDON:
PRINTED FOR G. G. AND J. ROBINSON, PATERNOSTER-ROW.
M.DCC.XCVII.

PREFACE.

THERE is ſcarcely a more difficult taſk than to convey an adequate notion of a plan of ſome extent, within the limits of a ſhort diſcourſe. Whatever may be the promiſes, the hopes, or the intentions of an Author or Editor, the world, as in juſtice it ought, will ſuſpend its judgment till the actual performance ſhall afford the knowledge which is indiſpenſable for that purpoſe. Correſpondents will arrange for themſelves ſuch materials as they think fit to publiſh, without committing to memory any of the outlines which the authors of Journals may have drawn up. For theſe reaſons, little more need be ſaid concerning the plan, than is preſented in the title-page. Whatever the activity of men of ſcience or of art may bring forward, of invention or improvement, in any country or nation, within the poſſibility of being procured, by means as reſpectable as the motives that call for them, ſhall appear in this Journal; either in the form of ſhort notices, or the full deſcriptions of their reſpective authors, or the more ample report deduced from actual viſitation and enquiry. The relative magnitude of each object will eſtabliſh the rule from which either of theſe modes will be adopted. Arrangements have already been made, channels of communication opened, and other correſpondences are in proſpect, which muſt increaſe, in value and extent, proportionate to the importance and curioſity of the ſubjects [illegible] diſplayed in this work, and the impartiality and care with which they ſhall be treated.

In a former Addreſs to the Public the Author has mentioned the advantages in regard to accuracy and fidelity which he apprehends muſt reſult from the conductor of a work of this nature becoming, in a certain degree, reſponſible by name for its contents: he has alluded to the general tenor of his purſuits, as known to the Public, and in ſome reſpect qualifying him to engage in ſuch a taſk; and he has deprecated the ſuppoſition of any vain pretence to ſuperiority, by remarking, that no one could ſerve the Public in this way if he were to wait with the abſurd hope of firſt bringing his own knowledge to a ſtate of perfection. This is all that need be ſaid relating to himſelf.

The leading character on which the ſelection of objects will be grounded is utility; and next to this, novelty and originality. The Author's reſearches and collections, and thoſe of his friends, will afford a conſiderable portion of new and curious matter, ſufficient to render the work intereſting, even to that extreme few who are ſo fortunate as to have acceſs to all the expanded ſources of philoſophical intelligence. But in the department of perfectly original matter, much of prudence is required to be exerciſed, in order that the claim of novelty may not operate to the excluſion of much more valuable and important ſubjects. It is certain that, if every article in a journal of ſcience were to be profeſſedly original [*], it would be a work of comparatively much leſs value to Philoſophers and the Public. Such a plan would in a great meaſure defeat the attempt to convey the beſt diſcoveries of our cotemporaries in the moſt authentic manner, namely, in their own words. And when we reflect on the very limited circulation of academical Tranſactions, from their price, their number, their extent, diſtance of publication, difference of language, labour of peruſal, and the efforts of mental abridgment, it is alſo certain that, from one or other of theſe cauſes, even the beſt memoirs they contain muſt continue unknown to a very large claſs of men of ſcience. Under the impreſſion of theſe truths, while no exertions will be ſpared to obtain immediate original information, concerning any object preſented to the world in this collection, the aim at originality muſt nevertheleſs be ſubordinate to the leſs eaſy but more eſſential requiſites of public utility and intereſting reſearch. Whenever, in the progreſs of inveſtigation, diſcoveries thus buried from the knowledge of the world, ſhall preſent themſelves, the rational plan of a public journal will require them to be brought forward, though years may have elapſed ſince their firſt publication. It would be eaſy to exhibit a numerous catalogue of errors retained in the works of authors of the firſt eminence, from the want of ſuch general communication.

---

[*] Lewis's Philoſophical Commerce of Arts, and various other publications of inferior note, have failed of public ſupport, chiefly from this circumſtance in their plan.

After this ſhort account of the materials, it might be expected that ſomething ſhould be ſaid of the manner in which this Journal is to be conducted. On this occaſion it would not be difficult to point out imperfections in the works of others, and promiſe to avoid them; or to enumerate the various requiſites which ought to characteriſe the journaliſt of ability and integrity, and promiſe to exert them. Such ſpecific engagements may have their value, and are probably entered into with great ſincerity. But it appears more natural and eaſy to leave every individual of principle and underſtanding to imagine what ought to be done. As the events preſent themſelves, the proper mode of conduct will itſelf ſtand forward and leave no cauſe for heſitation.

Yet, while the Author himſelf avows the decided purpoſe of exhibiting his ſources of communication in the moſt unreſerved manner, it is no leſs proper that he ſhould pay every attention to the rights of others. He will never take upon him to decide for another, how far he ſhall or ſhall not come forward as the author of any communications he may receive, and ſtill leſs will he dare to infringe that firſt and moſt ſacred property which men hold in the products of their own underſtanding. It is not, in his opinion, for any man or ſet of men to decide what are the caſes of moral obligation in an inventor to communicate his diſcoveries, or his private arrangements in buſineſs. Philoſophers, manufacturers, and others, may therefore reſt aſſured, that he will publiſh no communications he may acquire either in converſation or otherwiſe, nor mention the name of any individual in his writings; unleſs the object be already before the Public, or unleſs he ſhall receive permiſſion directly, or moſt clearly implied, to that effect. But in every caſe of anonymous communication he will be careful, according to the nature of the object, to mention the degree of credit he himſelf is diſpoſed to attach to the ſeveral facts.

Such papers as have no name or ſignature are written by himſelf. This will in general appear alſo from the manner, chiefly with reſpect to the uſe of the firſt perſon ſingular. The firſt perſon plural will probably be uſed in ſuch occaſional ſentences as refer to the Author and the Reader in the joint conſideration of any ſubject; but never in the manner adopted by anonymous writers, to denote the concealed individual.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL 1797.

ARTICLE I.

The Principles and Application of a New Method of conſtructing Achromatic Teleſcopes.

By ROBERT BLAIR[*], *M. D.*

AFTER a ſhort hiſtorical retroſpect to the diſcoveries of the unequal refrangibility of light by Newton, and the ſubſequent correction of its effects in optical inſtruments by Dollond, together with the remedy which is afforded in compound lenſes for the aberration ariſing from ſphericity, our author remarks, that it was expected by men of ſcience that an increaſe of the aperture and power of the refracting teleſcope would be the neceſſary conſequence of ſuch important ſteps towards the perfection of its theory. Theſe expectations have not hitherto been fully anſwered: for it is certain, that no Achromatic Teleſcopes have yet been made of equal aperture with the ſingle object-glaſſes of Huyghens and others, nor in this reſpect comparable to reflectors, though the errors of workmanſhip are much more noxious in theſe laſt than in lenſes.

The general anſwer made by artiſts to enquiries of this nature is, that the fault lies in the imperfection of glaſs, and particularly in the denſe glaſs known by the name of flint-glaſs.

The imperfection of glaſs, for optical purpoſes, ariſes partly from its opacity and colour, but chiefly from irregularities in the refractive denſities of its parts. The reſearches of chemiſts and manufacturers to remove this defect have been conſiderable, but hitherto without much ſucceſs.

From the conſideration that it is not impoſſible to introduce a fluid medium to ſupply the place of one of the lenſes in the compound object-glaſs, Dr. Blair was led to the experimental enquiry, whether nature afforded fluids poſſeſſed of the requiſite qualities.

---

[*] The original paper, from which I have made the above abridgement or ſelection of parts, is inſerted in the Tranſactions of the Royal Society of Edinburgh, vol. ii. It occupies 76 pages. N.

To aſcertain the mean refractive and diſperſive qualities of fluids, the Doctor made uſe of two kinds of apparatus. The firſt, intended to afford a groſs knowledge of their properties, conſiſted of an apparatus of priſms. In the ſecond method, where the fluids promiſed to be of practical uſe in Optics, they were more critically examined by means of lenſes, in which the effect, from being magnified, is rendered more conſpicuous.

The priſmatic apparatus conſiſts of a ſmall, equi-angular, three-ſided priſm, of braſs. Through this priſm, and parallel to one of its ſides, are bored two holes, at a ſmall diſtance from each other, equal in ſize to the pupil of the eye. The ſides of the priſm are ground flat, and there are two pieces of glaſs with parallel ſides, of the ſame dimenſions as the ſides of the priſm. There are alſo priſms of the ſame ſize, and with the ſame angles, of different kinds of glaſs; and ſome crown-glaſs priſms with ſmaller angles, which, by being applied to the large priſm, or to each other, vary the refracting angle at pleaſure.

When it is propoſed to try the properties of any fluid, one of the ſmall plates of glaſs is applied over the holes on the ſide of the braſs priſm. A few drops of the fluid are then dropped into the hole, and, when it is full, the other plate is laid over the holes upon the oppoſite ſide, and the whole is ſecured by tying a piece of packthread round the ends. One of the glaſs priſms is now to be applied to the braſs priſm contiguous with one of the parallel plates, the refracting angles of the two priſms being placed in oppoſite directions ſo as to form a ſmall parallelopiped.

Nothing farther is neceſſary than to apply the eye to the hole which contains the fluid, in ſuch a way as to obſerve through it any bright well-defined object. The bars of the window anſwer the purpoſe very well in the day-time, and the moon or a candle in the night. The intention of the two holes is for the ſake of greater expedition. The properties of two fluids may thus be examined and compared at the ſame time. As the priſmatic portion of fluid and the glaſs priſm have equal refracting angles, and refract in oppoſition to each other, it will eaſily be underſtood, that, if the object ſeen through the two priſms coincides with the ſame object ſeen directly, the mean refractive denſity of both mediums will be the ſame. When this is the caſe, if the object ſeen through theſe priſms appears free from priſmatic colour, the diſperſive power of the fluid medium is alſo the ſame with the diſperſive power of the glaſs priſm. But otherwiſe they will be different.

Thoſe mediums, it is to be obſerved, are ſaid to have the ſame mean refractive denſity, which, under equal obliquities of incidence, equally refract the mean refrangible rays; and two mediums are ſaid to have the ſame diſperſive power, which produce an equal inclination of rays of the ſame colour to the mean refrangible ray, when the whole refraction of the mean refrangible ray is equal in both.

When an object ſeen through the equal wedges of glaſs and fluid appears coloured, one of the ſmaller glaſs wedges is to be applied, and ſhifted, till the object appears colourleſs. It is eaſy to diſtinguiſh, by the order in which the priſmatic colour lies, whether the ſmall priſm is to be applied in ſuch a way as to increaſe the diſperſion of the rays occaſioned by the fluid ſo as to enable it to counterbalance that of the glaſs, or whether the refracting angle of the glaſs priſm requires to be enlarged, to enable it to counteract the diſperſion occaſioned by the fluid.

By proceeding in this way to ſhift the angles of the priſms till, firſt, the direct and refracted images of an object coincide, without regarding the colour, and, next, till the refracted image appears colourleſs, without regarding the coincidence, the ratio of the mean refractive and diſperſive powers of that kind of fluid, and that kind of glaſs, with which the experiments are made, will be obtained from the angles of the priſms being given in both caſes.

In order to aſcertain the abſolute refractive denſity of glaſs, or any other medium, that is to ſay, the general ratio of the ſines of the angles of incidence to the ſines of the angles of refraction of the mean refrangible ray which obtains in that medium, the Doctor took a direct method, ſimilar in principle to that employed by Sir Iſaac Newton, and deſcribed by him in the ſeventh propoſition of the firſt book of his Optics, and likewiſe in his Optical Lectures, p. 54; but which, as he juſtly remarks, will be found much eaſier, and perfectly accurate.

Inſtead of cauſing the rays to paſs through the ſights of a large and accurate quadrant at the diſtance of ten or twelve feet, as directed by Sir Iſaac Newton, the Doctor employed a Hadley's quadrant, in the following manner:

Plate I. fig. 1—I repreſents the index-glaſs, and H the horizon-glaſs of a Hadley's quadrant. SI repreſents a ſolar ray incident on the index-glaſs, thence reflected to the horizon-glaſs H, and from it to the eye at E. The line *ſg* repreſents another ſolar ray, incident on the priſm P, and through it refracted to the eye at E. When the priſm is turned ſlowly round ts axis till the ſpectrum G appears at its greateſt height, this is its proper poſition. The ngle formed by the direct and refracted ray is then the leaſt poſſible, and the angles of ncidence and emergence are equal. Let the priſm be ſecured in this poſition. A ſlight ſpection of the figure will ſhew, that when the reflected and refracted images of the ſun re made to coincide, the angle marked by the index of the quadrant is the ſame which the cident ray *ſg* forms with the refracted ray PE produced. For SZH is the angular diſnce of the ſun, and his doubly reflected image marked by the index; and the angle *ſg* G, hich the ray incident on the priſm forms with the refracted ray produced, is equal to it; and SI being parallel, and PZ and HZ being coincident.

The manner in which the ratio of the ſines of the angles of incidence and refraction may computed, from the above angle and the refracting angle of the priſm being given, is ly explained in the celebrated works which have juſt been quoted.

The Doctor here remarks, that as it is the ratio of refraction of the mean refrangible which is wanted, the centre of the reflected image of the ſun ought to be made to coin- with the centre of the coloured ſpectrum, as repreſented in the figure; and if, inſtead his, the coincidence be formed with the moſt or leaſt refrangible ray, or any of the inediate rays, it will be the ratio of refraction of theſe rays, and not of the mean refran- ray, which will be found from the obſervation. Hence this method might be prac- for determining the diſperſive power, as well as the mean refractive denſity of any parent ſubſtance, whether ſolid or fluid; but the Doctor has preferred a combination iſms or lenſes, becauſe it is the relative ratios, more than the abſolute ratios, which are immediately wanted.

this priſmatic apparatus, the optical properties of a great variety of fluids were exa- l. The ſolutions of metals and ſemi-metals proved in all caſes more diſperſive than -glaſs. Some of the ſalts, as, for example, ſal-ammoniac, greatly increaſed the diſ- power of water. The marine acid diſperſes very conſiderably, and this quality in- s with its ſtrength. The moſt diſperſive fluids were accordingly found to be thoſe in this acid and the metals were combined. The chemical preparation called *cauſticum* *antimoniale*, or *butyrum antimonii*, in its moſt concentrated ſtate, when it has juſt attracted ſufficient humidity to render it fluid, poſſeſſes the quality of diſperſing the rays in ſuch an aſtoniſhing degree, that three wedges of crown-glaſs are neceſſary to remove the colour produced by one wedge of this ſubſtance, of an equal refracting angle oppoſed to them. The great quantity of the ſemi-metal retained in ſolution, and the highly concentrated ſtate of the marine acid, are conſidered by the author as the cauſe of this ſcarcely credible effect.

Corroſive ſublimate mercury, added to a ſolution of *ſal ammoniacum* in water, poſſeſſes the next place to the butter of antimony among the diſperſive fluids which he examined. It may be made of ſuch a degree of ſtrength as to require a wedge of crown-glaſs of double the refracting angle, to remove the colour which a priſm of it produces. The mercury and marine acid contained in this ſolution are manifeſtly the cauſe of its diſperſive power: for neither the water, nor the volatile alkali, which are its other component parts, will be found capable, if tried ſeparately, of contributing towards this effect.

The eſſential oils were found to hold the next rank to metallic ſolutions, among fluids which poſſeſs the diſperſive quality. The moſt diſperſive were found to be thoſe obtained from bituminous minerals, ſuch as the native petrolea, pitcoal, and amber. When the refraction is without colour, the proportion of the refracting angle of a priſm of theſe, to the refracting angle of a priſm of crown-glaſs acting in oppoſition, is about two to three. The diſperſive power of the eſſential oil of ſaſſafras, is not much inferior to theſe. The eſſential oil of lemons, when genuine, requires the refracting angles of the priſms neceſſary to produce a colourleſs refraction, to be as three to four. In oil of turpentine, this proportion is as ſeven to ſix; and the eſſential oil of roſemary is ſtill leſs diſperſive.

Some expreſſed oils, which were examined, were found not to differ ſenſibly in diſperſive power from crown-glaſs; which was alſo the caſe with rectified ſpirits, and with nitrous and vitriolic ether.

Having been thus ſucceſsful beyond his hopes in diſcovering fluids capable of removing the great imperfection of teleſcopes ariſing from the different refrangibility of lights, the next object of the author was to ſelect from this variety thoſe that ſeemed beſt adapted to optical purpoſes.

There was no doubt that thoſe mediums which moſt diſperſe the rays were *cæteris paribus* to be preferred. It will alſo be found, when the method of correcting thoſe errors which ariſe from the ſpherical figures of lenſes comes to be conſidered, that there is apparently an advantage in uſing a diſperſive medium, whoſe mean refractive denſity exceeds the mean refractive denſity of crown-glaſs.

As the antimonial cauſtic poſſeſſes both theſe advantages in a degree far beyond what was to be expected in any fluid, ſome of it was included between two double convex lenſes of crown-glaſs, whoſe radii of convexity were as two to one. The leaſt convex ſides of theſe were turned toward each other, and they were kept at a proper diſtance by means of a glaſs ring. The cavity was then filled with the ſtrongeſt butter of antimony. Here it is evident, that there is a concave lens of the diſperſive fluid acting in oppoſition to the two convex lenſes of crown-glaſs, and that the proportion of the radii of theſe is the ſame which was found by the priſms to correct the colour, namely, three wedges of crown-glaſs to one of the butter of antimony.

This compound object-glaſs being put into a tube, an eye-glaſs was applied, and, according to expectation, the colour was found to be removed. But the Doctor was ſurpriſed to find, on directing the inſtrument to a planet, and uſing a deep eye-glaſs, that this fluid, in its highly concentrated ſtate, was ſubject, like flint-glaſs, to great irregularities in its denſity, diſcoverable by ſtreams of light, like comets' tails, iſſuing in different directions from the diſk of Venus, which was the planet obſerved. By ſhaking the object-glaſs, theſe might be in a great meaſure removed, but ſoon returned; and, after ſtanding all night, broad veins, in different parts of the included fluid, were perceptible to the naked eye.

It was neceſſary, on this account, to reject very denſe fluids. The antimonial preparation
was found to be reducible to a ſufficient degree of fluidity, by mixing it with ſpirit of wine
r vitriolic ether, into which a ſmall quantity of the marine acid had been previouſly dropped.
This prevents any precipitation of the ſemi-metal in the form of a calx. In this diluted
rm, either this preparation, or the ſolution of corroſive ſublimate mercury alone in ſpirit
f wine or in water, with the addition of crude *ſal ammoniacum*, may be employed for pro-
ucing refraction without colour, and without being ſubject to that irregularity of denſity
which flint-glaſs and very denſe diſperſive fluids are ſubject.

But as ſolutions of ſaline ſubſtances in this diluted ſtate do not differ materially in diſ-
rſive power from the eſſential oils, theſe two kinds of fluids may be uſed indifferently.

In one caſe, however, our author remarks, that water, or vitriolic ether, impregnated with
imony or mercury, will have the advantage from being leſs denſe than eſſential oils, and
t is, where it is required to produce a ſingle refraction, in which there ſhall be no differ-
e of refrangibility of heterogeneal light. As this expreſſion might ſound ſtrange in the
of Opticians, the Doctor has employed a conſiderable part of his paper to explain what
eant by it. He ſhews, that there are caſes of ſingle refraction in which *the violet rays*
the leaſt refrangible; or in which *all the rays are equally refrangible;* or in which the *red*
are refracted from the perpendicular, and the violet rays towards the perpendicular, while the
refrangible rays ſuffer no refraction.

heſe poſitions muſt, no doubt, at firſt conſideration, appear paradoxical; but their ſin-
ity will vaniſh on attending to two circumſtances: 1ſt, That theſe refractions, though
, are not effected by the ſimple agency of one medium on the confine of a vacuum, but
e difference of the contrary actions of two mediums at their common ſurface; and,
That Opticians, from the habit of contemplating the crown and flint glaſſes, have
ted the notion of a greater diſperſive power with that of a greater power of mean
tion.—But our author, having aſcertained various facts in which the greater power of
ion accompanies the leſs mean refractive power, has in conſequence ſhewn that
romatic lens may be conſtructed, in which all the refractions are made in the ſame
n *. The caſes of ſingle refraction here mentioned are very perſpicuouſly and at
ngth ſhewn, with ſuitable diagrams, as well from the general facts before enumerated,
aſſumed powers of attraction of the mediums on the rays of light, after the ſup-
of Sir Iſaac Newton. For the ſake of brevity, however, I ſhall give the ſubſtance
xplanation, without particularly attending to the order of arrangement.

---

object-glaſs formed of oil of turpentine included between two double convex lenſes, the radii of
vexities are as ſix to one, and the deep ſides inwards, there are four refractions, all towards the axis;
rration, from difference of refrangibility, is removed. This compound lens has twenty inches focal
one inch and half aperture. Its performance as a teleſcope is not contemptible. B.

If a pencil of compounded light be imagined to paſs obliquely out of the plane ſurface of any denſe medium into a vacuum, it is well known that the refraction will be made from the perpendicular, the violet rays being moſt refracted and the red leaſt, ſo that theſe rays will be inclined to each other. But ſuppoſe, again, that inſtead of the rays being ſuffered to paſs into the vacuum, another medium were applied, having a plane ſurface in contact with that of the former, and that its power of diſperſion were ſuch as preciſely to counteract the diſperſive power exerted on the rays by the former medium: in this caſe it is evident that the pencil of light would paſs forward colourleſs. Nevertheleſs, as the diſperſive power may be equal in two mediums which differ in mean refractive power, it is no leſs clear, that refraction will take place wherever theſe two powers are not preciſely equal; and that either towards the perpendicular, if the power, or, as it is called, refractive denſity of the ſecond medium, be greater than that of the firſt, or from it if leſs.

In the next place, ſuppoſe the two mediums not to agree in diſperſive power, but that the ſecond medium diſperſes more than the other. The violet ray will not only be prevented from ſeparating on the one ſide of the red, but, by the exceſs of diſperſive power, it will be drawn ſo far as to make an angle with that ray on the other ſide, and the diſperſion will be made the contrary way. If the mean refraction of the ſecond medium exceed that of the firſt, the whole pencil will be bended towards the perpendicular, and towards the ſame region as the diſperſion carried the violet ray; ſo that the effect will be ſimilar (though leſs in power) to that of a common refraction and diſperſion by a ſingle denſe medium in vacuo. This happens in the tranſition from crown-glaſs into flint-glaſs. But if the mean refractive power of the ſecond medium be leaſt, the pencil will be bended from the perpendicular, though the violet ray will notwithſtanding continue neareſt the ſame perpendicular. The red ray will therefore be the moſt deflected, and the violet leaſt. This effect takes place when light is refracted in the confine of crown-glaſs and oil of turpentine, and alſo of many other fluids. If the refractive powers of the two mediums upon the mean ray be equal, the direction of this ray will continue unaltered at its tranſition from the one to the other. But if the diſperſive power differs, the extreme rays will form an angle, of which the red rays will be either at the one or the other limit, according to circumſtances. For example, if the diſperſive power of the firſt medium be greateſt, the angle will merely be diminiſhed by the action of the ſecond medium; ſo that the violet ray will be the moſt, and the red the leaſt, refracted from the perpendicular; as would have happened (though more ſtrongly) in the tranſition into a vacuum. But if, on the contrary, the diſperſive power of the ſecond medium were greateſt, the violet ray will be carried more towards the perpendicular than the red by the difference of the two powers. This caſe of refraction is found to take place in the confine of crown-glaſs and butter of antimony, when the latter is ſo far diluted as that both mediums equally refract the mean ray under equal angles of incidence.

In the caſe of refraction without colour, as before explained, the power of diſperſion in the ſecond medium was aſſumed to be ſuch as preciſely to counteract the effect of the firſt; that is to ſay, not only with regard to the extreme red and violet rays, but alſo with regard to the mean and all the intermediate rays. But Doctor Blair's Experiments ſhew, that, in mediums which diſperſe the light but little, the green is the mean refrangible ray; that in by far the greateſt number of more diſperſive mediums, including flint-glaſs, metallic ſolutions, and eſſential oils, the green light is not the mean refrangible order, but forms one of the leſs refrangible orders of light, being found in the priſmatic ſpectrum nearer to the deep red than to the violet; and that in another claſs of diſperſive mediums, which includes the muriatic and nitrous acids, this ſame green light becomes one of the more refrangible orders, being now found nearer to the extreme violet than to the deep red. Whenever, therefore, the light paſſes out of one of theſe three claſſes of mediums into another of a different claſs, the diſperſive powers will not accurately counteract each other, even though they may be adopted to cauſe the extreme red and violet rays to become parallel; but the rays which would occupy the interior parts of the ſpectrum will be diſperſed, and that in a greater degree the more remote they are from the extremes.

Theſe ſeveral caſes of refraction were likewiſe tried with compound object-glaſſes, which ſhew the effect better than priſms. Thus, if a plano-convex lens have its plane ſide turned toward a diſtant object, the rays will enter it, as to ſenſe, perpendicularly, and will therefore ſuffer no refraction. If the convex ſurface of this lens be brought in contact with a fluid of leſs mean refractive denſity than the glaſs, but exceeding it in diſperſive power in that degree which occaſions an equal refraction of all the rays, all theſe rays will then be converged to the ſame point, which are incident at the ſame diſtance from the axis of the lens. The focal diſtance of this compound lens will be greater or leſs, in proportion to its radius of convexity, and to the difference of refraction between it and the fluid made uſe of. While the fluid is confined on one ſide by the plano-convex lens, let the lens which is rought in contact with it on the oppoſite ſide have one of its ſides ground convex, and he other concave; the radii of their ſphericities being equal to the focal diſtance at which the ays are made to converge by the refraction which takes place when light paſſes from the lano-convex lens into the fluid. It is manifeſt that the light will now both enter into this ompound lens, and emerge from it, perpendicularly, and will therefore ſuffer no refrac- on, except in the confine of the convex ſide of the plano-convex, and the diſperſive fluid here all the rays are equally refrangible. A compound lens of this kind is repreſented in e ſecond figure, Pl. I. which requires no farther explanation; excepting only, that, inſtead being ſpherical, it is repreſented with that curvature which converges homogeneal rays cident at all diſtances from the axis to the ſame point. If the required curvature could be ven to lenſes with ſufficient accuracy, this figure ſeems to repreſent as perfect a conſtruc- n of the object-glaſs of a teleſcope as can be deſired. But there is reaſon to think at a ſpherical figure may be communicated, not only much eaſier, but with greater accu- y than a ſpheroidal or hyperboloidal, which would then be required; and even if this ficulty could be got over, there would ſtill remain a fundamental fault in the theory. fore relating the obſervations by which this was detected, Dr. Blair explains the method removing the ſpherical aberration by a combination of convex and concave lenſes. , next to the indiſtinctneſs ariſing from the unequal refrangibility of light, this aberra- occaſioned by the ſpherical figures of lenſes is the great obſtacle to the advancement the powers of viſion. The aberration from the ſpherical figure has been treated of, ll the variety of caſes which can occur in ſingle glaſs lenſes by the great Hugenius in his ptrics, a poſthumous work. He there demonſtrates, that the quantity of this aberration ery different in different lenſes of the ſame focal diſtance, according to the convexities oncavities of their two ſides, and the manner in which they are expoſed to parallel rays.

In convex lenſes, thoſe rays which paſs at a diſtance from the axis are converged to a point nearer to the lens than its geometrical focus. The diſtance between the point at which the external ray of a pencil incident on a lens interſects its axis, and the geometrical focus, is called the linear aberration of that lens.

Hugenius demonſtrates, that when a plano-convex lens is expoſed to parallel rays, with its plane ſide towards them, this aberration will amount to four times and a half the thickneſs of the glaſs. By the thickneſs of a convex lens, is meant its greateſt thickneſs in the middle, after ſubtracting its thickneſs, if it has any, at the outer edge; and by the thickneſs of a concave lens, is meant its thickneſs at the external edge, after deducting its thickneſs in the middle.

On turning the convex ſide of the lens towards the light, the linear aberration will only exceed the thickneſs of the lens by one ſixth part.

When both ſides of a lens are convex, and the proportion of their convexities is as one to ſix; if the moſt convex ſide be expoſed to parallel rays, the aberration will exceed the thickneſs of the lens one fourteenth, which is the ſmalleſt poſſible aberration of any convex lens.

If it is required to increaſe the aberration, this may be done by grinding one ſide of the lens convex, and the other ſide concave, to a longer radius. Such a lens, with its concave ſide turned towards parallel rays, will have more aberration than any plano-convex or double convex lens of the ſame focal diſtance.

Hugenius proceeds to ſhew, that the ſame aberration is produced by concave lenſes as by ſimilar convex ones. When a plano-convex lens is expoſed to parallel rays with its plane ſide outward, the external ray of the pencil being produced backward, after refraction, will interſect the axis of the lens nearer to it than its focus by four times and a half the thickneſs of the lens. But if its concave ſide be expoſed to the parallel rays, the aberration will only exceed the thickneſs of the lens one fourteenth part. A double concave, whoſe radii are as one to ſix, with the moſt concave ſide turned outward, diſperſes the rays with the leaſt aberration; and a concave meniſcus, with its convex ſide outward, produces more aberration than any plano-concave or double concave lens of an equal focal diſtance.

Theſe are ſufficient data for correcting the aberration from the ſpherical figure, in caſes where both a convex and concave lens are required in the conſtruction of the compound object-glaſs.

Fig. 3. Pl. I. Let AB repreſent a convex lens receiving a pencil of rays from the object S, and converging rays incident near the axis at ST to the point F, and external rays as SB to the point D, ſo that DF repreſents the greateſt linear aberration in this caſe.

Again, let GH (Fig. 4.) repreſent a concave lens receiving the parallel rays SH, RK, which it refracts in the lines HX and KV. This ray KV being produced backward, will interſect the axis of the lens nearly at the point N, which is called the virtual focus of the concave; and the external ray HX produced backward, will interſect the axis in ſome point P nearer to the lens than its focus PN, being the linear aberration.

It may here be obſerved, that the convex is in that poſition which produces the leaſt aberration, and the concave in the poſition which produces moſt aberration. Hence, to render the aberrations DF (Fig. 3.) and PN (Fig. 4.) equal, the focal diſtance of the convex muſt be much ſhorter than that of the concave; and if the diſtances of the points F and N, from the convex and concave lenſes, be required to be the ſame as repreſented in the figures, then muſt the object be placed much nearer to the convex. Hence the image of the near object S is repreſented at the ſame diſtance from the convex lens in Fig. 3, as the virtual focus of the concave in Fig. 4, where it is repreſented as receiving parallel rays which are ſuppoſed to come from an infinitely diſtant object.

Now when the diſtance between K and N, which is the point from which parallel rays are made to diverge by the concave lens, is equal to the diſtance between T and F, which is the point to which rays iſſuing from S are made to converge by the convex, and when the aberrations DF and PN are alſo equal; it will follow in this caſe, that if the two lenſes be placed contiguous, in the manner repreſented in the twelfth figure, parallel rays incident on theſe lenſes will be converged to the point S, without any aberration of the external ray.

For it is an axiom in optics, that if a ray of light after refraction be returned directly back to the point of incidence, it will be refracted in the line which was before deſcribed by the incident ray.

If, therefore, we conceive the whole of the light emitted from the point S (Fig. 3.) and converged by the convex lens towards the points D and F, to be returned directly back from theſe points, it will be accurately converged to the point S, whence it iſſued. Now the parallel rays SH, RK (Fig. 4.) after their emergence from the concave lens in the lines HX, KV, are preciſely in the ſame relative ſituation as the rays ſuppoſed to be returned directly back from F and D are in at their incidence on the convex; and therefore, when theſe lenſes are placed contiguous in the manner repreſented in the twelfth figure, parallel rays incident on the concave lens, and immediately after their emergence from it, entering the convex lens, will be accurately converged to the point S without any aberration.

This, which is the moſt ſimple caſe, will ſuffice to explain the nature of that aberration which ariſes from the ſpherical figures of lenſes, and a method of obviating it, by combining a convex and concave.

The demonſtration is perfect as far as regards the external ray, which is here repreſented paſſing from the external part of the concave into the external part of the convex, in immediate contact with it; and if the ſurfaces of the two lenſes, which reſpect each other, were either in contact or parallel, it would be true with regard to all the rays. But as this is not the caſe, there ariſes a ſmall ſecondary aberration, the effect of which only becomes ſenſible in large apertures.

Hence may be underſtood the reaſon why the indiſtinctneſs ariſing from the ſpherical figures of lenſes, may, in the common Achromatic Teleſcope, be more nearly removed in thoſe conſtructions of object-glaſſes in which three lenſes are employed, than in thoſe compoſed only of two; and alſo the advantages in this reſpect which may be derived from introducing fluid mediums which differ from glaſs in their mean refractive denſity, and in the quantity of aberration produced by their refractions. For it will be found upon computation, that when the fluid medium is rarer than glaſs, the aberration from the ſpherical figure is increaſed, and becomes greater in proportion as its denſity diminiſhes. Now, by making the denſity of the fluid medium approach nearer and nearer to the denſity of the glaſs with which it is in contact, we may increaſe the rarity of our refracting medium, or, which amounts preciſely to the ſame thing, diminiſh the difference of denſity of the two mediums at pleaſure.

It will appear from what has been explained, that the aberration from the figure cannot be corrected by interpoſing a diſperſive fluid between two convex lenſes of a greater refractive denſity than the interpoſed fluid. For all the refractions, being made the ſame way, tend to converge the external rays to points nearer the lens than its geometrical focus. Hence, when rare fluids are made uſe of to remove the aberration from the difference of refrangibility, ſome farther contrivance becomes neceſſary to correct the ſpherical aberration.

The moſt obvious way, and which on trial was found ſucceſsful, is to include the rare diſperſive fluid between two glaſſes, ground concave on one ſide and convex on the other, and thus form ſuch a concave as ſhall be required. An Achromatic Object-glaſs may be formed by combining this with a convex. The objection to this conſtruction is, that one of the advantages ariſing from the uſe of fluids is given up, namely, the prevention of that loſs of light, by reflection, which is a conſequence of the fluid being in immediate contact with the glaſs, whereas in the preſent caſe the ſpace between the convex and concave is occupied by air.

On this account, Dr. Blair attempted to introduce a third medium, by filling this vacancy with a fluid of the leaſt diſperſive kind, and of leſs mean refractive denſity than the diſperſive fluid. For this purpoſe he employed ſometimes rectified ſpirit of wine, and ſometimes vitriolic ether; and by giving to the lenſes the proper degree of curvature, in which great variety may be introduced, he ſucceeded in forming object glaſſes in which both aberrations are removed, and hardly any more light loſt than in a ſimple object-glaſs.

Having gained this point, he now determined to try how far the aperture of the object-glaſs might be increaſed, without increaſing its focal length, expecting, at leaſt, to equal reflectors in this reſpect. But the firſt trials to execute object-glaſſes on this principle, though they left no reaſon to complain of want of ſucceſs when compared with ſuch inſtruments as are now in uſe, exhibited new phenomena and new obſtacles to the perfection of the theory of Teleſcopes more unaccountable and perplexing than any he had before encountered. The hiſtory of theſe intereſting facts, and the regular progreſs of diſcovery by which they were remedied, conſtitute a large part of the paper, which well deſerves to be conſulted by thoſe who wiſh effectually to proſecute this ſubject. Brevity, however, demands a leſs hiſtorical narration in this place.

Theſe new difficulties aroſe from the effects of contrary powers of diſperſion, which though equal upon the extreme rays, were not found to be the ſame upon the rays that occupy intermediate ſpaces in the coloured ſpectrum. From what has been already ſtated on this ſubject, the attentive reader may underſtand, that ſuch intermediate rays from a compound lens of the kind here deſcribed, would not be aſſembled at the common focus. The particular nature of this diſperſion is much more accurately ſeen by applying a deep eye-glaſs to the focal image, than by experiments with priſms. When the image of a lucid point is formed in the focus of a ſimple lens, the violet or moſt refrangible rays are converged to a focus neareſt to the lens, and the deep red rays are converged to a focus at the greateſt diſtance from it. The conſequence of this is, that if the image be examined by an eye-glaſs nearer to the lens than is required for diſtinct viſion, it will be ſurrounded with a red fringe, which is the prevailing colour of the leaſt refrangible rays; and if the eye-glaſs be placed at a diſtance beyond that which is required for diſtinct viſion, it will be ſurrounded with a blue fringe, which is the prevailing colour of the moſt refrangible rays.

The reaſon of this will appear more clearly from inſpecting the ſixth figure, where the red rays appear outermoſt within the focus at A, and the violet rays appear outermoſt beyond the focus at B. Theſe colours are alſo viſible when an image of any luminous object, as the ſun, is formed by a lens on a white ground; and they will be ſo much the more conſpicuous, the greater the diameter of the lens in proportion to its focal diſtance. The reverſe of this happens in compound lenſes, when the medium employed to correct the colour diſperſes more than it ought to do.

In this way the correction of the colour may be examined, and the qualities of refracting mediums inveſtigated, to an extreme degree of accuracy. Yet the effect will be rendered ſtill more ſenſible by covering half the object-glaſs. For, when this is done, the colour produced by the uncovered half of the object-glaſs appears, without being mixed with that of the oppoſite ſide, even when the eye-glaſs is adjuſted to diſtinct viſion. Thus, in Fig. 6, the colours produced by both ſides of the lens are mixed at the general focus F. But if the rays coming from one ſide be intercepted, thoſe which are refracted by the other ſide will appear in their proper colours. By theſe means, and by employing a very luminous object ſurrounded by a dark ground, and a high magnifying power, the leaſt uncorrected colour may be rendered ſenſible.

The firſt conjecture which preſented itſelf on the obſervation of irregularities of this kind, in compound lenſes adjuſted ſo as to correct the aberration of the extreme rays, was, that the colour might ſomehow proceed from the compenſation at different diſtances from the axis of lenſes not correſponding as the plane ſurfaces of priſms everywhere do. Trials on the images formed by the central parts, and afterwards by the parts near the circumference, proved, however, that this conjecture did not reach the cauſe. Subſequent experiments and examinations of the coloured fringes led the author to the true cauſe, that is to ſay, the want of proportionality in the diſperſive powers of the different mediums, as has been already ſtated. It remained, therefore, to diſcover, by a new proceſs of experimental enquiry, the adequate remedy for ſuch an extreme ſource of imperfection. This led to the valuable doctrine, that one claſs of diſperſive mediums throw the green nearer to the violet, while another claſs throw the ſame colour nearer to the red, than is ſeen in the ſpectrum formed by crown-glaſs and other mediums, which diſperſe but in a ſmall degree. Theſe mediums were therefore applied in oppoſition to each other, to correct this ſecondary aberration in lenſes more compounded than might have been required if ſuch a difficulty had not preſented itſelf. And laſtly, the greater degree of compoſition in theſe perfect lenſes was got rid of by the happy expedient of mixing the two different kinds of diſperſive mediums, which, as it fortunately turned out, did produce a compoſition of an intermediate nature, with regard to this proportionality in the arrangement of the priſmatic colours.

It has already been remarked, in the order of this abridgement, that, in metallic ſolutions and eſſential oils, the green light is among the leſs refrangible rays; but that, in the marine and nitrous acids, the ſame green light becomes one of the more refrangible. It was not probable that the eſſential oils could be united with marine acid ſo as to form a colourleſs fluid fit for optical uſes; but nothing could be better adapted for this purpoſe than metallic ſolutions.

The firſt trial was made with butter of antimony. On increaſing the proportion of marine acid, the fringes of green and purple, which, being the intermediate rays, were irregularly refracted by the metallic ſolution, grew narrower and narrower, till they entirely diſappeared; and if more was then added, they re-appeared in an inverted order. The ſame thing was tried with a ſolution of crude ſal ammoniac and corroſive ſublimate. With a certain proportion of theſe two ſubſtances, the rays of all colours emerge from the compound object-glaſs equally refracted. If the proportion of the ammoniacal ſalt, and conſequently of the marine acid it contains, be increaſed, the green rays which were the mean refrangible, in the diſperſive fluid as well as in crown-glaſs, draw nearer to the violet, making a part of the more refrangible half of the ſpectrum, and conſequently emerge leſs refracted than the united red and violet rays, and are converged to a focus at a greater diſtance from the object-glaſs; ſo that the green fringe now appears within the focus, and the purple fringe beyond it. But, on increaſing the proportion of mercurial particles, theſe ſame green rays ſhift their ſituation to the leſs refrangible half of the ſpectrum, which appears from their now emerging moſt refracted, and being converged to a point nearer to the object-glaſs than the united red and violet, whoſe refrangibility does not appear to be affected by theſe admixtures which occaſion ſuch remarkable fluctuations in the refrangibility of the green rays and other intermediate orders. It may poſſibly ſeem ſtrange at firſt view, that the green rays ſhould emerge moſt refracted from the compound object-glaſs, when their refrangibility in the diſperſive medium is diminiſhed, and leaſt refracted under the contrary circumſtances. The cauſe of this is, that the principal refraction of the compound object-glaſs is performed by the indiſperſive convex lens, which is oppoſite to the refraction produced by the diſperſive concave.

Fig. 7. repreſents an object-glaſs of this kind in Dr. Blair's poſſeſſion, in which the metallic particles are ſo far diminiſhed, and the particles of marine acid ſo far increaſed, as to render the refraction of the ſeveral orders of rays proportional in both mediums. There are two refractions in the confine of glaſs and the fluid, but not the leaſt colour whatever. Hence it is inferred that, notwithſtanding the conſiderable difference of denſity, and the refraction which, on account of the anterior ſurface being plain, and the poſterior ſurface ſpherical, having the focus of parallel rays for its centre, is performed only at the two ſurfaces of the fluid, there is no unequal refrangibility of light. The rays of different colours are, as the Doctor obſerves, bent from their rectilineal courſe with the ſame equality and regularity as in refraction.

As cuſtom has already appropriated the word Achromatic to that kind of refraction in which there is only a partial correction of colour, the author propoſes to diſtinguiſh this entire removal of aberration by the term Aplanatic.

Thus far I have endeavoured, partly by abridgement, partly by extracts, and partly by taking greater liberties of narration and arrangement, to convey the ſubſtance of Dr. Blair's important diſcoveries to the public. It is now ſome years ſince the attention of the world was firſt directed to his improvements. I have therefore been induced to make enquiry, among the artiſts of this metropolis, into the ſtate of the undertaking conſidered as a manufactory. Theſe enquiries led me to the Doctor himſelf; from whom I underſtand, that all the practical difficulties are removed, and that the delay which has prevented the philoſophical world from being yet ſupplied with theſe inſtruments has ariſen merely from the interruption of engagements. And as I have reaſon to expect ſome communications from him on this ſubject in future, it becomes unneceſſary to enter into any of thoſe remarks, in this place, which the immediate conſideration of the ſubject might ſuggeſt.

II.

A Remarkable Effect of the Inflection of Light paſſing through Wire Cloth, not yet clearly explained.

IN the ſecond volume of the Tranſactions of the American Philoſophical Society, p. 201, an optical problem is propoſed in a letter from Mr. Hopkinſon to Mr. David Rittenhouſe, who has given a ſolution in his anſwer. Mr. Hopkinſon, upon looking through the threads of a ſilk handkerchief, held cloſe before his face, at a diſtant lamp, was ſurpriſed to obſerve what he thought to be the threads magnified; and ſtill more, that theſe ſuppoſed threads remained ſtationary, notwithſtanding any motion he gave the handkerchief to the right or left.

Mr. Rittenhouſe, in his remarks on this phenomenon, obſerves very juſtly, that the appearance, which is that of certain luminous points regularly arranged in right lines croſſing each other, cannot conſiſt of any image of the threads of the handkerchief, but muſt be certain images of the lamp formed by the inflection of light paſſing near the threads. He appears to have conſidered the handkerchief as a very imperfect inſtrument, and therefore too haſtily threw it aſide for an inſtrument conſiſting of parallel hairs not croſſed by others. With this he obſerved that the line of light or portion of the ſky ſeen through a window-ſhutter very nearly cloſed, appeared multiplied, ſo as to conſiſt of three parallel lines almoſt equal in brightneſs, and on each ſide four or five others, much fainter, and growing more faint, coloured and indiſtinct the farther they were from the middle line. When the hairs were thicker, but at the ſame diſtance apart, or number in the inch, ſo as to diminiſh the opening between them, the middle lines were leſs bright, but the others ſtronger and more diſtinct; and he could count ſix on each ſide of the middle line ſeeming to be equally diſtant from each other, eſtimating the diſtance from the centre of the one to the centre of the next. In this experiment, the hairs were the 190th of an inch in diameter, and were regulated by paſſing them over the threads of a ſcrew of 106 turns in the inch: ſo that the opening or interval between hair and hair was $\frac{1}{250}$th of an inch. The middle line was well defined and colourleſs; the next two were likewiſe pretty well defined; but ſomething broader, having their inner edges tinged with blue and their outer with red. The others were more indiſtinct, and conſiſted each of the priſmatic colours in the ſame order; which, by ſpreading more and more, ſeemed to touch each other at the fifth or ſixth line; but thoſe neareſt the middle were ſeparated from each other by very dark lines much broader than the bright lines.

From experiments made by applying this frame of parallel hairs before the object-glaſs of a ſmall teleſcope furniſhed with a micrometer, he found that the poſition of the two neareſt ſide-lines indicated an angle of deflection equal to 7′ 45″; and, from eſtimate founded on the appearance of the lines, he judged that the other ſtreaks were deflected in angles of double and triple the ſame original angle.

After this ingenious philoſopher had proceeded ſo far, and found a ready ſolution for the facts he had obſerved, by referring them to the Newtonian doctrine of inflection, he went on, with more haſte than preciſion, to account for the appearance exhibited by the handkerchief. He remarks, that, if the handkerchief be held before the eye, ſo that its threads may be diſpoſed horizontally and perpendicularly, and a luminous ſpot be viewed, the perpendicular threads will, by inflection, on the principle explained in his frame of hairs, cauſe it to appear as a row or horizontal line of ſpots, for example, five; and that the horizontal threads will, on the ſame principle, convert every one of theſe into five perpendicular rows; by which means, a ſquare figure, compoſed of twenty-five luminous points, will be ſeen, of which he gives the figure.

On theſe laſt inductions it may be ſufficient to remark, that though his frame of parallel hairs might have converted a real row of luminous points into ſuch a ſquare, yet it is inconceivable how the ſame effect could have followed, with regard to rays of light which muſt have already paſſed through the handkerchief in order to receive the firſt modification. But as Mr. Rittenhouſe, from his figure of 25 ſpots, which are in no caſe produced by the handkerchief, ſhews that he did not ſtrictly attend to the phenomena, but wrote in this inſtance from recollection and reaſoning, I ſhall diſmiſs theſe particulars of the original obſervers, and mention the facts I have myſelf remarked.

When this problem was firſt pointed out to me a few months ago by a very active promoter of the ſciences, I took the earlieſt opportunity of looking at a ſtreet lamp through a piece of muſlin containing one hundred threads in the inch. Inſtead of one ſpot of light, there appeared nine; four of which occupied the corners of a ſquare, one the centre, and four others the middle points, in a right line between the corners. Upon examining them with an achromatic perſpective magnifying fourteen times, I ſaw that the ſpots were true images of the flame of the lamp, the central one being perfect, all the others coloured, with the red outermoſt, and the corner lights leaſt luminous and diſtinct. It made no difference in the appearance, whether the cloth was applied cloſe to the object-glaſs, or at any diſtance to which the arm could reach before it; or moved ſideways in its own plane; but the apparent dimenſions of the ſquare were leſs when the cloth was applied between the object-lens and its focal image, in proportion (by eſtimate) as the diſtance of the cloth from the focus was leſs. When coarſer muſlins were uſed, the ſquare was ſmaller according to the coarſeneſs, but in what proportion was not examined, becauſe it was ſuppoſed to follow no very ſimple ratio. No effect of this kind took place with the object-glaſs of a microſcope.

From theſe facts I inferred as follows:—The middle flame is formed of all the pencils of light which paſs through the central parts of the holes in the cloth without any deflection. The four flames in the middle of the ſides of the ſquare, are formed by the pencils of light inflected towards the ſhort threads bounding each ſquare hole in the cloth, aſſiſted by the deflective power of the oppoſite threads reſpectively. The corner flames are formed by the combined action of the two threads contiguous to the angles of every hole; the ſum of which force, acting in the diagonal with a power varying from the diſtance of the reſpective

parts

parts of the lines about the angle, cannot produce an image ſo clear and definite as the ſides. The images are farther aſunder the finer the cloth, becauſe the nearneſs of the threads increaſes the inflection, inſomuch that when the interval is about $\frac{1}{400}$th of an inch there will be no central image *. As the images are produced by an equal number of claſſes of pencils parallel to each other, any motion of the cloth either parallel or perpendicular to its own plane before the object glaſs will not affect them, becauſe they come to the object-lens under the ſame circumſtances of paralleliſm. But when the deflections were made by the cloth between the object-lens and its focus, the poſitions or diſtance of the images were affected more or leſs upon the principle of the priſmatic micrometer. And, laſtly, the microſcope does not ſhew this effect, becauſe the rays from the object, not being parallel, are not uniformly deflected, and have no virtual foci.

I was well ſatisfied at firſt with this theory, and ſtill think the greateſt part will, upon further examination, prove to be well founded. But upon repeating the experiment with a piece of new cloth made of braſs wire about fifty-five threads in the inch, the wire itſelf being by eſtimate about the $\frac{1}{160}$th of an inch thick, other lights became viſible, the cauſe of which does not appear ſo obvious. Beſides the ſquare, conſiſting of nine lights, there were others much fainter, more coloured, and elongated, as in Fig. 11. In order to diſcern theſe to the greateſt advantage, the ſmall focal image of the ſun was viewed in a concave metallic reflector of two inches focus, by means of the achromatic perſpective before mentioned, at the diſtance of nine feet, with a power of only eight †. All the images, except that in the centre, were coloured ſpectra, extremely brilliant and beautiful; the original nine, however, ſtill retaining their pre-eminence. They were fainter and more elongated, the more remote from the centre to which they all pointed; that is to ſay, an imaginary line drawn through the middle of all the colours of any one of the ſpectra, would have interſected the central colourleſs ſun. Nevertheleſs their arrangement with regard to each other was not radial, but in lines forming prolongations of the ſides of the middle ſquare, as in the figure of the cloth: theſe lines were parallel to the threads. The interval between each range of images, meaſured from the centre, was greater the farther off, but not regularly increaſing; thoſe near the middle ſquare being almoſt equal. Seven ranges were counted beyond the ſquare on each ſide, and a very long triple line of whitiſh faint light extended ſtill farther. There were alſo a great number of much fainter images irregularly ſcattered in the angular ſpaces between the croſs, all pointing to the centre, and produced, as I conjectured, by irregularities in the cloth.

As the diſtance of the cloth before the object-glaſs did not affect the pencils of light, it readily occurred, that by inclining its plane to the axis of viſion in ſuch a manner as that one ſet of the wires might ſtill remain perpendicular to that axis, the other ſet would apparently crowd together, and diminiſh the openings through which the rays were to paſs. In this manner the cloth might be made, in effect, of any required fineneſs. When the croſs wire or weft of the cloth was inclined to the axis, that part of the croſs in Fig. 11 which correſponded with the direction of the inclined wires, had the intervals of its ſpectra enlarged, at the ſame time that they became broader and more coloured, but fainter, and in particular the central colourleſs image nearly vaniſhed. I have no doubt

---

* Newton's Optics, III. obſ. 6.

† This was for the ſake of light, as the chamber was not darkened. The object-glaſs of 9 inches focus and 1 inch aperture bears a magnifying power of fifty, without producing any colour.

but it would have diſappeared totally, if the want of perfect flatneſs in the cloth had not prevented its being inclined beyond a certain angle with regularity. The branches of the croſs at right angles to the inclined wires alſo nearly vaniſhed.

But when the warp or longitudinal wires were inclined to the axis of viſion, the images were altered in their relative brightneſs and poſition, in a manner which it would be too prolix to relate. Theſe changes terminated in an hexagon with ſix radii, as in Fig. 12. when the inclination was about 45 degrees. In this, as in all the changes, the ſpectra were coloured with the red fartheſt from the centre, and they all pointed to the centre. This unexpected appearance led to an examination of the cloth by a ſimple magnifier in both poſitions of inclination, when it was found, that in the firſt deſcribed poſition the interſtices of the wires appeared to be nearly long ſquares, or parallelograms; but in this laſt deſcribed poſition the threads of the warp being in reality bended by the operation of weaving over the weft, in angles of about 35° out of the plane, did apparently meet, ſo as to leave apertures of the form of equiangular triangles, and no doubt cauſed the hexagonal combination in Fig. 12.

After this imperfect deſcription of a phenomenon of ſome curioſity, I muſt decline entering into any diſcuſſion of the conſequences to which it ſeems to lead. Sir Iſaac Newton's Experiments on Inflection, with the edges of two knives, are ſimple, and at firſt conſideration do not appear difficult to be underſtood. Yet, ſimple as they are, there are certainly many variations yet to be made, and admeaſurements to be taken, before the popular explanations of the mutual agency of bodies and light upon each other can be admitted without heſitation. This proceſs of examination affords ſome peculiar facilities, on account of the great quantity of light which is ſimilarly affected, and the poſſibility of applying a magnifying power and micrometer. Subſequent meditation may perhaps ſuggeſt the cauſe why the radii in theſe figures are triple, and parallel to each other, while all the ſpectra individually diverge from a centre: but I have rather choſen to refer them as they are to the examination of philoſophers. And I am the more induced to hope that this examination will be made, when it is conſidered, that the appearance preſents itſelf through any fine muſlin or cambric to the naked eye, though ſtill better with the opera-glaſs or perſpectives, which are in the hands of every one, and that the wire-cloth of fine ſieves or wine-ſtrainers is a material eaſy to be procured.

III.

Deſcription of an Inſtrument which renders the Electricity of the Atmoſphere and other weak Charges very perceptible, without the Poſſibility of an equivocal Reſult.

IN the year 1787, when the electrical doubler engaged the attention of philoſophers, for its aſtoniſhing power of magnifying the minuteſt quantity of ſimple electricity, and the ſubſequent diſcovery of its ſpontaneous electricity had greatly reduced its apparent utility, I had the pleaſure of a converſation with the Reverend Abraham Bennet, of Wirkſworth, the inventor, who ſhewed me his method of depriving this inſtrument of much of its adherent electricity, by working it for a time with all its parts in communication with the earth. But at the ſame time he remarked, that if he were to make an inſtrument in which this

electricity

electricity ſhould be totally removed, he would have recourſe to a ſimple multiplier, and not a doubler. I did not apprehend the contrivance from this ſlight deſcription, which at my requeſt, however, he readily extended, and convinced me of the full value of his invention. I then mentioned my intention to conſtruct an inſtrument on that principle, which ſoon afterwards I did. It was ſhewn to Sir Joſeph Banks and his friends, at his houſe, nearly at the ſame time, and in the ſame year transferred to the celebrated Mr. van Marum of Harlem, who now poſſeſſes it. From various other avocations, I was prevented from cauſing any others to be made. It is not therefore wonderful that the ſame thought ſhould ſince have occurred to ſo great a maſter of the ſubject as M. Cavallo, who in the third volume of his Electricity, publiſhed in 1795, gives a deſcription and engravings of an inſtrument very different in form, but the ſame in principle. The form I have given it, which is all the ſhare I had in the contrivance, is more calculated for a ſpeedy repetition of the proceſs than M. Cavallo's: but his inſtrument is much eaſier to be made by one who is not profeſſionally a workman. Mr. Bennet's notion with regard to form was very different from either. Both inſtruments have the property, that a ſimple or ſmall aggregate of electricity will not be multiplied unleſs its intenſity exceed that of the adherent electricity of the plates, ſuppoſed to be contrary; and both are capable of deſtroying their own electricity, and exhibiting unequivocally what they receive by communication, however weak it may naturally be, provided the ſupply be kept up. They are both alſo limited as to the extent of their multiplication; that is to ſay, the effect will be denoted by a fixed number or multiplier, ſo long as the neareſt diſtance of the plates continues unaltered. For example, if this number were 100, the inſtrument would ſhew no electricity which was leſs intenſe than the $\frac{1}{100}$th part of what the electrometer demanded to act upon it. This coefficient is nevertheleſs capable of being enlarged at pleaſure, by adjuſting the plates to a leſs diſtance aſunder.

Fig. 8. repreſents a vertical ſection of the inſtruments. A is a metallic vaſe, having a long ſteel axis which paſſes through an hole in the ſtand H at K, and reſts on its pointed end in an adjuſtable ſocket at C. The uſe of the vaſe is, by its weight, to preſerve, for a conſiderable time, the motion of ſpinning which is given by the finger and thumb applied to the nob at the top of the inſtrument. The ſhaded parts D and E repreſent two circular plates of glaſs nearly 1½ inch in diameter. The upper plate is fixed to the vaſe, and revolves with it; the lower is fixed to the ſtand. In the lower plate are inſerted two metallic hooks, diametrically oppoſite each other, at F and G. They are cemented into holes drilled in the edge of the glaſs, which is near two-tenths of an inch thick. In the upper plate are inſerted in the ſame manner two ſmall tails of the fine flatted wire uſed in making ſilver lace. Theſe tails are bended down ſo as to ſtrike the hooks in the revolution, but in all other poſitions they remain freely in the air without touching any part of the apparatus. At C is a ſcrew, which by raiſing or lowering the vaſe keeps the faces of the glaſs plates from each other at whatever diſtance may be required. The faces of the glaſs plates which are oppoſed to each other are coated with ſegments of tin foil, as repreſented Fig. 9 and 10, the latter of which repreſents the upper plate. Each of the tails communicates with the tin foil coating to which it is contiguous, as does alſo the hook F with that coating of the lower plate neareſt to it. But the hook G is entirely inſulated from the whole apparatus, and is intended to communicate only with the electrified body or atmoſpherical conductor L.

The lower coating neareſt to G is made to communicate permanently with the ſtand H, and conſequently with the earth.

In this ſituation, ſuppoſe the motion of ſpinning to be given to the apparatus, and the effects will be theſe:—One of the tails will ſtrike the hook G, by which means the upper coating annexed to that tail will aſſume the electric ſtate of L by communication. But this ſtate, on account of the proximity of the lower uninſulated plate to which it is, at that inſtant, directly oppoſed, will be as much ſtronger than that of L, as a charge exceeds ſimple electrization. The tail G with its plate or coating proceeds onward, and after half a revolution arrives at the ſituation to touch the hook F. The upper coating, the lower on the ſide of F, the hook F itſelf, and the tail V, muſt then conſtitute one jointly inſulated metallic maſs, in which no charge ſubſiſts, but which is ſimply electrified by the whole charge received at G. And of this maſs the ſurfaces of the plates themſelves, conſtituting the electric well of Franklin, will throw out all their electricity to the hook and tail. But the coating and its tail inſtantly paſs round, leaving F electrified, and proceed to bring another charge from G and depoſit it as before. The balls at F are therefore very ſpeedily made to diverge. It is ſcarcely neceſſary to remark, that the two upper coatings do nothing more than double the ſpeed of the operation; one of the tails being employed in collecting, while the other is depoſiting: and that the gold leaf electrometer may be advantageouſly ſubſtituted for the cork-balls.

The inſtrument I cauſed to be made was five inches high. The receiving ſide G was connected with a coated jar of four ſquare feet coating, and the giving ſide F was connected with Bennet's gold leaf electrometer. The electrometer was rendered as ſtrongly poſitive as it was capable of being, and the jar was rendered negative, by giving it as much of that power as was produced by drawing a common ſtick of ſealing-wax once through the hand. In this ſtate the jar was incapable of attracting the fineſt thread. The vaſe was then made to ſpin; and the effect was, that the leaves of the electrometer firſt gradually collapſed, and then in the ſame manner gradually opened, and ſtruck the ſides of the glaſs of the electrometer with negative electricity. The experiment was renewed and repeated with every requiſite variation.

IV.

Obſervations on the Art of Printing Books and Piece Goods by the Action of Cylinders.

—— Experto credite.

WE may conceive three ways of delineating figures, or writing. The firſt and moſt ancient conſiſts in making the traces ſucceſſively by a bruſh, a pen, or other inſtrument. This is deſign, painting, or writing. In the latter methods, either the whole or the greater part of the figures are made by the action or preſſure of an original pattern againſt the material intended to be written or painted upon. It is the art of printing. The colouring matter is either depoſited from the face of prominent parts of the original form, which is uſually called a block or type; or elſe it is preſſed from cavities cut in the face of the original, which in this caſe is called an engraved plate. Moſt books are printed from original patterns, in relief; and moſt of the imitations of paintings are performed by means of engravings. Theſe arts are moſt frequently diſtinguiſhed by the names of letter-preſs and copper-plate printing.

It can ſcarcely be matter of new information to thoſe who are but moderately acquainted with the ſtate of the Arts, to be told that letter-preſs or book-printing is performed by an aſſemblage of ſingle metallic letters, called types, made of lead hardened by an addition of antimony in the metallic ſtate; that theſe letters are compoſed in the form of book pages, and wedged together in iron frames called chaſes; that the ink is a compoſition of linſeed oil and lamp black, of ſo ſingular a nature, that it will adhere to a ball covered with a pelt or ſheep's ſkin ſoaked in water, and kneaded to extreme ſoftneſs under the feet, but quits this ſkin with great readineſs to apply to the face of the letter when dabbed with the ball; and ſtill more, that it almoſt totally quits the letter to adhere to paper rendered ſemitranſparent by ſoaking in water; or laſtly, that the paper is applied and preſſed againſt the form of compoſed letter by means of a flat piece of wood urged downwards by a ſcrew. Theſe and numerous early diſcovered principles of this moſt uſeful art are generally known, and require no more than mere recapitulation in this place.

The genius of the Chineſe language not permitting that people to analyſe its ſounds into an alphabet, as has been done by moſt other nations, has induced them to retain thoſe ſigns of things, and of their correſpondent words, which probably conſtituted the firſt picture or hieroglyphic writings of every rude ſociety. Changed and complicated as theſe may have become by the rapidity of tranſcription, the corruption of ignorance, or whatever other cauſes may have operated through a long ſucceſſion of ages, they ſtill for the moſt part uſe words that properly denote things, and not ſounds. Such words cannot, therefore, be ſubdivided; and it has accordingly been found moſt convenient, by theſe firſt poſſeſſors of the art, to print from entire blocks, as was alſo done by the firſt printers in Europe. But our artiſts ſoon diſcovered that a few of the ſimpleſt characters, namely, the letters of the alphabet, would be in many reſpects more uſeful, as the elements for compoſing blocks for printers, than a number of blocks originally cut for every page of every individual book.

Book-printing, therefore, though in fact of the ſame nature as block-printing, has been carried into effect by very different machinery from that made uſe of in the arts which ſtill retain the latter method. In book-printing, the heavy metallic form lies on a kind of table, and the colour and the paper are ſucceſſively applied to its face: but in block-printing, the block is carried and applied to the colour, and afterwards to the work intended to be printed. Thus, for example, in the printing of paper-hangings, the colour is ſpread with a bruſh upon a woollen cloth ſtretched over a ſurface of parchment or ſkin evenly ſupported by a half fluid maſs of water and maſhed paper. To this the block is carefully applied by a ſlight perpendicular ſtroke or two; after which it is applied to the dry paper on a table, and preſſed againſt it either by one or more blows with a mallet, or by the regular action of a lever. The mechanical part of callico-printing is effected nearly in the ſame manner; but with ſmaller blocks, becauſe of the greater difficulty of making the ſucceſſive fittings on ſo flexible a material. And in both theſe arts, as well as in book-printing, in red and black, the variety of colours are produced by repeated applications of forms or blocks, of which the prominent parts are made to fit each other according to the nature of the deſign.

In the art of printing from copper-plates, a colour ſomewhat more fluid than for book-printing is made uſe of. It is preſſed into the cavities of the plate by ſmearing it over the ſurface; and by ſubſequent careful wiping the redundant colour is cleared away. In this ſtate, if ſoaked paper, for which purpoſe the moſt ſpongy texture is the beſt, be ſtrongly preſſed againſt the plate, by paſſing both together between two cylinders of metal or hard wood, properly defended by woollen cloth, the greateſt part of the colour adheres to the paper, and forms what is called a print.

In all theſe proceſſes, it is eaſily ſeen, that in the ſucceſſive applications of colour, the accurate filling of the form or original with the material intended to receive the impreſſion, and in various other parts of the manipulation, there is much room for the diſplay of ſkill, or for injury from the want of it. It may moreover be collected, that the motions attendant on the various ſteps of manufacture are in many inſtances difficult to be performed with rapidity and eaſe, until by long continued habit the workman himſelf is converted as it were into a machine. A very ſlight degree of attention to this ſubject muſt alſo ſhew that, if the originals were of a cylindric form, with a contrivance for regularly applying the colour and performing the ſubſequent operations, it would be eaſy to print books and piece-goods with a degree of rapidity and uniformity, of which the uſual method of ſucceſſive applications ſeems ſcarcely capable without uncommon care and ſkill. This obvious concluſion has no doubt led to numerous experiments; none of which, as far as I can gather, whatever may have been their particular utility, have given much promiſe to ſuperſede the ordinary methods. But as the increaſed demand for the manufacture of printed goods has rendered ſuch an improvement an intereſting object to manufacturers, as well as to thoſe indefatigable artiſts who have directed their efforts towards improvements; and as the latter generally take up a new object under a ſtrong perſuaſion that it has not before been purſued by others, it will certainly be of advantage to theſe deſerving claſſes of men, to relate a few of the difficulties of this new art.

The difficulties attendant on any improvement in the arts may be conſidered either as moral or phyſical. Under the moral, I would claſs every thing that relates to the prejudices of men in favour of the old methods, and their fears of riſk, together with the œconomical and commercial inconveniencies attending the new proceſſes. The phyſical difficulties are ſuch as attend the actual performance of any project after the ſame has been carefully arranged in the mind of the inventor. It happens unfortunately here alſo that the inventor is ſeldom aware of the moral impediments, but almoſt always concludes, that if he can ſucceed in accompliſhing the purpoſe he has in view, his cares and labour will then be at an end; and that the manufacturer, in particular, inſtead of pointing out new impediments diſcernible only from long continued experience, will more readily embrace and approve of the new proceſſes, in conſequence of his ſuperior knowledge of their intrinſic value.

Every good invention appears ſimple in the proſpect, but it ſcarcely ever happens in the execution that the moſt direct road is taken; and in every caſe there will infallibly be many things unknown or unforeſeen, which practice only can point out as neceſſary to be done for the complete accompliſhment of the object in view. Hence, and likewiſe becauſe few men poſſeſſed of independent fortune are likely to engage or perſevere in a labour of this kind, it almoſt invariably happens that the expences exceed the ability of the inventor himſelf. For theſe and other reaſons, new undertakings are generally brought forward by the inventor, a man ſtrongly prejudiced in favour of his leading purſuit, together with a moneyed friend, who hopes ſpeedily to increaſe his capital from the abilities of the other. It is not neceſſary in this place to deſcribe the uſual conſequences of a partnerſhip, where the minds, the views, and the circumſtances of both individuals are ſo very different, and which may be modified ſtill more eſſentially if either of the parties be deficient in the common principles required to bind men to each other. It is certainly of the higheſt importance to both, that the circumſtances of ſuch connections ſhould be very maturely weighed before they are entered into.

The commercial difficulties or facilities attending any invention are alſo of great conſequence. Every inventor ought to enquire, not only what has been done before, but likewiſe into the preſent ſtate of the manufacture he means to improve. In this way it is aſcertained how ſmall a part the mere preſs-work conſtitutes in the price of a book. He will find that twelve yards of paper-hangings are printed for one penny, in a ſingle colour, by hand, which afterwards by the accumulation of price, in paper, colour, duty, and ordinary profit, are ſold for three ſhillings; none of which the inventor can pretend to diminiſh: and if he could annihilate the whole labour, his advantage would therefore be leſs than three per cent. without reckoning the coſt and operation of his machinery. In the callico-printing, with a more expenſive material, dyeing and field proceſſes, duty and profits of manufacturer and vender, the price of laying the block will turn out to be an object ſtill leſs conſiderable. Again: it will be ſeen that ſmall flat blocks coſt but little money, in compariſon with cylinders of ſufficient diameter to retain their figure, and long enough to apply to the whole breadth of the cloth.

Under theſe and other ſimilar points of view, the inventor, who may conſider the ſubject in a ſuperficial manner, would be ready to abandon his undertaking. But this again ought not to be raſhly done. It is true, that where the great force of capital is employed on objects not comprehended within his project, the ſaving, however large in its abſolute amount, or deſirable to a manufacturer, will ſcarcely come within the reach of the inventor by any bargain he can make ſhort of an actual partnerſhip. But it may be poſſible to ſeparate the reſpective departments of a manufactory. A ſpinner is not neceſſarily a weaver, nor a printer a linen-draper or a dealer in paper-hangings. The ſeveral departments of manufacture and commerce are, generally ſpeaking, in the hands of acute men, who ſeldom reaſon ill with regard to the advancement of their peculiar intereſts; and theſe departments are continually fluctuating in their arrangement, as convenience, profit, or the accumulation of capital may lead. Experiments are for ever on foot, from day-work to piece-work, and from piece-work to the employ of maſter-workmen with others under them, all ſupported by the capital of the large manufacturer, who himſelf in many inſtances is the mere inſtrument maintained by the advances or acceptances of the warehouſeman, the factor, or the merchant. An inventor who has not capital may ſeek for employ on the goods or the capital of others; and if he has ſkill to maintain his ground againſt the numerous enterpriſes which the activity of oppoſite intereſts will raiſe againſt him, he will find that the old order of things will readily alter, as ſoon as an evident intereſt in favour of the new is ſhewn by actual and continued proofs in the market.

Moſt of the phyſical difficulties attendant on any new proceſs are ſuch as experience only can ſhew. Thus, in the forging of iron by the preſſure of rollers inſtead of hammers,

mers, a ſcheme upon which many thouſands of pounds have been expended in this country, it was apprehended that the more impure parts, which are alſo the moſt fluid, might be preſſed out by the action of cylinders, with equal or perhaps more advantage than by that of hammers; at the ſame time that the determinate figure of bars of any required ſize might be given without ſkill in the operator. Experience nevertheleſs has ſhewn, that the more fluid part is driven out much more effectually by the ſudden action of a blow, than by the ſlower compreſſion of a cylinder, which allows time for much of the fluid matter to extend itſelf within the maſs. Various ſimilar effects preſent themſelves when cylinders for printing are ſubſtituted inſtead of planes. Inſtead of the action of dabbing, the colour is uſually applied by ſimple and gradual contact, to much leſs effect; and the impreſſion, though not eſſentially different from that of the block, is performed by a gradual action, which affords time for the cloth or paper to fold itſelf in a minute degree into the cavities of the ſculpture. Hence it is found that the length of paper or cloth printed from a cylinder by a definite number of revolutions, will be greater or leſs than another piece manufactured preciſely in the ſame way, but with a leſs or greater degree of preſſure. In a block this defect is much leſs, not only from the conſiderable hold it takes upon the ſurface of the material, but alſo becauſe the error is rectified at every ſucceſſive application. One of the chief difficulties of cylinder printing conſiſts, therefore, in the difficulty of laying one colour after another; and this would continue to be ſo even if the materials were not ſuſceptible of change, the contrary to which is the fact. There are two projects for obviating this. The one conſiſts in confining the whole piece to a long table, or to the circumference of a large cylinder, and cauſing the printing cylinder to move, not by the ſucceſſive appoſition of its carved ſurface, but of a bearing face regulated by a toothed wheel. The other method conſiſts in the uſe of a frame to confine two or more cylinders, each provided with its own toothed wheel, and revolving againſt a large clothed cylinder provided with a ſuitable wheel to drive the others. The piece is cauſed to paſs between the large cylinder and the others, in order to receive the impreſſion With regard to the firſt of theſe methods, it does not appear eaſy to confine paper, and ſtill leſs cloth, in ſuch a manner that its parts may continue without ſhift or wrinkle during the action of a cylinder, which not being allowed to roll without the check of a wheel, muſt draw the ſurface either the one way or the other. The difficulty of confinement will be very much increaſed, by the indiſpenſable requiſite that the paper ſhould be afterwards hung up to dry, and the callico be carried to the dye-houſe and the bleach-field, between the ſucceſſive impreſſions, by which means the dimenſions of both will be greatly altered. In the ſecond method, it is obſervable that no colours can be printed but ſuch as fall clear of each other. In this way, moreover, the gathering action of the cylinders may prove very miſchievous. For, if we ſuppoſe the paper or cloth to paſs between the great cylinder and the firſt printing roller by an action of the latter which tends to make it ſlip forward on the face of the great cylinder, and that when it arrives at the ſecond printing roller it there experiences an action of a contrary nature, the conſequence will be, that the material will become ſlack between the two rollers, and the fittings will be falſe. Not to dwell on that experience which brings forward this obſtacle among others, its great probability may be deduced from the allowable ſuppoſition that the circumference of the firſt printing cylinder ſhould be one thouſandth part of an inch too large, and that of the ſecond the ſame quantity too ſmall. For in this caſe the material will be ſhifted one-twentieth of an inch in fifty turns by the firſt cylinder, and the ſame quantity in the contrary direction by the ſecond; a quantity upon the whole quite ſufficient to deſtroy the effect of the colours in the progreſs of one ſingle piece. Such minute differences can hardly be avoided in the firſt inſtance; in addition to which we may place the varying dimenſions of the printing cylinder, if not made of metal; and of the great clothed cylinder, which in effect has a larger or ſmaller diameter in proportion to the preſſure which operates to render its elaſtic covering either thicker or thinner. The only method of diminiſhing theſe evils ſeems to be, that all the printing cylinders ſhould by dimenſion or preſſure, or both, be made to draw the ſame way, the outer cylinder moſt, and the others gradually leſs and leſs, ſo that the material ſhould have a tendency to apply itſelf more tightly during its paſſage through the apparatus.

The application of the colour to the ſurface of a cylinder block is attended with ſome difficulty. An ingenious mechanic may contrive various means to produce the action of dabbing, if required. When a ſtuffed cylinder covered with cloth is made to revolve in the colour, and thence after paſſing a ſcraper to apply itſelf to the block cylinder, it is found to be no inconſiderable difficulty that its dimenſions change, and its covering becomes wrinkled by the action of the ſcraper as well as that of the block. A better method, therefore, conſiſts in a revolving web of woollen cloth, like a jack-towel, ſtretched over three horizontal cylinders parallel to each other, two of which ſupport the elaſtic ſurface of the web, which in its revolution accompanies the block cylinder, and the other ſerves to guide the ſame web to the colour, or a cylinder revolving in it. This method would be very eaſy and pleaſant in its operation, if it were not for a property common to all ſtraps which revolve on the ſurface of two or more wheels. Theſe are obſerved always to ſeek the higheſt place; ſo that if a cutler's wheel were made with a groove to carry a ſtrap, inſtead of a round edge, the ſtrap would infallibly mount the ledge, inſtead of remaining in the groove*. On this principle, the web would very ſpeedily ſhift itſelf to one end of the cylinders, if it were not confined ſideways, or the lower roller were not made conſiderably thickeſt in the middle, and gradually tapering towards its extremities. This laſt ſimple expedient is not without its difficulties; but as I have not actually tried it, I ſhall defer entering into any diſcuſſion on that head.

The running of the paper or piece-goods towards one end of the leading cylinder is alſo one of the greateſt difficulties attending this method of printing. It is not perfectly removed by tapering the leading cylinders.

The nature of the trade of paper-ſtaining in this country, which requires a large ſum to be immediately veſted in the payment of the exciſe duty, and conſequently prevents any conſiderable ſtock from being manufactured until orders are actually received, and the varying faſhions in printed callicoes, which render the expence of cutting the block by far the heavieſt part of the diſburſement for printing, are probably the chief reaſons why manufacturers in this country have been leſs ſolicitous for the conſtruction of machines calculated to afford profit only in the caſe of very numerous impreſſions. The phyſical difficulties of this art have likewiſe conſpired, in no ſmall degree, to prevent its having been applied in the large way to any but a few ſimple deſigns of the ſort called running patterns in one colour.

---

* This curious effect ariſes from a flexure produced in the edge of the ſtrap, by the elevated part of the wheel, which throws the advancing part of the ſtrap more and more towards that elevation. It cannot be explained in a few words, but may eaſily be ſeen by wrapping a ſtraight ſlip of paper round an extinguiſher or any other cone.

An

V.

An Account of the Diamonds of Brazil. By M. D'ANDRADA.

AS the Society * is deſirous of an account of the Diamonds of Brazil, I ſhall endeavour to ſatisfy them to the utmoſt of my power; but previous to a deſcription of their form, the place where they are found, and the manner of ſearching for them, I think it will be uſeful to convey an idea of the country in which theſe diamonds are found.

The province of Brazil which produces diamonds is ſituated inland between 22½ and 16 degrees of ſouth latitude. Its circumference is near 670 leagues. On the eaſt it is limited by the captainry or province of Rio Janeiro; on the ſouth, by that of St. Paul; on the north, by the *Sertoens*, or interior part of the maritime province of the bay of All Saints, and part of that of the mines of Goyarel; on the weſt, laſtly, by another part of the laſt-mentioned province, and by thoſe deſerts and foreſts which are inhabited by the ſavages, and extend to the frontiers of Paraguay. On the ſide neareſt St. Paul there are vaſt uncultivated plains; the interior is divided by chains of mountains and hills, with ſuperb valleys and luxuriant fertile plains. It abounds with wood, and is watered by a great number of rivers and brooks, that facilitate the working of the mines of gold, which is obtained by waſhing in ſpangles from the river ſands, or in veins open to the day. This province is divided into four *comarcas* or diſtricts, which, reckoning from north to ſouth, are *Santo Joao del Rei*, *Villa Rica*, *Sabara* and *Sero Doſrio*, or cold mountain, called in the language of the ſavages *Yritauray*. The diamonds are found in this laſt diſtrict. The whole province is very rich in the ores of iron, antimony, zinc, tin, ſilver and gold.

The Pauliſts and inhabitants of the ancient captainry of St. Vincent were the firſt who diſcovered theſe mines, and peopled in great part the whole of this rich province, as well as thoſe of *Mato Groſſo*, *Cuiaba*, *Goyares*, and *Rio grande de San Pedro*. In a word, almoſt the whole of the interior of Brazil, with its immenſe riches, would have been ſtill unknown but for them. The metropolis at preſent enjoys the fruit of their eccentric activity and hazardous diſcoveries. Conſtantly with their arms in their hands to defend themſelves againſt the ſavages, in the midſt of impenetrable foreſts and ſolitary waſtes, expoſed for twelve years to famine and the inclemency of the ſeaſons, they overcame every obſtacle: nothing could check their unconquerable ſpirit. There is not a ſingle mountain, brook, or mine, which has not been traverſed, diſcovered, and viſited by them. Antonio Soary, a Pauliſt, who gave his name to one of theſe mountains, was the firſt who diſcovered and viſited the *Sero Doſrio*. Gold only was ſought for, but at laſt diamonds were diſcovered in the *Riacho Fundo*, whence they were firſt obtained, and afterwards in the *Rio de Peire*; a great number were likewiſe obtained from the *Giguitignogna*, a very rich ſtream; and laſtly, at the end of 1780 and beginning of 1781, a gang of near three thouſand interlopers, called *Grimpeiros*, diſcovered diamonds, and obtained an immenſe quantity from the *Terra de Santo Antonio*: but they were forced to abandon this ſpot to the Royal Farm, who took poſſeſſion of it. Then it was that the ſuſpicion was confirmed, that the mountains are the true matrix of diamonds; but as the work in the beds of rivers and on their banks is leſs tedious, can be conducted on a larger ſcale, and affords larger diamonds, the

---

* The Society of Natural Hiſtory of Paris. This account is inſerted in the Annales de Chimie, XV. 82, from which this tranſlation is made.

Farm abandoned the mountains, and formed great eſtabliſhments in the river of *Toucan-birnen*, which flows through the valleys of this chain, and is near ninety leagues in length. It was found by examination and digging, that the whole ſurface of the ground, immediately beneath the vegetable ſtratum, contained more or leſs of diamonds, diſſeminated and attached to a matrix ferruginous and compact in various degrees, but never in veins, or in the diviſions of geodes.

Attempts were made at firſt to prohibit the working; but the activity of individuals who infringed the order of Government, and ſent home diamonds by the ſhipping from Brazil, under the denomination of oriental diamonds, induced Government to eſtabliſh a farm. The firſt farmer was Riſberto Caldera, a Pauliſt, with the condition that no more than 600 negroes ſhould be employed in this work. This condition has always been evaded; for the number of ſlaves employed are from ſix to eight thouſand: and this number was ſcarcely diminiſhed when the Portugueſe Government, to put an end to this fraud, and the depreciation in price of diamonds proportional to the quantity brought to market, cauſed the undertaking to be carried on for its own account.. But at preſent, from other conſiderations, it is farmed again to individuals. Notwithſtanding the great profits which enter the royal treaſury, it is certain that the inhabitants of the province are greatly injured by it, becauſe the Diſtrict of Diamonds being continually enlarged, has condemned to deſtructive repoſe immenſe tracts very rich in gold.

Let us now proceed to the diamonds.

The figure of the diamonds of Brazil varies. Some are octahedral, formed by the union of two tetrahedral pyramids. This is the *adamas octaëdrus turbinatus* of Wallerius, or the octahedral diamond of Romé de l'Iſle. Theſe are almoſt always found in the cruſt of the mountains; others are nearly round, whether by a peculiar cryſtallization or by rolling. They reſemble thoſe oriental ſtones which the Portugueſe and the natives of India call *reboludos*, which ſignifies rolled. And laſtly, others are oblong, and appear to me to be the *adamas hexaëdrus tabellatus* of Wallerius. The two laſt are uſually found in the beds of rivers and broken places of their banks.

Diamonds are alſo found, as I have remarked, in the cruſt or external covering of mountains. Theſe maſſes are formed of a bed of ferruginous ſand, with rolled flints, forming an ochreous pudding-ſtone from the decompoſition of emery and muddy iron-ore; it is called *caſcalho*, and the beds or ſtrata *taboleiros*. Theſe *taboleiros* have different names, according to their ſituation or their nature. When the ſtratum is horizontal, and in the plane of the bed of the river, it is properly a *taboleiro;* but if it riſes in banks, it is called *gopiara;* laſtly, if the pudding-ſtone contains much emery, it is then denominated *tabanhua cauga* in Brazilian, that is to ſay, black-ſtone, or iron-ſtone.

In ſome places the *caſcalho* is uncovered; in others, it lies beneath a kind of vegetable muddy earth, *humus damaſcena* Linn. or beneath a reddiſh fat ſand, which ſometimes contains rounded flints. This happens in the returns of the mountains, or upon the banks of great torrents. This ſand is called *piſarra.* The bank or ſtratum beneath the *caſcalho* is either ſhiſtus rather ſandy, or the ſolid bog-ore of iron. It is likewiſe in the *caſcalho* that gold in ſpangles and in pyrites is found; the former of which is in my opinion afforded by the decompoſition of the auriferous pyrites. For the gold in veins has another form, and its matrix is either fat quartz or fine-grained tender *cos*, micaceous gneis, or the quartzoſe ore of iron, *tophus ferreus* Linn.

The exploring of diamonds is performed by changing the beds of ſtreams, in order that the ſand or gravel may be waſhed, and the diamonds ſelected; or by breaking the *caſcalbo* with large hammers, and afterwards waſhing it in troughs. This waſhing differs from that of gold, becauſe it requires a ſmall quantity of very clear water, and very little of the *caſcalbo* at a time; proportions which are preciſely contrary to thoſe required in waſhing gold. Black ſlaves are employed in this buſineſs, entirely naked excepting a cloth round their middle, in order that they may not embezzle any of the diamonds; but in ſpite of every precaution, and the vigilance of numerous inſpectors, they nevertheleſs find means of concealing them, which they ſell at a very low price, to the interlopers, for tobacco and rum.

This is all the information I can with certainty ſtate reſpecting diamonds. I have only to remark, that other provinces likewiſe afford them; as Cuiaba, and the diſtricts of Guarapuara, in the province of St. Paul; but theſe parts are not explored.

VI.

Abſtract of the Specification of Mr. William Desmond's *new Method of Tanning; with Obſervations relative to that Subject.*

EARLY laſt year * a patent was taken out by Mr. William Deſmond for the method or proceſs of tanning practiſed, as it is ſaid, with great ſucceſs in France by Seguin. The ſubſtance of Mr. Deſmond's ſpecification is as follows:

The art of tanning conſiſts in impregnating ſkins with a principle obtained from tan, with which they form a compound, inſoluble in water, and poſſeſſing other qualities well known in the ſubſtance called leather. He obtains the tanning principle by digeſting oak-bark, or other proper material, in cold water, in an apparatus nearly ſimilar to that uſed in the ſalt-petre works. That is to ſay, the water which has remained upon the powdered bark for a certain time, in one veſſel, is drawn off by a cock, and poured upon freſh tan. This is again to be drawn off, and poured upon other freſh tan; and in this way the proceſs is to be continued to the fifth veſſel. The liquor is then highly coloured, and marks, as Mr. Deſmond ſays, from ſix to eight degrees on the hydrometer for ſalts †. He calls this the tanning lixivium. The criterion to diſtinguiſh its preſence is, that it precipitates glue from its aqueous ſolution, and is alſo uſeful to examine how far other vegetable ſubſtances, as well as oak-bark, may be ſuitable to the purpoſe of tanning. The ſtrong tanning liquor is to be kept by itſelf. It is found by trials with the glue, that the tanning principle of the firſt digeſter which receives the clear water, is, of courſe, firſt exhauſted. But the ſame tan will ſtill give a certain portion of the aſtringent principle, or gallic lixivium, to water. The preſence of this principle is aſcertained by its ſtriking a black colour when added to a ſmall quantity of the ſolution of vitriol of iron or green copperas. As ſoon as the water from the digeſter ceaſes to exhibit this ſign, the tan is exhauſted, and muſt be replaced with new. The gallic lixivium is reſerved for the purpoſe of taking the hair off from hides.

---

* Jan. 15, 1795.

† Probably that of Baumé, deſcribed in his Elemens de Pharmacie. For the correſponding ſpecific gravity in the uſual form, ſee p. 39 of this Journal.

Strong hides, after waſhing, cleaning, and fleſhing, in the uſual way, are to be immerſed for two or three days in a mixture of gallic lixivium and one thouſandth part by meaſure of denſe vitriolic acid *. By this means the hair is detached from the hides, ſo that it may be ſcraped off with a round knife. When ſwelling or raiſing is required, the hides are to be immerſed for ten or twelve hours in another vat filled with water and one five-hundredth part of the ſame vitriolic acid. The hides being then repeatedly waſhed and dreſſed, are ready for tanning; for which purpoſe they are to be immerſed for ſome hours in a weak tanning lixivium of only one or two degrees; to obtain which, the latter portions of the infuſions are ſet apart; or elſe ſome of that which has been partly exhauſted by uſe in tanning. The hides are then to be put into a ſtronger lixivium, where in a few days they will be brought to the ſame degree of ſaturation with the liquor in which they are immerſed. The ſtrength of the liquor will by this means be conſiderably diminiſhed, and muſt therefore be renewed. When the hides are by this means completely ſaturated, that is to ſay, perfectly tanned, they are to be removed, and ſlowly dried in the ſhade.

Calf-ſkins, goat-ſkins, and the like, are to be ſteeped in lime-water after the uſual fleſhing and waſhing. Theſe are to remain in the lime-water, which contains more lime than it can diſſolve, and requires to be ſtirred ſeveral times a-day. After two or three days, the ſkins are to be removed, and perfectly cleared of their lime by waſhing and preſſing in water. The tanning proceſs is then to be accompliſhed in the ſame manner as for the ſtrong hides, but the lixivium muſt be conſiderably weaker. Mr. Deſmond remarks, that lime is uſed inſtead of the gallic lixivium for ſuch hides as are required to have a cloſe grain; becauſe the acid mixed with that lixivium always ſwells the ſkins more or leſs; but that it cannot with the ſame convenience be uſed with thick ſkins, on account of the conſiderable labour required to clear them of the lime, any part of which, if left, would render them harſh and liable to crack. He recommends, likewiſe, as the beſt method to bring the whole ſurface of the hides in contact with the lixivium, that they ſhould be ſuſpended vertically in the fluid by means of tranſverſe rods or bars, at ſuch a diſtance as not to touch each other. By this practice much of the labour of turning and handling may be ſaved.

Mr. Deſmond concludes his ſpecification by obſerving that in ſome caſes it will be expedient to mix freſh tan with the lixivium; and that various modifications of ſtrength and other circumſtances will preſent themſelves to the operator. He affirms that, in addition to the great ſaving of time and labour in this method, the leather, being more completely tanned, will weigh heavier, wear better, and be leſs ſuſceptible of moiſture than leather tanned in the uſual way; that cords, ropes, and cables, made of hemp or ſpeartery, impregnated with the tanning principle will ſupport much greater weights without breaking, be leſs liable to be worn out by friction, and will run more ſmoothly on pulleys; inſomuch that, in his opinion, it will render the uſe of tar in many caſes, particularly in the rigging of ſhips, unneceſſary; and, laſtly, that it may be ſubſtituted for the preſervation of animal food inſtead of ſalt.

---

* Marking 66 degrees on the hydrometer for acids. Sp. gr. 1.848 of Baumé.

The intelligent manufacturer will readily perceive that this new method is grounded on two particular circumſtances, beſides a more ſcientific management of the general proceſs than has been uſual. The firſt conſiſts in the method of determining the preſence and quantity of the tanning principle, by the hydrometer and the precipitation of glue: the ſecond, in applying this principle in a concentrated ſtate, more early in point of time than has, perhaps, been hitherto done. Our tanners, after the common previous proceſſes and unhairing by acids, by lime, or by piling the hides that they may heat and begin to putrefy, apply the ſolution of tan, which they call ouze, in a great number of pits in the tan-yard. They begin with the weakeſt ſolution, which has been uſed, and is of a lighter colour than the other. And they paſs the hides, according to their judgment and experience, into ouzes which are ſtronger and ſtronger, until at laſt, in certain caſes, the hides come to be buried for a certain time in a ſolid maſs of tan or oak-bark. The oak-bark itſelf, in the pits, is not only the ſource from which the water extracts the tanning principle; but ſeems, likewiſe, in ſome meaſure, during the laſt ſtages of the proceſs, to operate mechanically by keeping the ſurfaces of the hides from touching each other.

On the occaſion of this apparently important diſcovery, I applied to one of the firſt manufacturing houſes in the Borough of Southwark, to make enquiries concerning its value. One of the partners, who appeared to have paid conſiderable attention to the proceſſes of his manufactory, informed me, that the ſtrong ſolution obtained from tan had been well known to them for ſome years under the name of eſſence of tan; and that it had been propoſed to uſe it as the means of bringing out the complete leather in a ſhort time. I aſked whether the objection to its uſe might conſiſt in the outer part of the ſkin becoming more perfectly tanned in a ſhort time than the inner part; ſo as to defend this laſt from the ſubſequent change that might have taken place by a ſlower operation. He anſwered, that this was partly the reaſon why it was neceſſary that the ouzes ſhould be gradually applied from the loweſt to the higheſt ſtrength. But the chief reaſon he urged in favour of the ſlow proceſs was, that the hides were found to feed and improve in their quality by remaining in the pit. I could not gain ſatisfactory information what the nature of this feedingand improvement might be. The fact, as looſely ſtated, appeared to be, that the ſkin became much thicker, denſer, and heavier in the firſt ouzes during a certain time; and that theſe advantages of thickneſs, denſity, and weight continued to exiſt in a certain degree to the very end of the entire proceſs. For, as he remarked, they know very well that, by bringing the ſkins more haſtily into the ſtronger ouzes, they will be ſooner converted into leather; and that a ſkin which ordinarily requires fifteen months, might be tanned in nine. But then, added he, the ſkin which comes forward in nine months, from its thinneſs, and other inferior qualities, will afford ninepenny ſoles for ſhoes; whereas, if it had undergone the longer proceſs, the ſoles would have been worth a ſhilling:—a difference which largely repays the common intereſt of capital for the exceſs of time.

On the other hand it may be remarked, that reſults of this nature require to be grounded on experiments carefully undertaken, under circumſtances differing only with regard to the object of inveſtigation, and often repeated. It does not appear from my enquiries that this has actually been the caſe; and I have been informed that excellent leather has been produced in the new method. But I have heard nothing of its appearance in the market, though upwards of a year has elapſed ſince this patent was ſealed.

A very full and circumſtantial account of this proceſs, which Mr. Deſmond affirms he received from a certain learned foreigner, was inſerted in the *Moniteur*, and thence tranſlated and publiſhed in the Engliſh *Courier* ſome time in the month of Auguſt 1795.

VII.

Deſcription and Account of a New Preſs operating by the Action of Water, on the Principle of the Hydroſtatic Paradox. Invented by JOSEPH BRAMAH, *Engineer.*

FIG. 1, Plate II, is a front ſection of a common preſs for books and papers, and the like. Fig. 2 exhibits a perſpective view of the ſame inſtrument on that ſide to which the pump is fixed. The letters diſtinguiſh the ſame parts in both. ABCD is the frame. I is a ſtrong metallic cylinder, in which the rammer or piſton EF moves. To the upper part of this piſton is applied an iron table, by the motion of which upwards the preſſure is communicated to the articles H. QR repreſents a ſmall ciſtern, containing water. Within this ciſtern is fixed a ſmall forcing pump, of which K is the barrel, L the piſton, M a ſide valve formed of metal, and opening inwards beneath the piſton. The nature and effect of this valve may be eaſily underſtood from the figure. It conſiſts of a metallic rod, at one end of which there is a nob turned conical next the ſtem, ſo as accurately to fit the conical face of the hole into which it is put. The tail is filed on one ſide, ſo that it does not entirely fill the cylindrical hole which it occupies; by which means a paſſage is afforded for water when the head of the valve is raiſed. In the ſtate of inaction, the valve is kept ſhut by the operation of a ſpiral ſpring at the other end of the tail. N is another valve of the ſame kind opening downwards. O repreſents the rod of the piſton, with a contrivance for keeping a vertical poſition during the working. The effect of this contrivance will be underſtood without difficulty by comparing the two drawings. S is the lever or handle for working the engine.

Its action is as follows: When the handle S is raiſed, it brings up the piſton L, which would leave a vacuum beneath if the preſſure of the atmoſphere did not force the water in through the ſide valve M. The lever is then to be preſſed down, which cauſes the ſide valve to ſhut, and forces the water through the bottom valve N, whence it paſſes through the pipe P into the cavity F of the great cylinder I, and raiſes the piſton or preſſing rammer. A repetition of the ſame proceſs forces more water in, and the preſſure may in this manner be carried to a great extent. When it is propoſed to relieve the action, the lever S muſt be preſſed down, which, by the mechanical contact of the lower extremity of the piſton L, againſt the tail of the lower valve N, keeps that valve open. In this ſituation, the lever TU is to be preſſed towards R, and will open the valve M. Both valves being in this manner opened at once, the paſſage between the internal part of the great cylinder and the ciſtern QR becomes free, and conſequently the table G and the rammer EF deſcend by their own weight, and reſtore the engine to its original ſituation.

There is no difficulty in computing the force of this inſtrument. If the diameter of the barrel K be one quarter of an inch, and that of I one inch, that is to ſay, four quarters of an inch; one pound lodged upon the piſton-rod W will be in equilibrio with ſixteen pounds lodged upon the table G; the weights of the parts of the engine attached to, and moving with, each piſton, being reſpectively included. And if the length of the lever SY be fifteen inches, and the diſtance XY between the centres of motion and of action be two inches, one pound at the end of S will gain an advantage of 7½ times when compared with that at W. Inſtead, therefore, of ſixteen pounds upon the table G being equal in effect to counterpoiſe this laſt action, there will be required upwards of 120 pounds. But a man, in this action of pumping by a downward preſſure, can without difficulty apply his whole weight, and with great eaſe one third or one fourth of his weight, ſuppoſe 50 pounds. In this caſe the preſſure will be equivalent to fifty times 120 pounds, or 6000 pounds, that is to ſay, nearly three tons.

To compare this engine with a ſcrew, in theory, we muſt enquire what fineneſs of thread and length of lever would afford a purchaſe of 120 to one. Let us ſuppoſe the thread of a ſcrew, ſubſtituted in the place of the barrel I, to be one tenth of an inch thick; the diſtance from the top of one thread to the top of the next will in this caſe be one fifth of an inch. This is the ſpace through which the weight muſt riſe in one revolution. The power muſt therefore move through 120 times that ſpace, namely twenty-five inches; but a lever or radius four inches long will deſcribe a circle ſomewhat larger than this, and conſequently ſuch an engine would in theory be equal in power to the hydraulic engine we have been contemplating.

But when the ſubject is viewed practically, the difference between the two engines appears to be very remarkable. All practical men know how very large a part of the force operating by means of engines is employed in overcoming frictions. Every one is aware of the extreme friction between ſolids, and the very ſlight friction which takes place between the parts of fluids. This is ſeen in the common expedient of oiling the pivots of wheels, and in the very gradual decay of motion in fluid bodies; while ſolids moving on each other ſtop at once, as ſoon as the force is diminiſhed to a certain degree. The ſcrew is an organ peculiarly liable to friction, and this friction is always much greater than the whole of the reacting force; for there are few inſtances where a ſcrew will return from extreme preſſure, when the agency upon the lever is withdrawn. It is alſo to be conſidered, that the whole force of the weight or reſiſtance acts directly upon the face of the ſcrew, at which the motion is required to take place. It has not been appretiated in what degree this reſiſtance or friction increaſes with the weight. In lighter actions the ſimple ratio has been inferred; but under more ſevere preſſures the two metallic faces extrude the greater part of the half-fluid matter between them, and appear by the magnitude of their reſiſtance to be attached to each other by a proceſs of the nature of coheſive attraction. For theſe and other reaſons, it appears nearly impracticable to form any compariſon between two engines ſo different in principle, but ſuch as ſhall be deduced from immediate experiment of their effects. I am not in poſſeſſion of numerical data to indicate the actual power of ſcrew-engines or preſſes; which are perhaps the leſs neceſſary, becauſe thoſe who are the moſt intereſted in the ſucceſs of an improvement like the preſent, are for the moſt part able to come at theſe without difficulty. I intend, beſides, to make theſe the ſubject of a future communication. The effects which I obſerved were theſe.

A machine of the new conſtruction here deſcribed, was employed to preſs ſome papers. The force applied to the lever was ſo ſlight, that the inſtrument required no faſtening to the table on which it ſtood: but the effect on the upper bar, A B, which was 3½ inches thick, was ſuch as bended it out of a ſtraight line upwards of a quarter of an inch, and I apprehend that it might have eaſily been broken by continuing the preſſure. With a ſcrew-preſs, the ſcrew of which was iron, and nearly of the dimenſions before mentioned, excepting that the lever was twelve inches long inſtead of four inches, and the action on the lever upwards of two hundred weight, applied with a jerk, the effect was nearly the ſame. Here I ſhould eſtimate the advantage to be very much in favour of the hydraulic engine.

In another engine of this kind, the diameter of the great piſton was four inches, and of the ſmaller three-eighths of an inch; and the advantage given by the lever or handle was twelve to one. Above the piſton of the great cylinder was applied a long lever, at one end of which was an axis, and at the other end a large ſcale to hold weights: it contained twenty hundred weight. The diſtance between the axis of motion of this lever and the part where it acted on the piſton was ſix inches; and the diſtance from the ſame axis to the extremity where the ſcale was hung, was 126 inches. Every hundred weight in the ſcale conſequently preſſed upon the piſton with a force equal to twenty-one hundred weight; whence the whole preſſure was twenty-one tons. It was eaſy to work the lever briſkly with one hand, and each ſtroke raiſed the ſcale near one-third of an inch. Forty-ſeven pounds hung at the end of the lever, carried it down with a moderate ſwiftneſs of working; but a weight of only forty-three pounds remained in equilibrio, and did not deſcend. Now, as the true weight in theory was thirty-two pounds, as deduced from the action of the parts in the manner already done with regard to the ſmall machine, it follows that leſs than one-third of the actual power was employed to give velocity and overcome all friction.

It may be remarked, that the principal frictions in theſe machines muſt be at the circumferences of the piſtons, and that theſe do not increaſe in the ſimple, but in leſs than the ſub-duplicate ratio of the power. For if the diameter of the great cylinder were double, every thing elſe remaining unchanged, the ſurface of its piſton, and conſequently the power, would be quadrupled. But the friction would be only doubled, and that merely at the leathering of the greater piſton.

As the preſſure in the experiment laſt mentioned amounted to 47040 pounds upon the great piſton of four inches diameter, or ſixteen circular inches ſurface, it amounted to 2940 pounds upon each round inch. But the medium preſſure of the atmoſphere on a round inch is near twelve pounds, conſequently the action was equal to 245 atmoſpheres: and as each of theſe correſponds with a column of 34 feet of freſh water at a medium, the water in the cylinder was preſſed in the ſame manner as if the whole column had been 8330 feet, or 1⅔ mile, long.

Large preſſes of this conſtruction are made with two pumps of 1¼ inch bore, and a cylinder of ſeven inches. Theſe have been uſed in preſſing hay and cotton for package; and, as I am informed, are effective in producing a greater condenſation on the material with a much leſs application of moving power and conſumption of time. But of this and other particulars, as I have not yet had an opportunity of examining the facts myſelf, I ſhall forbear to ſpeak at preſent *.

---

* Mr. Bramah, who conſtructs theſe engines, has obtained a patent for the invention.

VIII.

The Proceſs for giving a beautiful White Colour to Raw Silk, without Scouring.

By M. Baumé[*].

BERTHOLLET, in his Elemens de l'Art de la Teinture, publiſhed in the year 1791, after deſcribing the uſual methods of depriving ſilk of its reſinous or gummy matter[†], proceeds to remark, that, in the manufacture of blonds and gauzes, the natural elaſticity and ſtiffneſs of this article are required to be preſerved; whence it has become a deſideratum to render the yellow ſilk of Europe white like that of China, without depriving it of its gum. He adds, that Mr. Baumé has ſolved this intereſting problem, but had kept his proceſs a ſecret; but from the facts he had poſſeſſed the means of obtaining, it appeared liable to accidents, and that the chief difficulty conſiſted in giving an uniform white colour when large quantities were operated upon. He alſo mentions a difficulty in dreſſing the whitened ſilk ſo as to prevent its curling, and obſerves that it ought certainly to be kept conſtantly ſtretched during the drying. It is beſides requiſite that the ſpirit of wine ſhould be recovered after the proceſs, which would elſe be rendered too expenſive. This author does not ſay whether the white Chineſe ſilk is ſubject to the ſame inconvenience of curling when dyed, which, it may be remarked, is a property of no conſequence where the material is to be applied in the manufacture of white goods. The motives which led Mr. Baumé to communicate his proceſs to the world, originally retained by him as a lucrative ſecret, do not appear. Whether the miſtakes of thoſe who carried it into effect in the large way might have led him to vindicate the reality of his diſcovery by publication; or whether the commercial advantages derived from ſuperiority of quality and cheapneſs in his article over the Chineſe ſilk in the market of France, might in the end have proved of leſs value than the ſcientific reputation to be derived from its diſcloſure; are circumſtances which will, no doubt, have their proper weight with ſuch manufacturers as may be induced gradually to adopt this proceſs.

The ſilk of Nankin is perfectly white, ſilvery, brilliant, and poſſeſſes all the elaſticity of raw ſilk. Our author affirms, that the value of this article imported into Europe amounts to upwards of twenty millions of livres[‡], of which France conſumes about four or five millions in gauzes, blonds, ribbons, &c. This was formerly ſuppoſed to be produced of a white colour from the worm. The late Mr. Trudaine, intendant of commerce, procured the eggs of the ſilk-worm from China, and cultivated them. The produce conſiſted of yellow cocons, and others of the moſt perfect whiteneſs. The latter afforded ſilk equal in this reſpect to that of Nankin. But Mr. Baumé affirms, that moſt of the Nankin ſilk is bleached by art, and, as he thinks, by a proceſs ſimilar to his own.

As it is impoſſible to wind off a large quantity of ſilk in the ſhort time previous to that of the inſects eating their way through the maſs, it is uſual, in the firſt place, to deprive them of life. This is commonly done by expoſing the cocons, properly wrapped up, for two hours to the heat of about 158 degrees of Fahrenheit in an oven; after which they are kept for a certain time in a maſs to preſerve their heat, and effectually deſtroy ſuch of the inſects as might have eſcaped the power of the oven. The effect of this proceſs is, that the ſilk is hardened, and is more difficult to wind off than before. Hence the product of ſilk is leſs by one ninth part in quantity, and inferior in quality to what might have been obtained by winding off without this previous baking. Mr. Baumé, not only from theſe views, but likewiſe becauſe the ſilk which has not been baked proves ſuſceptible of a greater luſtre, was induced to deſtroy the chryſalis by ſpirit of wine. For this purpoſe he diſpoſes them in a wooden box in a ſtratum ſix inches deep: upon each ſquare foot half a chopin, or ſomewhat more, of ſpirit of wine is to be ſprinkled with a ſmall watering-pot made for that purpoſe. This quantity anſwers ſufficiently near to our half-pint. The liquid is to be equally diſtributed, but it is not neceſſary that all the cocons ſhould be wetted. They are then to be mixed by hand. In the next place another ſtratum is to be formed over the firſt, nearly of the ſame depth, which is to be ſprinkled and treated as before. By this method of proceeding, the box becomes filled, and muſt then be covered, and left for twenty-four hours, during which time they become ſpontaneouſly heated to about 100 degrees, and the vapour of the ſpirit of wine exerts itſelf with wonderful activity. Five hundred French pounds * of the cocons require ten French pints, which is nearly the ſame number of Engliſh quarts. After this treatment they muſt be ſpread out to dry, which happens in a ſhort time, and is abſolutely neceſſary previous to winding off.

---

[*] The original Memoire from which the facts here related are taken, is inſerted in the Journal de Phyſique, xlii. 375—399.

[†] Tom. i. p. 146; or p. 141 of Dr. Hamilton's tranſlation.

[‡] About eight hundred and thirty thouſand pounds ſterling.

When the operator propoſes in this manner to extinguiſh various parcels of cocons belonging to different individuals, each parcel may be tied up looſely in a canvas bag, and wetted on the outſide previous to cloſing the box.

The ſpirit of wine to be uſed in this operation, ought to be of the ſtrength of 34 degrees of Baumé's hydrometer at the temperature of 55 degrees. It is of the greateſt importance to uſe that ſpirit only which has been kept in veſſels of glaſs, of tinned copper, or of pure tin. Leaden veſſels are abſolutely to be rejected; wooden veſſels tinge the ſpirit, which gives the ſilk a degree of colour of conſiderable ſolidity, and very inimical to the bleaching proceſs.

With regard to the advantages of this method of extinction, in preference to that of the oven, the author remarks, that the coſt of labour and fuel added to the loſs of ſilk, and the probability of injury from too much or too little heat, conſtitute a ſum of diſadvantage much greater than the coſt of the ſpirit of wine. It is beſides a conſiderable advantage, that the ſpirit of wine renders more diſtinguiſhable ſuch cocons as have periſhed previous to the application of the ſpirit. Theſe afford a much worſe ſilk, and muſt be picked out.

The ſilk is wound off upon a reel, while the cocons are kept immerſed in water almoſt boiling. Upon this part of the proceſs Mr. Baumé remarks, 1ſt, That the dead cocons muſt be ſeparated. Theſe are known by the brown or black ſpots on their ſurface. 2. That well-water, which on account of its clearneſs is almoſt univerſally uſed in the ſilk manufactories, moſtly contains nitre, and is extremely prejudicial to the bleaching proceſs. The preſence of nitrous acid gives a yellow colour, which reſiſts bleaching and even ſcouring; he therefore recommends river-water. 3. In ſome countries a ſmall quantity of

---

* The Paris pound is to the Engliſh avoirdupois pound as 756 to 700. I have not reduced theſe quantities, becauſe the operation requires no great preciſion. N.

alum is uſed. Neither this nor any other ſaline ſubſtance is of the leaſt advantage to the colour, beauty, or quality of the ſilk.

At the four places of contact of the ſilk upon the reel, all the threads ſtick together. It is abſolutely neceſſary that this ſhould be remedied. The method conſiſts in ſoaking the ſilk in a ſufficient quantity of warm water, at about 90 degrees, for about two hours; after which the threads are to be ſeparated by opening the hanks upon a pin, and lightly rubbing the parts which cohere. When the ſilk is dry, it is to be looſely folded in its original form, and is ready for bleaching.

The ſilk while wet is ſoft, and part of its gummy matter is in ſuch a ſtate, that its threads would readily adhere, if wrung while warm for the purpoſe of clearing it of the water. After ſuch improper treatment there would be no other remedy than to ſoak it again in warm water.

The apparatus for bleaching the ſilk conſiſts of a ſtone-ware veſſel, nearly of a conical form, capable of holding about twelve gallons, having a large opening at the one end, and a ſmaller of about an inch diameter at the other end. Common pottery cannot be uſed in this operation, becauſe it is ſoon rendered unſerviceable by the action of the marine acid, and the ſtone-ware itſelf is not very durable. This veſſel muſt be carefully examined, to aſcertain that it does not leak in the ſlighteſt degree; after which the inſide is to be rubbed with a pumice-ſtone, to clear it of aſperities which might break the threads. A cover of the ſame material is to be fitted on by grinding; and the ſmaller aperture, which in the uſe is the loweſt, is to be cloſed with a good cork, in the middle of which is thruſt a ſmall glaſs tube about a quarter of an inch in diameter; this is likewiſe ſtopped with a cork, excepting at the time when it is required to draw off the liquid contents of the jar. A ſmall perforated falſe bottom is placed within the veſſel, to prevent this tube from being obſtructed.

This jar, or as many of them as the purpoſes of the manufactory may require, is ſupported by a wooden frame or table, at ſuch a height that a caſk may be conveniently placed beneath to receive what may flow from the glaſs tube in the ſeveral periods of the operation.

Six pounds of yellow raw ſilk are to be diſpoſed in the earthen pot; upon this is to be poured a mixture, previouſly made, of forty-eight pounds* of ſpirit of wine at 30 degrees, with twelve ounces of very pure marine acid, abſolutely exempt from all preſence of nitrous acid, and of the ſtrength of 14 or 15 degrees of Baumé's hydrometer. The pot is then to be covered, and the whole left in digeſtion till the following day, or until the liquor, which at firſt aſſumes a fine green colour, ſhall begin to aſſume that of a duſky brown (feuille morte).

The acidulated ſpirit is then to be drawn off. To prevent evaporation, M. Baumé thruſts a cork in the bung-hole of the receiving caſk, in which is a ſliding glaſs tube. The uſe of this tube is completely to ſurround the ſmall tube proceeding from the earthen veſſel. When the whole of the fluid is thus almoſt entirely drawn off, clean ſpirit of wine is poured upon the ſilk, and drawn off repeatedly until it paſſes colourleſs. The ſilk is then ſuffered to drain without ſtirring it. In this ſtate it is ready for a ſecond infuſion.

Forty-eight pounds of ſpirit of wine acidulated with twelve ounces of marine acid is now to be poured on the ſilk, and the whole ſuffered to remain for twenty-four hours or longer, until the ſilk becomes perfectly white. The time required for this ſecond infuſion is commonly longer than for the firſt: it ſometimes amounts to two, three, or even ſix days, according to circumſtances, particularly the temperature and the nature of the ſilk. Silk which has been in the oven is in general more difficult to bleach.

---

* The pound is nearly a pint, and is divided into ſixteen ounces. With regard to the ſtrengths, ſee Article IX. p. 37.

When the ſilk has thus obtained its utmoſt degree of whiteneſs, the acidulated ſpirit is to be drawn off into a ſeparate veſſel. This fluid is but ſlightly coloured, and may be uſed again in the firſt infuſion of other yellow ſilk, with the addition of ſix ounces more of marine acid. The receiving veſſel is to be removed, and another clean veſſel ſubſtituted in its place. The ſilk is then ſprinkled with clean ſpirit, and occaſionally preſſed down with the hand. As ſoon as the ſpirit of wine comes off abſolutely colourleſs, a third infuſion is to be made by pouring upon the ſilk forty-eight pounds of the pure ſpirit without acid, which is to remain till the following day: it is then to be drawn off, and reſerved for waſhing other ſilk after the firſt infuſion.

After the ſilk has been left to drain, and affords no more ſpirit, it ſtill retains its own weight of that fluid. This is recovered by the very ſimple proceſs of ſprinkling the ſilk with a ſmall quantity of very clear river-water at a time. While the water applies itſelf and ſubſides along the ſilk, it drives the ſpirit of wine before it, ſo that the firſt portions which flow from the tube are ſcarcely diminiſhed in ſtrength. The addition of water is to be continued until nothing but mere water comes off below.

In this ſituation the ſilk is found to be well bleached, but ſtill retains a portion of marine acid ſufficient to render it harſh to the touch, and after a time brittle. It muſt be waſhed off with water. The beſt method is to put the ſilk looſely into a coarſe woollen bag, which is to be ſecured looſely in another cloth like a ſmall bed or pillow, then placed in a baſket and left in a running ſtream for five or ſix hours; but where the convenience of a ſtream is wanting, the earthen pot containing the ſilk is to be covered with a cloth, and water pumped through it for five or ſix hours, or until that which iſſues from the lower aperture gives no red colour to the tincture of tournſol. At this period the lower opening is to be cloſed and the veſſel filled with water, which muſt be changed once or twice in twenty-four hours.

The time required for waſhing was occaſionally abridged by paſſing ſpirit of wine, or river-water impregnated with a ſmall portion of alkali, through the ſilk. The neutral ſalt thus produced is in fact leſs adherent to the ſilk than the acid itſelf, but nevertheleſs requires to be waſhed off with a very large quantity of water.

In theſe as in every other proceſs relating to the ſilk, great care muſt be taken to aſcertain that the water made uſe of contains no nitrous acid, which would infallibly occaſion imperfection of colour, or ſpots in the article. After this treatment the ſilk is ready for drying and luſtering; previous to the deſcription of which, the author makes ſeveral remarks to the following purport:

Though the mineral acids are the moſt powerful and deſtructive of all ſaline ſubſtances, yet they may be applied to ſilk when diluted with ſpirit of wine in very conſiderable doſes. In trials made to aſcertain the maximum, two ounces of marine acid were added to one pound of ſpirit of wine, without altering the ſilk. Two drams of marine acid cauſe a very perceptible alteration in one pound of ſilk. I ſuppoſe he means pure acid, or perhaps diluted with water; for the paſſage as it ſtands is obſcure. Numerous experiments have ſhewn that the marine acid is preferable to any other. The proportions admit of much latitude, though he prefers the doſe hereinbefore deſcribed.

Spirit of wine which has been mixed with nitrous acid, cannot be uſed in bleaching, even though afterwards rectified upon an alkali, becauſe it ſtill retains a portion of nitrous gas.

Pure ſpirit of wine without acid extracts a fine yellow colour from ſilk, which does not ſeparate for years, even though expoſed to the ſun's light. Yellow ſilk expoſed to the ſun loſes its colour in a ſhort time. The acidulated ſpirit which has been uſed in the infuſion of ſilk, is changed by expoſure to the ſun, but not in ſuch a manner as to be rendered fit for uſe a ſecond time.

In order to obtain a beautiful white colour, it is eſſential that the ſilk ſhould be immerſed in a large quantity of the fluid, eſpecially at the firſt infuſion. Without this management it would become neceſſary to make three infuſions in the acidulated ſpirit. When the firſt infuſion is well managed, the ſilk will have loſt all its yellow colour, and become conſiderably white, at the ſame time that the liquor will have begun to change colour a little. As long as it continues of a fine green, it is certain that it has not exhauſted its whole action upon the ſilk.

The duration of this firſt infuſion may be longer or ſhorter, without inconvenience, according to the temperature. When the temperature is at 20 degrees of Reaumur, which anſwers to 77 of Fahrenheit, the firſt infuſion is often made in ten or twelve hours. In ſmall experiments the heat of the atmoſphere may be ſupplied by the water-bath; in which caſe, all the infuſions are eaſily made in the courſe of a day.

When the firſt infuſion is finiſhed and the liquor drawn off, the ſilk appears greeniſh: the ſubſequent waſhings in ſpirit of wine clear it of the liquor it retained. This ſprinkling ſhould be made with the watering-pot, otherwiſe the quantity poured will be greater, and the management more waſteful.

The cocons may be bleached in this way, but the inconveniences are too great to render this proceſs deſirable.

Pieces of gauze and entire garments of ſilk have been ſucceſsfully bleached in this way.

The fineſt natural white ſilks are rendered infinitely whiter by this proceſs. Spirit of wine alone has the property of depriving yellow ſilk of its colour, which it brings to the ſtate of the naturally white ſilk. In this ſtate the ſilk is diſpoſed to acquire a greater degree of brightneſs by a ſingle infuſion in the acidulated ſpirit. This proceſs has its advantages over the other, to which it is alſo inferior in certain reſpects; concerning neither of which the author has entered into any detail.

The colouring matter was found to be a reſin perfectly animalized, affording by diſtillation the ſame products as other animal matters, and the concrete volatile alkali.

Silk whitened by ſcouring may be dried freely in the air without affecting its luſtre. This is not the caſe with the ſilk bleached in the gum: if it be left at liberty to dry in the air, it reſembles white flax without any luſtre. The beauty of this ſilk conſiſts in its ſhining brilliancy; to ſecure which, it muſt be dried in a ſtate of tenſion. Mr. Baumé has contrived a ſimple machine for this purpoſe. It conſiſts of a ſtrong ſquare frame of wood ſtanding upright upon feet: the upper horizontal bar is ſix feet long, and has ſix iron pins driven through it at equal diſtances, ſo as to project on each ſide for the purpoſe of receiving twelve bobbins. The lower horizontal bar is moveable up and down in a mortice by means of a ſcrew at each end: it is furniſhed with ſix holes, adapted to receive as many pins to correſpond with thoſe above. The ſkains of ſilk are to be dreſſed and arranged upon wooden pins, as they are taken out of the ſack from waſhing. As ſoon as there are twelve together, they are to be wrung with a ſtaff; after which the ſkains are to be hung one by one upon as many bobbins put upon the upper pins of the ſquare frame. Another bobbin with tails is to be inſerted in the lower loop of the ſkain, and faſtened to the correſponding pin of the lower bar, by means of a ſtrap and hook, which need not be deſcribed to ſuch as are ſlightly acquainted with mechanical objects. When the machine is thus ſupplied with ſkains on both ſides, the lower bar of the frame is to be preſſed down by the ſcrews until the ſilk is moderately ſtretched. When it is dry, the ſcrews are to be equally ſlackened, the ſkains taken off, and folded with a ſlight twiſt, that they may not become entangled.

After this deſcription of the whole of his proceſs, the author proceeds to make certain general remarks on the white China ſilk. He obſerves, that in his proceſs the ſilks acquire the perfect whiteneſs without much handling, and conſequently that there is little cauſe for them to become entangled. Accordingly the loſs in unwinding is found to be no greater than when they are unwound in the yellow ſtate: that is to ſay, from a dram to a dram and a half in the pound. This ſaving is of the greateſt importance in the price of the ſilk.

The ſilk of Nankin, which he ſuppoſes to be bleached by ſome proceſs of the ſame nature, is probably handled much more. The loſs is nearly twelve per cent. when it comes to be opened, and not unfrequently even twenty-five per cent.; a loſs which cannot in any reſpect ariſe from the package. The quality of the Nankin ſilk differs much in the package; the external part being always of the beſt quality, and that which is packed within is of ſuch an inferior quality as ſometimes not to exceed half the value. On examining this ſilk, it not only exhibited unequivocal marks of alkali, but its imperfections were alſo of the ſame kind as thoſe which had occurred to Mr. Baumé during the progreſſive improvement of his own manipulations. The beſt China ſilk was neither improved nor injured by the proceſs of Baumé; whence he concludes that they are not naturally white, but have undergone a proceſs ſimilar to his.

The reſult of the whole is, that the yellow ſilks of Europe may be bleached to equal or greater perfection than thoſe of Nankin; and that theſe may be even greatly exceeded by winding the naturally white ſilk apart from the other, and bleaching it by itſelf.

The methods of recovering the ſpirit ſo as to be uſed again, and of obtaining the marine acid in the requiſite ſtate of purity, will be deſcribed in our next.

IX.

On the Hydrometer of BAUME.

AS many French chemiſts refer to the peſe-liqueur of Baumé, which has never been uſed in this country, it will be of advantage to deſcribe the method by which it is conſtructed, and ſhew the ſpecific gravities indicated by the graduations upon the ſtem. Inſtead

ſtead of adopting the ſimpler method of immediate numerical reference to the denſity of water expreſſed by unity, as is done in all modern tables of ſpecific gravity, he had recourſe to a proceſs ſimilar to that of graduating the ſtems of thermometers from two fixed points. The firſt of theſe points was obtained by immerſing his inſtrument, which is the common areometer, conſiſting of a ball, ſtem, and counterpoiſe, in pure water. At that point of the ſtem which was interſected by the ſurface of the fluid, he marked *zero*, or the commencement of his graduations. In the next place he provided a number of ſolutions of pure dry common ſalt in water: theſe ſolutions contained reſpectively one, two, three, four, &c. pounds of the ſalt; and in each ſolution the quantity of water was ſuch, as to make up the weight equal to one hundred pounds in the whole; ſo that in the ſolution containing one pound of ſalt, there were ninety-nine pounds of water; in the ſolution containing two pounds of ſalt, there were ninety-eight pounds of water, and ſo of the reſt. The inſtrument was then plunged in the firſt ſolution, in which of courſe it floated with a larger portion of the ſtem above the fluid, than when pure water was uſed. The fluid, by the interſection of its ſurface upon the ſtem, indicated the place for marking his firſt degree; the ſame operation repeated, with the fluid containing two pounds of ſalt, indicated the mark for the ſecond degree; the ſolution of three pounds afforded the third degree; and in this manner his enumeration was carried as far as fifteen degrees. The firſt fifteen degrees afterwards, applied with the compaſſes repeatedly along the ſtem, ſerved to extend the graduation as far as eighty degrees, if required.

This inſtrument, which is applicable to the admeaſurement of denſities exceeding that of pure water, is commonly diſtinguiſhed by the name of the Hydrometer for ſalts.

The Hydrometer for ſpirits is conſtructed upon the ſame principle; but in this the counterpoiſe is ſo adjuſted, that moſt part of the ſtem riſes above the fluid when immerſed in pure water, and the graduations to expreſs inferior denſities are continued upwards. A ſolution of ten parts by weight of ſalt in ninety parts of pure water, affords the firſt point, or zero, upon the ſtem; and the mark indicated by pure water is called the tenth degree; whence, by equal diviſions, the remaining degrees are continued upwards upon the ſtem as far as the fiftieth degree.

Theſe experiments, in both caſes, are made at the tenth degree of Reaumur, which anſwers very nearly to fifty-five of Fahrenheit.

M. Baumé affirms, that all his inſtruments, conſtructed in theſe methods, agreed together with the utmoſt preciſion. From a few experiments, which however require to be carefully repeated, I am diſpoſed to apprehend that the ſolutions of common ſalt do not give a ſufficiently accurate original point, and that they may differ not only from the comparative dryneſs of the ſalt in different experiments, but likewiſe the ſtate of its cryſtals, whether haſtily or ſlowly ſeparated in their original fabrication, the purity being ſuppoſed the ſame. Such differences muſt conſiderably affect the remote terms formed by repetition of the experimental interval in either inſtrument. I ſuppoſe all M. Baumé's inſtruments were conſtructed from ſolutions made once for all, and reſerved for this purpoſe; and that the French chemiſts who uſed them were ſupplied under his direction. For theſe reaſons I am more inclined to deduce the ſpecific gravities from the experiments of himſelf and another accurate poſſeſſor of theſe inſtruments, than to recur to the original method of conſtruction.

M. Baumé,

M. Baumé, in his Elemens de Pharmacie *, from which the whole of this account is deduced, has given a table (p. 410.) of the degrees of his Hydrometer, indicated by different mixtures of ardent ſpirit and pure water; where, he ſays, the ſpirit made uſe of gave thirty-ſeven degrees at the freezing point of water; and in a column of the table he ſtates the bulk of this ſpirit, compared with that of an equal weight of water, as 35½ to 30. The laſt proportion anſwers to a ſpecific gravity of 0.842, very nearly. A mixture of two parts, by weight, of this ſpirit, with thirty of pure water gave twelve degrees of the Hydrometer at the freezing point. This mixture, therefore, contained 6¼ parts of Blagden's ſtandard to 100 water, and, by Gilpin's moſt excellent tables †, its ſpecific gravity muſt have been 0.9915. By the ſame tables, theſe ſpecific gravities of 0.842 and 0.9915 would, at 10° Reaumur, or 55° Fahrenheit, have fallen to 0.832 and 0.9905. Here then are two ſpecific gravities of ſpirit correſponding with the degrees 12 and 37, whence the following table is conſtructed.

BAUMÉ's Hydrometer for Spirits.

Temperature 55° Fahrenheit, or 10° Reaumur.

Degrees		Sp. Gravity	Degrees		Sp. Gravity.
10	=	1.000	26	=	.892
11	=	.990	27	=	.886
12	=	.985	28	=	.880
13	=	.977	29	=	.874
14	=	.970	30	=	.867
15	=	.963	31	=	.8~~7~~1 861
16	=	.955	32	=	.856
17	=	.949	33	=	.852
18	=	.942	34	=	.847
19	=	.935	35	=	.842
20	=	.928	36	=	.837
21	=	.922	37	=	.832
22	=	.915	38	=	.827
23	=	.909	39	=	.822
24	=	.903	40	=	.817
25	=	.897			

With regard to the Hydrometer for ſalts, the learned author of the firſt part of the Encyclopédie ‡, M. de Morveau, who by no means conſiders this an accurate inſtrument §, affirms, that the ſixty-ſixth degree correſponds nearly with a ſpecific gravity of 1.848; and as this number lies near the extreme of the ſcale, I ſhall uſe it to deduce the reſt.

BAUMÉ's Hydrometer for Salts.

Temperature 55° Fahrenheit, or 10° Reaumur.

Deg.		Sp. Gr.	Deg.		Sp. Gr.	Deg.		Sp. Gr.	Deg.		Sp. Gr.	Deg.		Sp. Gr.
0	-	1.000	15	-	1.114	30	-	1.261	45	-	1.455	60	-	1.717
3	-	1.020	18	-	1.140	33	-	1.295	48	-	1.500	63	-	1.779
6	-	1.040	21	-	1.170	36	-	1.333	51	-	1.547	66	-	1.848
9	-	1.064	24	-	1.200	39	-	1.373	54	-	1.594	69	-	1.920
12	-	1.089	27	-	1.230	42	-	1.414	57	-	1.659	72	-	2.000

---

* Fifth edition. Paris 1784. † In Philoſoph. Tranſ. 1794. ‡ Tome I. p. 360. à Paris 1796. § Ibid. p. 361.

X. *Obſervations*

X.

Obſervations on the Soap of Wool, and its Uſes in the Arts[*]. *By* J. A. CHAPTAL, *Inſtitutor of the Polytechnic School.*

I HAVE ſhewn the method of forming, at any time or place, and at a ſmall charge, a ſaponaceous liquid adapted to ſupply the place of ſoap for domeſtic purpoſes[†]. On the preſent occaſion I ſhall offer a ſupplement to my former work, by exhibiting, as a ſubſtitute for the ſoft ſoap uſed in fulling almoſt every kind of woollen ſtuff, a ſoap of little expence, which may be eaſily made in every manufactory.

In every manufactory of broad-cloth, and other fabrics of wool, it is uſual to full the ſtuff immediately after it has paſſed the loom. This operation is performed not only for the purpoſe of clearing them of the oil, but to give them the requiſite denſity. For this purpoſe about thirty pounds of ſoap are uſed for every eighty pounds of the ſtuff. This ſoap in the ſouth (of France) coſt twenty livres the hundred weight before the Revolution. It conſumed a large part of our oils, as well as thoſe of Italy, and all the wood-aſhes of the domeſtic fires in the reſpective countries in which it was made.

Hence it is obvious how greatly beneficial it muſt prove to the manufacturer, as well as to commerce in general, to be able to ſubſtitute without difficulty, inſtead of the ſoft ſoap, another compound of materials, eaſy to be procured, and of moderate coſt. In addition to the ſaving in the fabrication of the ſtuff, very great advantage would be derived from the wood-aſhes of our fires being left for domeſtic uſe, or for the ſalt-works, or manufactories of green glaſs; at the ſame time that the oil formerly conſumed would remain to be totally applied to ſuch works as cannot be conducted without it.

In all ages this problem has offered itſelf for ſolution to the Manufacturer and the Government. Fullers'-earth, pure alkalis, and other agents, have been ſucceſſively employed. The firſt of theſe is of inferior quality, either for bleaching or fulling; the ſecond diſſolves the ſtuff. The manufacturers of Lodève ſtill recollect with terror a charlatan ſent to them by Government a few years ago, who pretended to ſubſtitute the mineral alkali in the place of ſoap.

To theſe inconveniencies we muſt add that of not rendering the cloth ſupple, but leaving it in poſſeſſion of that harſhneſs which ſoap alone removes. It is requiſite, therefore, that whatever ſubſtance may be offered as a ſubſtitute for this article ſhould poſſeſs the qualities of cleaning, fulling, and ſoftening, the ſtuff. The compoſition I am about to deſcribe poſſeſſes all theſe advantages. Experiments have been made at my requeſt, at Lodève, by Citizen Michel Fabriguette, who is intimately acquainted with natural philoſophy, and a ſkilful manufacturer of drapery.

The whole operation conſiſts in making an alkaline lixivium of wood-aſhes or pot-aſh, and diſſolving therein, at the boiling heat, old rags[‡], or clippings of wool, to the point of ſaturation. The product is a ſoft ſoap, very ſoluble in water, of a green greyiſh colour, well blended (bien lié), and poſſeſſing an animal ſmell, which the cloths loſe by waſhing and expoſure to the air.

---

[*] Nearly a literal tranſlation from the Annales de Chimie, XXI. 27.

[†] See the Report of Citizens Pelletier, D'Arcet, and Le Lievre, on the fabrication of Soap.

[‡] Old woollen rags are a very cheap article in this country. But, as every other kind of hair muſt certainly anſwer, and horns and hoofs probably will, there muſt be an immenſe and probably cheaper ſource in the refuſe of the tanners, hog-butchers, horners, and comb-cutters. All theſe, at preſent, are uſed only as manure. N.

The various experiments I have made on this ſubject have preſented the following reſults:

1. As ſoon as the wool is plunged in the boiling liquid, the filaments adhere together, and a ſlight agitation is ſufficient to effect the complete ſolution.

2. The lye becomes coloured, and gradually thickens, in proportion as more wool is added.

3. The ſoap is more or leſs coloured accordingly as the wool is leſs or more clean and white.

4. The pile or hairs which are mixed with the wool are more difficult of ſolution.

5. The quantity of wool the alkali is capable of diſſolving, depends upon the ſtrength of the lixivium, its cauſticity, and the degree of heat. Two pounds three ounces and ſix drams of cauſtic alkali, at twelve degrees * of concentration, and at the boiling heat, diſſolved ten ounces four drams of wool. The ſoap, when cooled, weighed one pound four ounces.

An equal quantity of alkali, at the ſame degree of cauſticity, heat and concentration, in which I diſſolved four ounces of wool, did not acquire conſiſtence ſufficient to anſwer ſeveral of the purpoſes required.

An equal quantity of alkali, marking four degrees, diſſolved only two ounces ſeven drams of wool. The ſoap, when cooled, weighed fourteen ounces. It was of a good conſiſtence.

6. In proportion as the wool is diſſolved in the lixivium, the ſolvent power of the alkali decreaſes, and at laſt it takes up no more. It is at this period, namely, when the wool being agitated in the fluid is no longer diſſolved, that the operation muſt be terminated.

I. *The Choice and Preparation of Materials.*

The materials required to form this ſoap are two, alkaline matters and wool.

The alkaline ſubſtances may be obtained from the aſhes of common culinary fires †, and the lye made by the well-known proceſſes. Lime is to be ſlaked with a ſmall quantity of water; the paſte is to be mixed with ſifted wood-aſhes, in the proportion of one-tenth part of quicklime compared with the weight of the aſhes. The mixture is to be put into a ſmall ſtone trough (for wooden veſſels colour the lye, and become ſpeedily uſeleſs); water is to be poured on to the depth of ſeveral inches. After a certain time, the ſolution may be drawn off at an aperture formed in the bottom of the veſſel for that purpoſe. It muſt not be drawn off but at the moment previous to its uſe, and may have the ſtrength from four to fifteen degrees. But, indeed, it is of little conſequence what the ſtrength may be, becauſe the only difference reſulting from the uſe of a weak or a ſtrong lye is, that the quantities of wool which are diſſolved, will differ accordingly.

The potaſh of commerce may be employed in the ſame manner, by mixing one-third of its weight of quicklime.

As to the choice of the wool, every one knows, that in the manufactories of woollen cloths of

---

* Qu. By what meaſure ?—It is greatly to be wiſhed that all meaſures derived from the denſity of fluids were reduced to the common expreſſion of the tables wherein water is taken as unity or 1.000. N.

† Wood being much more uſually burned in France than in England, their common aſhes are what in London are obtained only from the bakers. The uninſtructed workman ſhould be aware that coal-aſhes are unfit for this purpoſe.

every kind, there are a number of operations performed, from the firſt waſhing of the material to the laſt package of the finiſhed article, which occaſion more or leſs of loſs. The water in which the wool is agitated to cleanſe it, the floor on which it is ſpread out, the warehouſe where it is depoſited, all afford waſte wool; as do the operations of beating, carding, ſpinning, weaving, fulling, napping and folding. In all theſe ſeveral manipulations we every where ſee a reſidue of wool, which, it is true, is collected with ſome care; but many of theſe operations are of ſuch a nature, that the remains of wool they afford are ſoiled and mixed with foreign matters, or elſe cut and rendered too ſhort to enter into ſome fabrics; ſo that they are moſtly thrown on the dunghill. This manufacture of ſoap affords the means of converting them all to uſe. Nothing more is required, but to collect them all in thoſe baſkets in which the wool is waſhed, and to waſh them with care, for the purpoſe of ſeparating impurities and foreign ſubſtances; after which, they are to be reſerved for this uſe.

The cuttings of all the woollen ſtuffs, afforded by the ſhops of manufacturers, dealers, taylors, and the like, may be advantageouſly collected for this purpoſe; and the ſame advantage may be derived from the remains of garments after they are worn out.

II. *Method of Making the Soap.*

When the lye and the wool are both ready, it remains only to cauſe the lye to boil in a veſſel of the common form. When it has arrived at this point, the wool is to be added by ſmall quantities at a time, and agitated to cauſe a more ſpeedy ſolution. Care muſt be taken not to add more wool, until the firſt portions are diſſolved. The operation muſt be ſtopped the moment the liquor refuſes to diſſolve more.

From the operations in the large way, made by Michel Fabriguette, with ſoaps of his own fabrication, after the method I communicated to him, it is certain that this ſoap cleans, felts, and ſupples the cloths perfectly well. But its uſe requires a few important obſervations to be made.

1. When the ſoap is not made with the requiſite care, or when dirty or coloured wool has been employed, the fabric receives from the ſoap a grey tinge, which it is very difficult to eradicate. This tinge is of no conſequence when the ſtuff is intended to be dyed; but it would injure the beauty of that white colour which in certain goods is intended to be preſerved. The remedy conſiſts in employing the moſt ſelect materials to form the ſoap intended for ſuch delicate applications.

2. Stuffs fulled with this ſoap contract an animal odour, which, though not very ſtrong, is nevertheleſs diſagreeable; but water and the air completely remove it.

After having ſucceeded in the employ of this ſoap in fulling cloths made of wool, I attempted to ſubſtitute ſoda for potaſh, and to form, according to the proceſs here deſcribed, a ſolid ſoap, proper for the operations of dyeing cottons. My experiments have ſucceeded beyond my hopes.

Forty-ſix pounds of ſoda at eight degrees diſſolved at the temperature of ebullition five pounds of wool *, and afforded, by cooling, ſixteen pounds fourteen ounces of ſoap ſufficiently ſolid not to be ſpread (couler).

---

* It is affirmed, that when common ſea ſalt is thrown into the combination of oil and vegetable alkali in the proceſs of ſoap-making, the effect conſiſts not merely in the ſeparation of the ſoap from the water, now rendered ſalt, but that the alkalis change place; ſo that the ſoap obtains the mineral alkali, and the fluid, inſtead of containing

The firſt wool which is thrown into the ſoda diſſolves readily; but it is afterwards ſeen that the fluid gradually becomes thicker, and that the diſſolution becomes more difficult and ſlow.

The firſt ſolutions render the liquor green; after which it becomes black, and the ſoap, when cooled, preſerves a blackiſh green colour.

This ſoap has been employed in every manner, and under every form, in my manufactory for dyeing cottons; and I am at preſent convinced that it may be ſubſtituted, inſtead of the ſaponaceous liquid we make from the lixivium of ſoda and oil, to prepare (apprêter) the cottons. I have conſtantly obſerved, that by diſſolving a ſufficient quantity of this ſoap in cold water to render the fluid milky, and by working (foulant) the cotton with the apparatus which is well known, it is ſufficient to paſs the cotton three times through, drying it each time, in order that it may be as well diſpoſed to receive the dye as that which has been paſſed ſeven times through the ordinary ſolution of ſoap. This will not appear ſurpriſing when it is conſidered that animal matters are very proper to diſpoſe thread and cotton to receive the dye, and that ſome of the operations of our dye-works conſiſt ſimply in impregnating them with theſe ſubſtances.

It is to be obſerved, that cotton which has paſſed through a ſolution of this ſoap acquires a grey tinge nearly ſimilar to what it gains by aluming, while the common ſoap liquors give it the moſt beautiful white colour. But this grey colour is not at all prejudicial to the dyeing proceſſes, as we have remarked in ſpeaking of woollens.

I muſt remark, in confirmation of this laſt uſe, which I attribute to the ſoap of wool, that after having impregnated cotton with it by the ordinary proceſs, I cauſed it to paſs through all the operations to which wool is ſubjected to produce the ſcarlet dye. The cotton acquired a deep and very agreeable fleſh colour; whereas the cotton which had not received this preparation came out of the bath with its natural colour. This firſt eſſay promiſes advantageous reſults, which I mean to purſue.

It may be of ſome utility to obſerve, that the ſoap of wool may be beneficially ſubſtituted inſtead of common ſoap. In domeſtic operations I have profitably applied it to waſh linen, and particularly woollen garments and other articles. I have no doubt but the facility and economy which it preſents in its fabrication[*] will ſerve to extend its uſe ſtill further; but in the mean time I have thought it proper to ſhew the various objects to which I have applied it.

Obſervation.

As the ſoap of wool gives a grey tinge to piece-goods, which it is difficult to eradicate, it follows, that it cannot be uſed for bleaching linen, unleſs it be made of white wool ſelected and carefully waſhed.

---

ing common ſalt, will be found to contain the combination of marine acid and vegetable alkali. I do not know if this has been ſhown to be really the caſe, nor whether this indirect proceſs be of much value. If it be fully as here ſtated (which I doubt), our ſoaps muſt owe their inferiority to thoſe of Spain to the animal oil they contain, and not to their alkali. N.

[*] The effect of the Exciſe Laws in Britain confines the manufacture of ſoap to premiſes regiſtered in form, and regularly viſited. What the general effects of this arrangement may prove on our great national manufacture of woollens, more eſpecially if the preſent invention ſhould amply come up to the expectations here excited, is a queſtion that well deſerves to be inveſtigated. N.

XI.

Extract of a Memoir concerning three different Species of Carbonated Hydrogenous Gas, obtained from Ether and Alcohol by different Proceſſes, forwarded to the National Inſtitute of France by the Society of Dutch Chemiſts; being Part of a Report read to the Firſt Claſs of the Inſtitute, by Citizen FOURCROY, *at the Sitting of the 26th Frimaire, 16th December* 1796*.

CITIZENS Bondt, Deiman, Van Trooſtwyk and Lauwerenberg, chemiſts, of Amſterdam, who for ſeveral years paſt have made experiments together, and have already rendered great ſervices to the ſciences, ſent on the firſt Fructidor of the fourth year to the Inſtitute, a Memoir addreſſed to that body by C. Van Mons of Bruſſels, its aſſociate. As the ſubject it treats of relates intimately to the progreſs and ſtate of modern chemiſtry, it becomes neceſſary to give an account as ample as the novelty and importance of the ſubject demand.

Several months before the arrival of this Memoir, the Inſtitute had received an account of the diſcovery it announces. The contents of the letters of C. Van Mons, on this diſcovery made in Holland, forwarded to the Inſtitute in Ventoſe the fourth year (March 1796), were as follows:

The olefiant gas, which is ſo called from its characteriſtic property of forming oil in a circumſtance which ſhall be deſcribed, is formed of a mixture of ſeventy-five parts of concentrated ſulphuric acid with twenty-five parts of alcohol, even without the aſſiſtance of foreign heat. It is likewiſe formed by paſſing alcohol or ether in vapour over ſilex or alumine in a tube of glaſs, or ſimply in a tube of pipe earth ignited without addition. It is not formed by the paſſage of the alcoholic or ethereal vapour in a tube of glaſs ignited without ſilex or alumine, nor in the ſame tube containing lime or magneſia. The inflammable gas, which is obtained in this laſt caſe, is no longer ſuſceptible of becoming olefiant, by a ſecond tranſition through ſilex or alumine. The olefiant gas is not abſorbed nor altered by remaining over water; with a ſmall quantity of oxigenated muriatic acid gas it forms an ethereal oil. Mixed with this gas in equal proportions, and ſet on fire, it lets fall a great quantity of carbone; when 0.25, or 0.20, or 0.15 of oxigenated muriatic acid gas is added to 0.75, or 0.80, or 0.85, of olefiant gas, and the mixture ſet on fire, the carbone appears immediately in the form of very fine lamp-black. The greater the proportion of the olefiant gas, the more perceptible is the appearance of carbone during the inflammation. Too large a portion of oxigenated muriatic acid converts it into carbonic acid. This experiment proves that the hydrogene attracts oxigene more ſtrongly than the carbone does. C. Van Mons thinks, in his letters on this ſubject, that the olefiant gas is a true carbonated hydrogenous gas.

Five months after this firſt account, on the firſt of Fructidor, in the fourth year of the Republic of France (18th Auguſt 1796), C. Van Mons forwarded to the Inſtitute the Memoir of Citizens Bondt, Deiman, Van Trooſtwyk and Lauwerenberg, in which theſe chemiſts have with great care deſcribed the properties of this gas, which they had diſcovered, and which was already known by the name of the olefiant gas. This Memoir, which is very well drawn up, conſiſts of twenty-four paragraphs; the ſubſtance of which we ſhall here relate.

In the firſt paragraph the authors obſerve, that the gas which is diſengaged during the mutual action of concentrated ſulphuric acid and alcohol, which was known to occaſion frequent ruptures of the veſſels, to burn with an oily flame, which had cauſed it to be preferred in lamps ſupported by inflammable air, appeared to them deſerving of a particular examination in conſequence of the curious properties it preſented, particularly when compared with the gaſſes afforded by alcohol and ether treated by other methods.

---

* Annales de Chimie, XXI. 48.

Secondly, From having remarked that the gas is diſengaged towards the end of the proceſs for making ether, they took the proportions of the mixture which exiſts at this period of the etherification; that is to ſay, four parts of concentrated ſulphuric acid and one of alcohol, and treated this mixture in a common glaſs body appropriated to the production of aeriform fluids.

The third paragraph deſcribes the ſeries of phenomena which take place during the production of this gas. The mixture heats and becomes brown; gas is extricated without the application of external heat; but when ſuch heat is applied, the effervesſcence greatly increaſes, the colour of the mixture becomes black, the gas paſſes abundantly; it is even neceſſary, to prevent the whole of the liquor from quitting the veſſel, that the heat ſhould be withdrawn. The reſidue, after the extraction of the gas, conſiſts of ſulphureous acid mixed with coal, which renders it black. Citizen Fourcroy remarks, that the deſcription makes no mention of ether being produced.

[*To be concluded in the next Number.*]

MATHEMATICAL CORRESPONDENCE.

AS the preſent Journal is deſigned to embrace every branch of uſeful knowledge, both in ſcience and the arts, a certain portion of it will be devoted to the inſertion of ſuch mathematical diſſertations and queſtions, as by their novelty or importance appear to deſerve the attention of the public. Every communication therefore, of this kind, directed to the Editor, poſt paid, will be thankfully received, and publiſhed as early, in ſome of the ſucceeding numbers, as the plan of the work will admit; but it is particularly requeſted that the contributors of queſtions will direct them, as much as poſſible, to practical or theoretical improvement; as none that are forced, or framed upon obſcure enigmatical principles, which when diſcovered are of no value, will be inſerted.

The following QUESTIONS are propoſed for Solution.

QUESTION I. *By J. B.*

IT is required to divide the half of a given right line into a given number of parts, ſo that each part, and the ſum of that part and the remainder of the whole line may be in geometrical progreſſion; this being a queſtion of practical utility in the diviſion of the monochord or muſical ſtring.

QUESTION II. *By Capt. W. MUDGE.*

IT is required to determine the centrifugal force of a body moving in the circumference of a circle, by the pure principles of fluxions, inſtead of deriving it from the doctrine of indiviſibles, as is done by Newton in the Principia.

SCIENTIFIC NEWS.

THE Public will hear with much pleaſure, that the Journal des Sçavans and the Annales de Chimie are both revived. The former work, as uſual, is anonymous, though the papers are marked by initials or characters. The latter preſents the reſpectable names of its authors, Guyton, Monge, Berthollet, Fourcroy, Adet, Seguin, Vauquelin, Pelletier, C. A. Prieur, Chaptal, and Van Mons.

Of the Journal des Sçavans, four numbers have reached England, commencing 16 Nivoſe, an. 5 (Jan. 5, 1797), continued on the 30th of the ſame trimeſtre, and afterwards publiſhed on the ſame days of each trimeſtre reſpectively. The laſt, 30 Pluvioſe, therefore anſwers to Feb. 18. Regular extracts will in future be given in our Journal.

The firſt number contains an introduction and a general ſketch of the ſtate of letters, ſciences, and arts in Europe, at the commencement of the fifth year of the French Republic (23 Sept. 1796). The latter is drawn up with the hand of a maſter.

Scientific matters contained in the ſecond number are: 1. The public ſitting of the National Inſtitute of the 15th Nivoſe. Citizen Prony announced three aſtronomical memoirs of Citizen Flaugergue, aſſociate; and the continuation of the great work of Citizens Delambre and Mechain on the Meridian from Bayonne to Dunkirk, which will be finiſhed in the year 6. Citizen Lacepede announced the following memoirs:—On the ſulphureous acid, by C. Fourcroy and Vauquelin—Various works of C. Lamarque on the general principles of Chemiſtry, from which a chromometric ſcale is obtained—Defence of the new chemical theory againſt a German, by C. Van Mons, aſſociate—On the ſubſtance of gold, by C. Chaptal, aſſociate—On vegetable juices, by the ſame—On the gluten of wheat, by C. Texier—On the teeth of animals, particularly the horſe, by C. Tenon—On the nature and cauſes of vertigo in horſes, by C. Huzard—Reſearches on the epidemical diſorders of cattle, by the ſame—An elementary table of the hiſtory of animals, by C. Cuvier—On the uſe of mercury in the ſmall-pox, by C. Deſeſſarts.

The travels continued with ſucceſs by various members of the Inſtitute, afforded Citizen Lacepede an opportunity for a well-timed digreſſion in form of an invocation to Peace, which was greatly applauded.

The Citizen Talleyrand, one of the ſecretaries of the ſecond claſs, gave an account of its labours. They conſiſt of two memoirs on Ideology, by C. Tracy, aſſociate—Two memoirs of C. Laromiguiere, aſſociate; one on the operations of the human underſtanding; the other on the ſignification of the word Idea—On the converſion of the territorial impoſt into a duty on ſucceſſions, by C. Duvillard, aſſociate—On public credit, by C. Dyanniere, aſſociate—Inquiries reſpecting the Arabian Gulf, by C. Goſſelin—Concerning the manners of the Greeks in the time of Homer, by C. Leveſque—Three hiſtorical memoirs; one reſpecting the Egyptians, another on the Swiſs, and the third concerning Peru, by C. Anquetil—Notice reſpecting Sylvain Bailly, by C. Deliſle de Sales.

Citizen Seguin, aſſociate, read a memoir on the tanning of ſkins. His proceſs, of which ſome account has already been given*, is ſaid to produce the ſame effect in one month, as by the ancient proceſs required fifteen or eighteen.

---

* Page 26 of this Journal.

Citizen Desfontaines read a memoir on the cultivation of ſpices in Guiana. Three hundred exotic trees were carried thither by C. Martin; and it appeared that the clove, among others, afforded a very conſiderable product.

Citizen Dupont de Namours read a memoir on the ſociability and morality of the dog, the fox, and the wolf. The title excited the public attention; and the effects of the diſcourſe, which was original, philoſophical, and lively, was ſuch that it gained much applauſe.

ACCOUNTS OF FOREIGN PUBLICATIONS.

Beſchreibung einer Reiſe, &c. or Travels in Germany and Switzerland in the Year 1781, with Obſervations on the Sciences, Induſtry, Religion and Manners. By Frederick Nicolai. Vol. XI. and XII. Berlin and Stettin, 1796, in octavo.

Nicolai publiſhed the firſt volumes of his Travels in 1783, at that time offered for ſubſcription, and he has ſince brought them out two volumes at a time. The 9th and 10th appeared in 1795. This work is greatly eſteemed in Germany. The title of Travels, though the author really paſſed through the countries he deſcribes, is, properly ſpeaking, no more than a vehicle for numerous obſervations on manners, population, induſtry, and ſcience. Particular diſſertations on various objects are inſerted. Thus the xith volume contains diſſertations on the Celts, the Suabians, the Celtic language, &c.; and the xiith, obſervations of much importance on certain geographical denominations, which Pliny, Ptolomy, and others, give to places ſituated at the Black Foreſt and in Helvetia. It muſt not be concluded that this account contains little novelty becauſe the Travels publiſhed by the author were made in 1781. To the recital of his actual travels Nicolai has added a work of the cloſet, perhaps much too extended, which conſiſts in a numerous collection of notes and tables relative to ſtatiſtics. Some are dated in the year 1796.

The works of Nicolai are more particularly eſteemed in Germany, in reſpect to the objects of political œconomy they contain.

Ueber die Kultur-verhältniſſe, &c. or An Eſſay on the comparative Culture of Land in the various States of Europe; in which an Attempt is made to determine, by the Magnitude and Population of the ſeveral States, the Degree of Cultivation of the European Dominions. With ſixteen large Tables, exhibiting the Surface and Population of the European States. By Aug. Fred. Guil. Crome. Leipſick, 1792; 1 vol. octavo; 398 pages of text, and 112 of additions.

Statiſtiche Ueberſicht, &c. A Statiſtical View of the Provinces of the Ruſſian Empire, with regard to the comparative Cultivation of the Land. By H. Storch. Riga, 1795; Hartknock; 1 vol. ſmall folio, 131 pages.

Geographiſch-Statiſtiſche Tabellen; Tables of the Statiſtical Geography of Switzerland. By H. K. Zurich, 1795; Ziegler and Sons; 1 vol. ſmall folio; ſeven tables.

Annalen der Staatskräfte von Europa. Annals of the Powers of the ſeveral States of Europe conſidered, with regard to the preſent State of Natural Philoſophy, Commerce, Science, and Political Relation. The whole drawn up in the Form of Tables. By Ad. Frid. Randel. Berlin, 1792; Frid. Vieweg; 1 vol. ſmall folio.

Der Polynomiſche Lehrſatz, &c. Principles of the Doctrine of Polynomials, the moſt intereſting Problem of Analytical Science, with ſome other Theorems relating thereto, explained and developed, by Tetens, Kluegel, Kramp, Pfaff, and Hindenburg. The latter has added an abridged Summary of the Method of Combinations, and their Uſe in analytical Proceſſes. Leipſick, Fleiſcher the younger, 1796, 8vo.

Ueber die Methode des Hrn. La Grange, &c. A Diſſertation on the Method of Mr. La Grange, for reſolving all Equations by means of Series. By F. W. A. Murhard.—Gottingen, Roſenburg, 1796, 4to.—16 pages.

Die neueſten Entdeckungen, &c. The new Diſcoveries in Electricity conſidered with regard to Natural Philoſophy and Medicine, collected by Charles Gottlieb Kuenh. Part I. Leipſick, 1796, 8vo.

Dr. Lorenz Decrell has tranſlated into German the firſt volume which has appeared of Kirwan's improved edition of Mineralogy, with Obſervations. It is printed for Nicolai at Berlin.

Handbuch der neueſten Erdbeſchreibung; or, A Manual of the moſt modern Geography. By Gaſparis, Profeſſor at Jena. Vol. I. 8vo. Weimar, December 1796.

Acta Elect. Mogunt. Scientiarum utilium, quæ Erfurti eſt, ad Annum 1794 & 1795, cum Figuris in 4to maj. Erfort, G. A. Reyſer.

Traité d'Harmonie & de Modulation. Par H. F. M. Langlé. A Treatiſe of Harmony and Modulation. By H. F. M. Langlé, ſome time Firſt Maſter of the Conſervatory of Piety at Naples, and Profeſſor at the Conſervatory at Paris.—This treatiſe is divided into two parts. The firſt exhibits all the practicable concords in harmony, and the ſecond all the poſſible modulations. "The authors have treated this ſecond part," ſay the Journaliſts, "in ſuch a manner as to facilitate to beginners on the piano-forte and the harp the difficult accompliſhment of preludes." Boyer, Paris.

Two Tranſlations of Spalanzani's Travels in the Two Sicilies have appeared; the one at Berne, by Senebier; and the other at Paris by Toſcan and Duval, with Notes by Faujas. Neither of theſe can yet be complete, as Spalanzani's Travels are to form ſix volumes in 8vo.

The Encyclopédie Methodique par ordre de Matiéres, which was interrupted in its progreſs by the French Revolution, is again continued. The ſixtieth and ſixty-firſt livraiſons are publiſhed for Agaſſe, Rue des Poitevins, No. 18. The ſixtieth livraiſon contains three half-volumes:

1. The ſeventeenth Part of the Plates of Natural Hiſtory, forming the ſeventh Century of Botany; by Citizen Lamarck, of the National Inſtitute, Profeſſor and Adminiſtrator of the Muſeum of Natural Hiſtory.

2. The ſecond Part of the third and laſt Volume of Ancient Geography; by Citizen Mentelle, of the National Inſtitute.

3. The Dictionary of Fiſhery; by Citizen Lacombe, Author of ſeveral Dictionaries of the Encyclopédie, particularly that of Arts and Trades. The price of this is thirty-one livres in ſheets, or thirty-three ſtitched. A table is given of the parts already publiſhed, by which it is ſeen that the work is already far advanced, and that there is no probability of its not being completed.

The price of the ſixty-firſt livraiſon is the ſame as the foregoing. It contains the eighteenth part of the plates of Natural Hiſtory, conſiſting of one hundred plates of Inſects.

Vol. IV. Part 1, Dictionary of Botany, by Lamarck.

Vol. VII. Part 2, of the Hiſtory of Inſects.

[*Accounts of Engliſh Publications in the next Number.*]

Dr. Blair's Improvement of compound object Lenses.

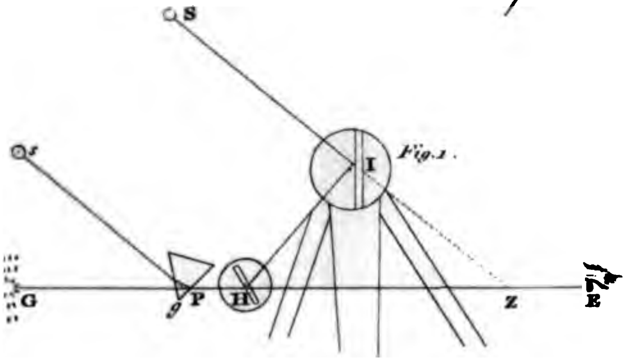


Fig. 1.

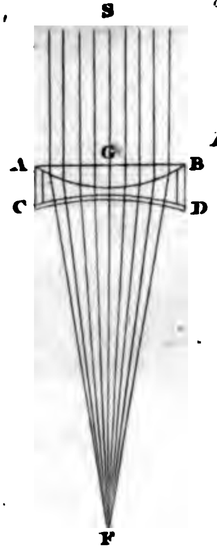


Fig. 2.

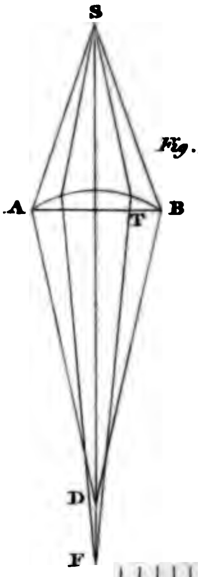


Fig. 3.

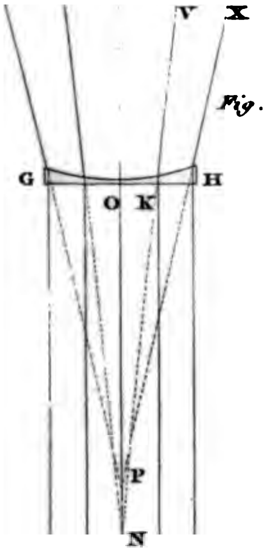


Fig. 4.

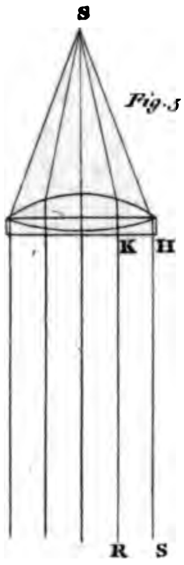


Fig. 5.

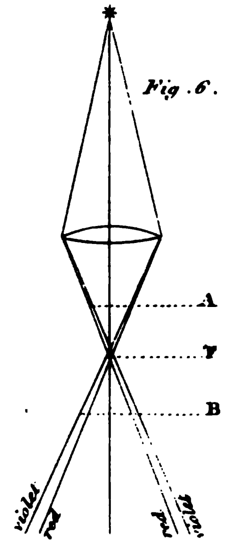


Fig. 6.

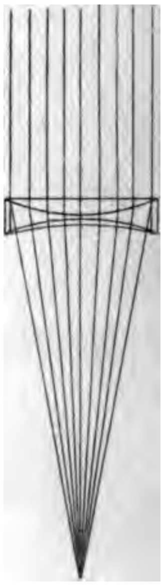


Fig.

Fig. 8.

Electrical Instrument. p. 16.

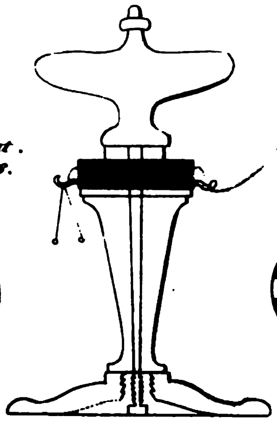


Fig. 9.

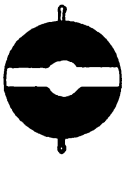
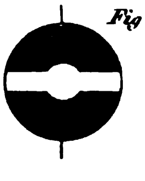


Fig. 10.



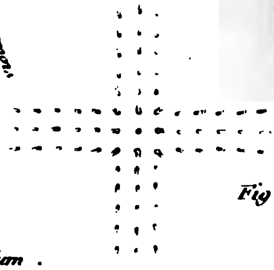


Fig. 11.

Uncommon Solar Spectrum. p. 13.

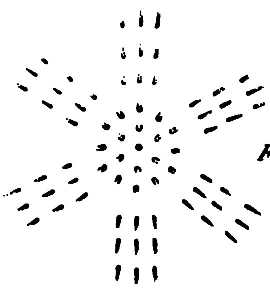


Fig. 12.

Mr. BRAMAH'S PRESS, OPERATING BY THE ACTION OF WATER.

Fig. 1

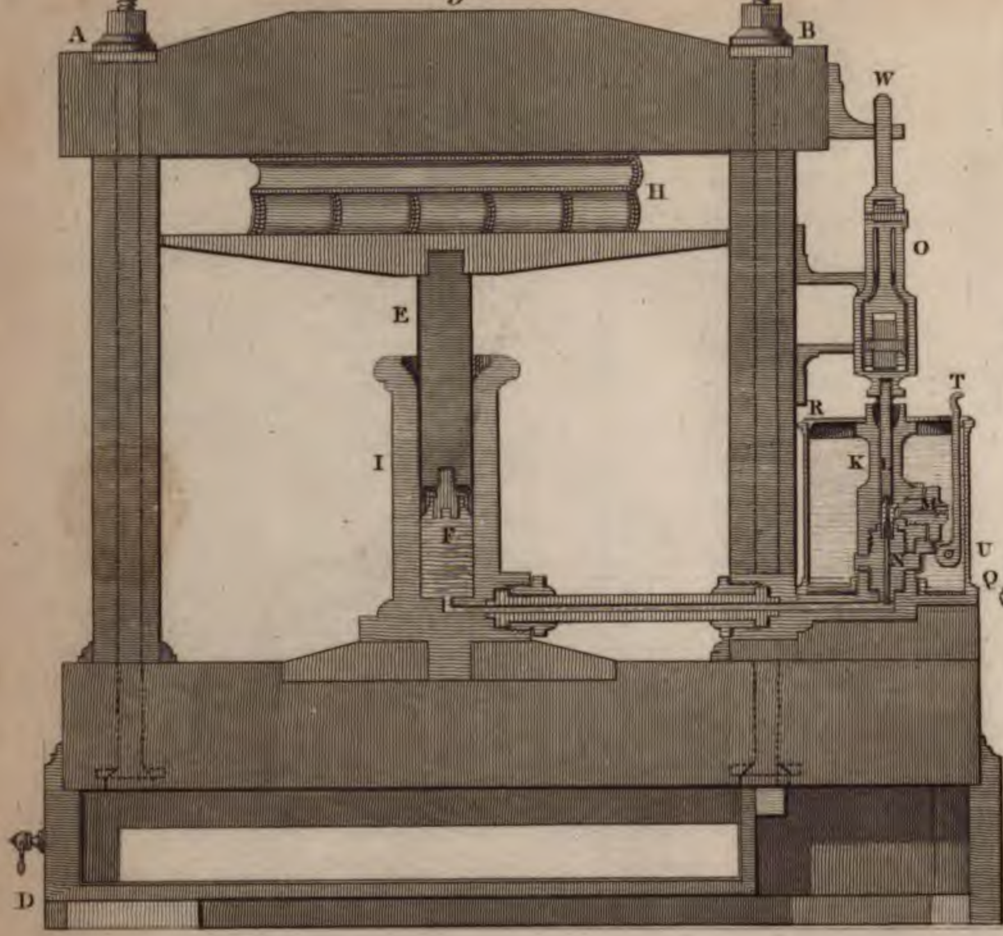
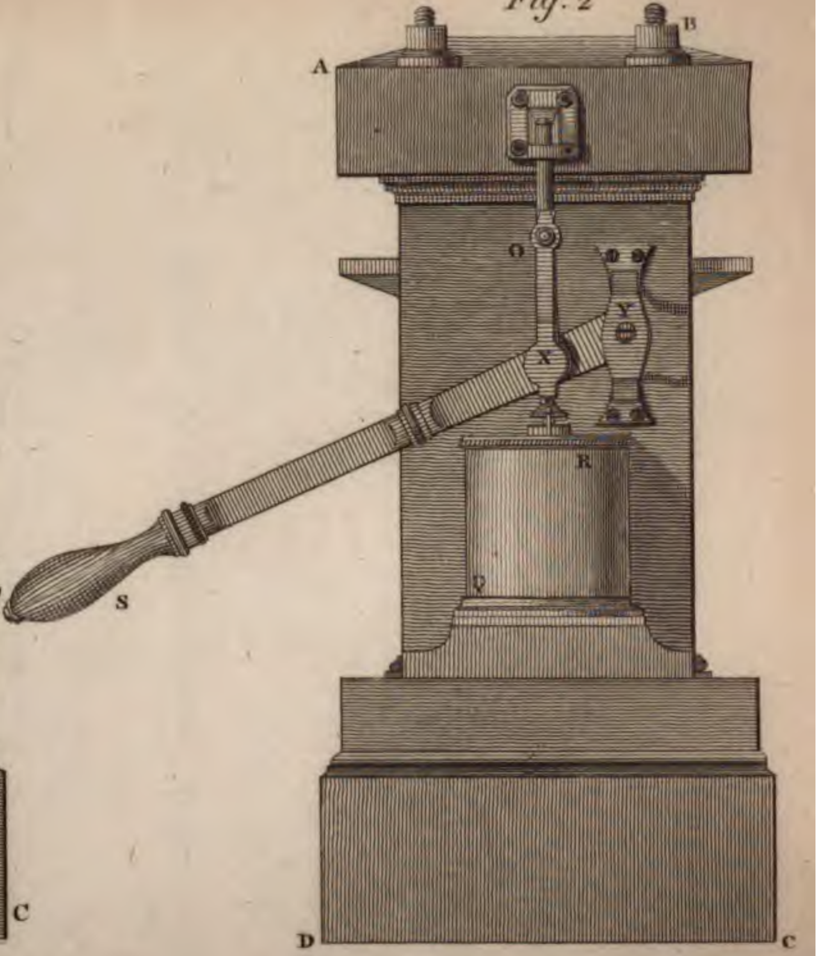


Fig. 2



Scale of Inches.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MAY 1797.

ARTICLE I.

Extract of a Memoir concerning three different Species of Carbonated Hydrogenous Gas, obtained from Ether and Alcohol by different Proceſſes, forwarded to the National Inſtitute of France by the Society of Dutch Chemiſts; being Part of a Report read to the Firſt Claſs of the Inſtitute, by Citizen FOURCROY, *at the Sitting of the 26th Primaire, 16th December* 1796.

[Concluded from the laſt Number.]

THE fourth paragraph treats of the choice and purification of the gas. At the commencement and towards the end of the operation, it is mixed with ſulphureous acid gas; towards the middle it is purer, and contains only one ſixth of ſulphureous acid; when waſhed with water and ammoniac, it is rendered very pure. It does not contain carbonic acid gas.

The fifth paragraph exhibits the phyſical properties of this gas. Its weight compared with that of common air, is as 909 to 1000; its odour is fetid, when well purged from ether and ſulphureous acid. It burns with a ſtrong compact flame ſimilar to that of a reſinous oil.

The ſixth paragraph enumerates ſeveral properties in ſome meaſure negative and characteriſtic of this gas. When left for ſeveral months over water, it ſuffers no alteration. The ſulphuric, ſulphureous, nitric and muriatic acids do not act upon it; neither does the nitrous gas, nor alkalis. Ammoniac in the elaſtic ſtate adds merely to its volume, without occaſioning any change. Phoſphorus heated even to fuſion did not affect it.

In the ſeventh paragraph the authors deſcribe the action of oxigenated muriatic acid gas upon their newly diſcovered gas. As this is the only ſubſtance which acts in any remarkable manner upon it, their deſcription is very accurate and full. They announce with reaſon this effect as no leſs curious than new and hitherto unknown. They at firſt applied the oxigenated muriatic acid, with the intention of proving the preſence of carbone in this gas; becauſe the proceſs had before ſucceeded in ſimilar caſes. Having mixed in a tube over water equal parts of their inflammable gas and oxigenated muriatic gas, an abſorption took place which was more rapid than what obtains between the latter gas and water; a thick oil of a pearl-grey colour more ponderous than water was depoſited. The tube was filled with a white vapour; much caloric was diſengaged, and there remained one eighth part of the gas made uſe of, which was ſtill inflammable. A ſecond mixture of four parts of oxigenated muriatic acid gas, and one part of the inflammable gas produced by alcohol and the ſulphuric acid, and kept over water for eight days, preſented the ſame phænomena as the foregoing. After the ſeparation of the oxigenated muriatic acid gas, there remained only $\frac{1}{10}$th part, which was azotic gas afforded by the oxide of manganeſe.

The eighth paragraph is employed in deſcribing the properties of the oil obtained in the foregoing experiment. When collected in an apparatus which the authors do not deſcribe, it preſented the following characters: Its ſemi-tranſparence reſembled the colour of pearls; it ſunk in water, and became yellow by expoſure to air; its ſmell was agreeable and penetrating, its taſte rather ſweet; both theſe properties were very different from that of ether. It was diſſoluble in water, which acquired its odour; liquid vegetable alkali rendered it ſweeter, by depriving it of the ſmell of oxigenated muriatic acid.

In their enquiries, in the ninth paragraph, concerning the compoſition of this gas from the known properties of ſulphuric acid and alcohol, of which it is formed, the Dutch chemiſts aſſume in the firſt place as a principle that it can contain no other ingredient than hydrogene, carbone, and oxigene. This laſt ſubſtance does not appear to them to be preſent, becauſe it ought either to form water or carbonic acid, neither of which compounds appears*. The gas is not ether diſſolved in ſulphureous acid gas; becauſe after the combuſtion with vital air by means of electricity, as well as after its converſion into oil by the oxigenated muriatic acid, the muriate of barytes does not indicate ſulphuric acid, which ought in that caſe to be formed. Neither does ſulphur enter into its compoſition; for it does not exhibit the ſmell of ſulphurated hydrogenous gas, nor does it throw down ſulphur in proportion as the gas is burned. By this method of excluſion, the learned chemiſts of Amſterdam conclude that their inflammable gas can be compounded of nothing but hydrogene and carbone.

The tenth paragraph is employed in proving the exiſtence of hydrogene in this gas. Though the formation of water and of oil by the oxigenated muriatic acid ſufficiently declares this; yet, as the preſence of the water over which the experiment was made might leave ſome doubt, the Dutch chemiſts had recourſe to other proofs. By paſſing their gas through a tube filled with ſulphur in fuſion, they obtained ſulphurated hydrogenous gas, and the ſulphur was blackened.

In the eleventh paragraph they prove the preſence of carbone in their gas, not only by the black colour of the ſulphur indicated in the foregoing experiments, but by the formation of carbonic acid, which takes place whether the gas be burned with vital air, by means of the electric ſpark, or by paſſing it through an ignited tube filled with the oxide of manganeſe.

---

* To theſe two conditions may be added another, which the learned authors have not directly conſidered, namely, that it may form a triple compound. This, in fact, may happen in the oil; for which vide infrà. N.

A later experiment, which likewiſe proves it, conſiſts in mixing the gas with oxigenated muriatic acid gas, and burning it before their reaction has preſented the oil before mentioned. In this experiment the glaſs veſſel is covered with coal reſembling lamp-black.

The twelfth paragraph announces the name the authors have choſen for their gas, in conſequence of its nature and properties: it is *carbonated oily hydrogenous gas*.

In the thirteenth, they ſpeak of the elaſtic fluid diſengaged from ether treated with the ſulphuric acid. Only three-fourths of this gas is converted into oil by the oxigenated muriatic acid; the reſidue burns blue, and is no longer reducible into oil by this acid. Alcohol and ether paſſing through a tube of pipe-clay, when ignited, afford a gas of this nature.

The fourteenth paragraph announces the formation of an inflammable gas different from the laſt mentioned, or from the olefiant gas, by paſſing ether and alcohol through an ignited glaſs tube. This gas does not afford oil by the oxigenated muriatic acid gas. In order to determine whether the poroſity of the tube of clay was the cauſe of the formation of the oily gas, by ſuffering ſome principles to paſs through, or by admitting others, theſe philoſophers included a tube of clay in another of glaſs, and then paſſed alcohol and ether, after having previouſly ignited the tubes. There was a production of olefiant gas in this caſe, as well as when the glaſs tube contained fragments of pipe-clay. Hence they conclude that the clay contributes to the formation of this gas.

Similar experiments form the object of the three following paragraphs, which Mr. Fourcroy abridges, by merely prefixing the number without any prefatory connection.

15. A tube of glaſs charged with alumine and ignited, through which they paſſed alcohol, afforded an oily gas, the reſidue of which, after the formation of the oil, burned with a blue flame: the ſame thing happened when ſilex was uſed inſtead of alumine. When the tube of glaſs was filled with lime, or magneſia, or vegetable alkali, or charcoal, or ſulphate of pot-aſh, and the alcohol was reſpectively paſſed through theſe in the ignited ſtate, the product was a gas not capable of forming oil; the earths were blackened in theſe experiments. Sulphur, in a trial of the ſame nature, afforded ſulphurated hydrogenous gas not oily.

16. Alcohol and ether muſt neceſſarily paſs, according to theſe authors, over alumine or ſilex, ignited in a tube of glaſs, to afford the olefiant gas. The gas obtained from theſe two fluids by paſſing them through a tube of glaſs does not become olefiant when paſſed a ſecond time over alumine or ſilex, or through the tube of clay. This property, therefore, when once loſt, cannot be reſtored.

17. Carbonated oily hydrogenous gas paſſed through an ignited glaſs tube does not diminiſh its volume, and loſes the property of forming oil with the oxigenated muriatic acid. The tube and the glaſs which have been uſed in this experiment, are blackened and covered with drops of empyreumatic oil; they have the ſmell of this laſt product; a black ſoot covers the water of the apparatus. Six hundred electric ſhocks paſſing through carbonated oily hydrogenous gas increaſed the bulk by two-fifths, and deprived it of its property of forming oil, without having precipitated carbone.

After having thus examined, in the firſt ſeventeen paragraphs, the properties of the carbonated oily hydrogenous gas, the chemiſts of the ſociety of Amſterdam are employed in the five following upon two other kinds of elaſtic fluids obtained from ether and alcohol by different proceſſes.

In the eighteenth paragraph they deſcribe the proceſs by which they have produced theſe laſt gaſes. It is performed by diſtilling ether and alcohol through a tube of ignited glaſs, the extremity of which is received under an inverted glaſs filled with water. The gas formed in this manner precipitates neither oil nor charcoal by the contact of the oxigenated muriatic acid. During its formation, part of the ether or alcohol paſſes unchanged.

The characteriſtic properties of the gas obtained from ether by this proceſs are ſhewn in the nineteenth paragraph. Its weight, compared with that of air, is as 0.709 to 1.000. The ſmell of ether, which is perceptible in the firſt portions, is ſoon ſucceeded by a fetid ſmell. It burns with a compact oily flame, like the former gas; water neither diſſolves nor alters it; it does not render lime-water turbid; and it is unalterable by acids or alkalis. The oxigenated muriatic acid reduces its volume $\frac{1}{7}$th. It burns with a blue flame, and affords no trace of oil.

The reſearches which form the object of the twentieth paragraph ſhew the preſence of, 1. Hydrogene, by the formation of ſulphurated hydrogenous gas, which is produced when it paſſes over fuſed ſulphur. 2. Carbone, by the black precipitate it affords by burning, after mixture with the oxigenated muriatic acid gas, as well as by the carbonic acid which reſults from its combination with vital air. The authors of the memoir, to diſtinguiſh this gas from the former, call it *carbonated hydrogenous gas obtained from ether.*

The gas which is afforded by alcohol diſtilled through an ignited glaſs tube, forms the ſubject of the twenty-firſt paragraph, and differs from the foregoing in two properties:—1. By its ſpecific gravity, which is to that of air as 0.436 to 1.000. 2. By its paler and leſs oily flame, which reſembles that of alcohol. This is named by the Dutch chemiſts, *carbonated hydrogenous gas obtained from alcohol.*

As the analyſis of theſe three ſpecies of gas preſented to their notice the ſame principles in their compoſition, and as they did not appear to them to differ but in the mere proportion of theſe principles, they have in their twenty-ſecond paragraph united the reſults of their experiments in this behalf. Though they were not entirely ſatisfactory, they were nevertheleſs ſufficiently clear to enable them to deduce an uſeful concluſion. In order to arrive at a knowledge of this proportion of the principles in the three kinds of gas they had to compare, they mixed each of them in tubes of glaſs cloſed at one end, or long glaſs veſſels, over mercury, with a larger quantity of oxigenous gas than was ſufficient for their complete combuſtion. Theſe mixtures were ſet on fire by the electric ſpark; the diminution was carefully determined. The proportion of carbonic acid formed was eſtimated from the quantity of precipitate thrown down from lime-water introduced over mercury. The reſult of their trials on this point was, that theſe gaſes contained from 80 to 74 parts of carbone, with from 20 to 26 of hydrogene. In general the three gaſes exhibited little difference between each. The carbonated oily hydrogenous gas preſented the largeſt proportion of carbone; the carbonated hydrogenous gas from ether, a medium proportion of this principle; and the carbonated hydrogenous gas from alcohol, the leaſt of the three.

The twenty-third paragraph of the memoir, abridged by Mr. F. contains a retroſpect of the principal facts exhibited in the whole treatiſe. This retroſpect is ſo well drawn up, and preſents a view of the whole with ſo much accuracy and condenſation, that he has preferred the method of quotation without abridgment.

" Here then," ſay the Dutch chemiſts, " are three kinds of inflammable gas obtained from alcohol and ether treated in different manners.

" Theſe gaſes have this in common, that they are compoſed of hydrogene and carbone. They are ſpecies of carbonated hydrogenous gas.

" It appears probable, moreover, that the proportions of theſe conſtituent parts do not differ, or at leaſt that they differ very little if equal weights of each were examined.

" They differ from each other in ſeveral other reſpects; as their ſpecific gravity, their manner of burning, and the ſeveral methods by which they are produced.

" The moſt remarkable difference is certainly the formation of an oil by the mixture of oxigenated muriatic acid gas with the carbonated oily gas.

" This gas is produced in its greateſt purity in the diſtillation of ether, or of a mixture of alcohol and concentrated ſulphuric acid.

" Ether mixed with the ſame ſulphuric acid alſo affords it, but leſs pure.

" It is alſo obtained by cauſing the vapours of alcohol and ether to paſs through a tube of clay ignited by fire; but this likewiſe is not perfectly pure.

" This effect is equally obſerved by taking the component parts of the tube, namely, alumine and ſilex; which taken ſeparately communicate to the gas this property of forming oil, if the vapour of the alcohol or the ether paſs over theſe ſubſtances.

" Theſe vapours paſſing through a ſimple tube of glaſs ignited, afford the two other ſpecies of gas, accordingly as either ether or alcohol is taken: and theſe two laſt gaſes do not preſent the leaſt appearance of oil, formed by mixing them with the oxigenated muriatic acid gas.

" The gas which has the property of forming oil loſes it by being made to paſs through an ignited glaſs tube. It depoſits charcoal.

" Electric ſhocks have the ſame effect, but without the precipitation of carbone. It is moreover obſerved, that the volume of the gas is augmented when it paſſes from the oily ſtate to that which is incapable of forming oil.

" Theſe ſpecies of gas, laſtly, as well the oily as thoſe obtained from ether or alcohol, are truly permanent gaſeous fluids, and muſt not be regarded either as the vapours of ether preſerving the aeriform ſtate for a time, or as hydrogene gas holding particles of ether or alcohol in ſuſpenſion. We have kept theſe gaſes for whole months over water; we have repeatedly paſſed them through that fluid; and we have expoſed them to re-agents. They have always preſerved their properties without alteration and without loſs."

In the twenty-fourth and laſt paragraph, the Citizens Bondt, Deiman, Paats van Trooſtwyk, and Lauwerenberg, conclude their intereſting reſearches by various queſtions which their experiments have not reſolved. They demand, 1. How the oil is formed by the carbonated oily hydrogenous gas, in its mixture with the oxigenated muriatic acid gas: whether it be by the addition of a portion of oxigene from the latter, or a ſubtraction of part of the hydrogene from the former. 2. What is the nature of this oil, of which hitherto they have not yet procured a ſufficient quantity for a proper examination; but which nevertheleſs appears to them to be a kind of ether. 3. Why is the oily gas formed when ether and alcohol paſs over alumine, ſilex and clay, but not over glaſs. 4. Whether the difference do not ariſe from the circumſtance (ſay they) that the one contains a greater quantity of caloric than the other; and whether it be not thus that the firſt gas loſes its property of affording oil in proportion as its bulk is augmented by electricity, without occaſioning a precipitation of carbone.—That they cannot poſitively reply to theſe queſtions is aſcribed by them to the ſtate of vegetable chemiſtry, which is leſs advanced than that of the air and of minerals.

The experiments made upon the olefiant gas by the citizens Hecht and Vauquelin, in

conſequence

conſequence of the invitation of the Philomathic Society of Paris, throw ſome light on the queſtions of the Dutch chemiſts. The olefiant gas, paſſed through an ignited porcelain tube, afforded hydrogene gas mixed with carbonic acid; a large quantity of carbone was depoſited in the glaſs tube in which the tube of porcelain terminated. The difference remarked in this experiment and that of the Dutch chemiſts is owing probably to the high degree of heat given to the tube of porcelain *. The carbonated hydrogenous gas, deprived of carbonic acid, and afterwards mixed with the oxigenated muriatic acid gas, did not form oil as before. The olefiant gas had depoſited its carbone upon the alumine in paſſing through tubes which contained that earth. The ethereal gas burns with the oxigenated muriatic acid gas, and affords with it the ſame oil as the olefiant gas; which appears to mark a great analogy between them. They may probably differ only in the unequal quantity of combined caloric.

In general it follows, from all theſe comparative experiments, that the moſt denſe olefiant gas, containing leſs of caloric than the carbonated hydrogenous gaſes afforded by alcohol and ether paſſed through tubes of glaſs, poſſeſſes the diſpoſition to form oil only by virtue of the proximity of its conſtituent molecules; and that it loſes this property only when they become ſeparated more apart by the intromiſſion of a greater quantity of caloric between them.

It follows alſo, that the olefiant gas is converted into ſimple carbonated hydrogenous gas, whenever a proceſs is executed which increaſes the diſtance between its component parts. In this way, the attraction between the particles of the hydrogene and carbone is diminiſhed, upon ſeparating them by virtue of a greater proportion of caloric introduced between them. This happens whenever the olefiant gas is made to paſs through ignited tubes, of what nature ſoever; or by ſtrongly electrifying it. The ſame experiments prove alſo, that the difference obſerved, and ſo accurately noted, by the Dutch chemiſts, between the three kinds of carbonated hydrogenous gas, which they diſtinguiſh by the names of *oily*, *obtained from ether*, and *obtained from alcohol*, though each may be obtained indiſcriminately either from alcohol or ether, ariſes only from the methods of treating theſe inflammable fluids, and is conſtantly reducible to this; that in forming the olefiant gas a leſs quantity of caloric enters the maſs, and a compound is determined, in which the hydrogene and carbone are more concentrated, and more diſpoſed to form oil by the addition of oxigene, more condenſed itſelf than the ſtate of vital air as it exiſts in the oxigenated muriatic acid gas; whereas, by more ſtrongly heating the alcohol or the ether, by accumulating more caloric in their vapour, theſe fluids become more completely decompoſed; their elements more widely ſeparated; and their ſimultaneous attraction, which might be adapted to form oil with oxigene, is ſo far diminiſhed, that they are no longer capable of that tranſition. Hence we may conceive, how from the ſpecific gravity of 0.909, which diſtinguiſhes the olefiant gas, it becomes, at the ſame time that it loſes this property, ſo light as 0.436.

We † are not of opinion with the Dutch chemiſts, from the alumen, ſilex, and tube of clay having ſerved to form the olefiant gas, while glaſs, lime, and magneſia afforded only the carbonated hydrogenous gas, not olefiant, that the former of theſe bodies poſſeſs a peculiar tendency to produce the olefiant gas, which property is wanting in the lime and magneſia. Theſe different effects certainly ariſe from a leſs degree of heat having been applied in all the caſes where the olefiant gas was produced, and a greater in all thoſe which preſented the ſimple carbonated hydrogenous gas.

---

* Or perhaps to the difference between the tubes made uſe of. In the porcelain tube (like that of glaſs) the ſemi-vitrification might cover the ſilex and alumine, or change its qualities in other reſpects. N.

† Either M. Fourcroy or MM. Hecht and Vauquelin. But this department of reſearch certainly demands experiment. Is it probable that any conſiderable difference of temperature took place when clay and lime were reſpectively ignited in the glaſs tube? It would be eaſy to prove this by giving the higher temperature deciſively to the clay. N.

But the circumſtance which is truly important for the general theory of ſcience in theſe experiments, is the new light they throw upon the formation of oil; and the force they give to the notions already received and the conſiderations long ago preſented in the pneumatic doctrine, reſpecting the nature and compoſition of vegetable oily bodies. We here ſee that an oil is a compound of hydrogene, carbone, and a ſmall portion of oxigene. We find that, in the mixture of the olefiant gas, or denſe carbonated hydrogene, with the oxigenated muriatic acid, an oil is immediately formed in the ſame manner as when wood, or any other vegetable matter, not in itſelf oily, is heated in a retort. It appears, that the olefiant gas, or a gas very ſimilar to it, is diſengaged from wood, or any ſolid vegetable not of an oily nature, which is ſlowly burned. This gas, when burned in the open air, depoſits its ſoot or carbone, which blackens the wood, and clogs the channels through which the elaſtic fluid muſt paſs. If wood, mucilage, &c. be more ſuddenly and ſtrongly heated in cloſe veſſels, they afford leſs oil, more coal, and more carbonic acid; and in the open air there is more flame, and leſs coal or ſoot depoſited. It is probable that, in the firſt caſe, the olefiant gas is diſengaged, and in the ſecond, the ſimple carbonated hydrogenous gas[*]. We are therefore in progreſs towards the artificial compoſition of oil. Nothing more ſeems to be required for its artificial production, than to obtain, without vegetable matter, a denſe carbonated hydrogenous gas weighing 0.909, and to mix it with the oxigenated muriatic acid gas: and we may hope to ſucceed in the production of this olefiant gas with mineral matters containing much carbone, ſuch as certain kinds of ſteel, which when diſſolved in an acid, by the decompoſition of water they promote, afford a diſengagement of carbonated hydrogenous gas. This laſt object deſerves all the attention of chemiſts, becauſe it opens a new path to the knowledge of vegetables, and particularly of the formation of oil.

The ingenious experiments of the Dutch chemiſts are among the few which afford new proſpects to philoſophers. Together with the diſcoveries for which we are already indebted to them, on the decompoſition and recompoſition of water by electricity, on the alkaline and metallic ſulphures, &c. they will poſſeſs a diſtinguiſhed rank in the pneumatic chemiſtry, to the progreſs of which they have dedicated the reputation of their labours and their diſcoveries.

---

[*] This excellent courſe of experiments, and the general inductions founded upon them, bring to my recollection ſome obſervations I had occaſion to make many years ago, when alcohol was burned in the lamp of Argand. In this experiment, the ſtrata of ignited matter forming the interior and exterior flames (which have not yet been accounted for) are ſtrikingly diſtinct. I have ſuppoſed them to be referable to a general law of the combuſtion of volatile matters at certain limits of temperature; as is moſt eminently ſeen in phoſphorus and ſulphur, both of which have the two kinds of flame in ſucceſſion, and both begin their ſlow combuſtion at very moderate temperatures. But the fact immediately referable to the preſent theory is this: When the lower aperture or air paſſage of the lamp is left open, the flame of the alcohol is as uſual faint and blueiſh, reſembling that of carbonated hydrogene; but if the paſſage be gradually obſtructed by ſlowly applying the palm of the hand, or any more ſuitable obſtacle, the flame becomes more and more luminous, like that of oil, until, in the progreſs of obſtruction, the aperture is ſo much cloſed that the combuſtion begins to decay for want of air. By a proper adjuſtment of the aperture, the alcohol may be made to burn with a conſtantly luminous white internal flame. In this caſe it may be ſuppoſed that the temperature is preciſely ſuch as to extricate the olefiant gas. N.

II.

On the Methods of obviating the Effects of Heat and Cold in Time-Pieces.

IN every mechanical inſtrument for meaſuring the flow of time, ſome natural power is applied to produce motion; and the portions of this motion being uſually regiſtered by an index, are taken to denote certain correſpondent portions of time. The ſimpleſt inſtruments of this nature are the hour-glaſs, which is ſtill in uſe; and certain clocks, regulated by the flow of ſand or water, upon the ſame principle as the hour-glaſs; of which a conſiderable number are deſcribed in Ozanam's Mathematical Recreations. Of theſe it is unneceſſary to ſay more in this place, than that the paſſage of ſand through an aperture is far from being uniform, from a variety of obvious cauſes; and that the flow of water not only varies from circumſtances capable of hydroſtatical eſtimation, but alſo from its fluidity, which theſe very inſtruments have ſhewn to be greater or leſs in proportion to its heat. The clock and watch, which conſiſt of an aſſemblage of wheels acted upon by a weight or ſpring, and regulated by a pendulum or balance, are ſo much ſuperior in their performance to every ancient inſtrument applied to this purpoſe, that the attention of men of ſcience has long been confined excluſively to theſe inſtruments.

When a larger wheel drives a ſmaller, the number of revolutions performed by the latter will be more numerous in a given time than thoſe of the driving wheel in the inverted proportion of their diameters, or the numbers of their teeth. Hence it may eaſily be deduced, from the infinity of aſſumable ratios, that combinations or trains of wheels may be made ſo that the ſlow deſcent of a weight, or other equivalent agent, may cauſe certain wheels in the ſyſtem to revolve with any determinate velocity, provided the rate or velocity of the firſt mover be determined. It is an object of mechanical ſkill to adjuſt a train of wheels, applied to the ſolution of a problem of this nature, in the beſt manner poſſible. For the frictions will increaſe the more complex the train, the lower the numbers of the teeth, and the larger the pivots. Theſe are not, however, the objects of the preſent diſſertation.

When a train of wheels is acted upon by a weight, this laſt, inſtead of deſcending with the accelerated velocity produced by the force of gravitation, is continually checked by a reſiſtance from the train, chiefly ariſing from the ſum of the frictions of the parts againſt each other, and in a certain degree from the reſiſtance of the air, which againſt the quicker moving parts is conſiderable. Theſe retarding cauſes ſpeedily increaſe, until they counterbalance the whole force of gravitation; when the weight of courſe being deprived of the cauſe of acceleration, deſcends uniformly. Here then we obſerve an uniform motion, produced ſimply by the application of mechaniſm, to impede the free deſcent of a weight: but a mere train of wheels, when acted upon by a large weight, ſuffers it very ſoon to run through any moderate height; and if the weight be diminiſhed, the reſiſtances from friction and imperfection of work being far from uniform, and occurring at certain periods of the rotations, are liable to ſtop the machine altogether. For this reaſon it has been found expedient to increaſe that part of the reſiſtance which ariſes from the air, by the addition of a fly at the end of the train. The common jack for roaſting meat, which is to be ſeen in every kitchen, is an engine of this kind, in which the weight deſcends very ſlowly and uniformly, becauſe reſiſted by the friction of a ſcrew in the laſt arbor of the train, and a fly which terminates the whole. The late Mr. John Whitehurſt, F. R. S. conſtructed a clock on this principle for meaſuring ſhort portions of time, to the hundredth part of a ſecond, in which the regulating agent was a broad-leafed fly, which acted upon the air. It was regulated in the firſt place by adjuſting the weight, and in the next place by altering the obliquity of the ſtriking ſurface of the fly. It may readily be imagined, as was in fact the caſe, that its rate would vary according to the denſity of the air. Perhaps it might have been of uſe in ſome experiments to aſcertain that varying reſiſtance. Huygens, in his treatiſe De Horologio Oſcillatorio, deſcribes a clock regulated by a fly, which derived its reſiſtance from the gravitating power of a ball at the end of a ſuſpenſion by ſtrings. When the maintaining power was weak, it ſwung out to a ſmall diſtance from a vertical axis, to the upper part of which the ſtrings were faſtened, and was therefore eaſily carried round; but when a greater force was applied, the ball ſwung out farther, and conſequently afforded a greater reſiſtance. The ſtrings were made to apply againſt a curve, by virtue of which the divergencies of the ball produced iſochronal rotations.

It is probable that the firſt wheel clocks were regulated by a fly; an organ, of which the inconveniencies are too great to require enumeration to ſuch as are moderately ſkilled in purſuits of this nature. Striking and repeating trains are ſtill regulated in this way. M. Le Roy, in the Encyclopédie *, article *Balancier*, affirms, that before the admirable invention of the pendulum by Huygens, clocks were regulated by an horizontal balance, having a vertical axis, that paſſed through two holes, with liberty to play up and down; and that it was ſuſpended by means of a ſtring paſſed through a hole in the axis, and faſtened at both ends, ſo as to form equal angles with the axis itſelf. Conſequently, when the balance revolved in one direction, the ſtring was wound upon the verge, and, being thus ſhortened, raiſed it up until the weight of the balance had overcome the force of rotation: after which it revolved the contrary way, and deſcended to perform a ſimilar aſcent by winding the ſtring the oppoſite way. He does not tell us the nature of the connection between this balance and the wheels which were regulated by it.

The application of the pendulum to clock-work conſtituted ſo important an improvement in engines for meaſuring time, that it will probably be very long before our cotemporaries, or even their deſcendants, will be able to bring the balance in competition with it. In this organ, the regulating force is ſo ſteady, and the loſs from the reſiſtance of the air ſo ſmall, that the imperfections of the train through which the force of the firſt mover muſt be conveyed, are capable of producing no more than a ſmall part of that irregularity which neceſſarily follows when the actual regulator derives moſt of its effect from the train itſelf.

From the contemplation of this moſt valuable meaſure of time, and the ſources of its regularity, the artiſts of modern times have endeavoured to place it in a ſituation which ſhall as nearly as poſſible reſemble that of a pendulum detached from a train of wheels, and vibrating in vacuo without friction or reſiſtance. This ſimilarity is aimed at by a ſkilful diſpoſition of that part of the engine which connects the pendulum with the train, and is called the eſcapement, for which there are many contrivances well entitled to the attention of artiſts and men of ſcience. Another moſt eſſential particular is, that the organ for meaſuring time, whether it be a balance or a pendulum, ſhould invariably preſerve its dimenſions

---

* Geneva edition, 1778, vol. iv.

dimenſions with regard to the centre of oſcillation. But heat and cold alter the dimenſions of all the ſubſtances we know, though not equally in each. The remedy is derived from this inequality, and conſtitutes the ſubject of the preſent memoir.

Since the dimenſions of all bodies increaſe with their temperature, it naturally follows that the pendulums of clocks are longer in hot weather than in cold; and will therefore make their vibrations in longer times. As far, therefore, as this cauſe of irregularity extends, every time-piece, regulated by a pendulum or balance, will go ſlower in ſummer and faſter in winter. In the pendulum for ſeconds, the quantity required to be added to, or ſubtracted from, the length, to cauſe a gain or loſs of one minute per day, is 0.0545 inch at a medium, which does not differ from either extreme more than the half of unity in the laſt figure. Hence the mean variation of length anſwering to one ſecond is 0.000908 inch. And by General Roy's experiments, in the ſeventy-fifth volume of the Philoſophical Tranſactions, the expanſions of five feet of the undermentioned ſubſtances, by a variation of temperature of 180 degrees of Fahrenheit, namely from 32° to 212° are,

	Inch.
Rod of Engliſh plate braſs	0.113568
Rod of ſteel	0.068684
Priſm of caſt-iron	0.066563
Glaſs tube	0.046569
Solid glaſs rod made long before	0.048472

Hence it is ſeen that, in a clock with a ſimple pendulum, having the rod of ſteel, every difference of temperature of four degrees will cauſe a variation of one ſecond per day.

That the variation with a braſs rod is nearly twice as much.

And with a glaſs rod, not much more than half the quantity.

The difference between the mean heights of the thermometer within, at the Royal Society's apartments, in ſummer and winter, is about 25°. Hence the difference of rate between the going of a clock with a ſimple pendulum in ſummer and in winter, will be about ſix ſeconds per day, or one minute in ten days.

The difference of length in ſtraight grained deal wood is found to be very ſmall; whence it is a general opinion, that in clocks of the common conſtructions, in which the pendulum is without intermiſſion connected with the train, and the parts of the eſcapement are oiled, the errors from expanſion in ſuch a pendulum are much leſs than thoſe which ariſe from the unequal tranſmiſſion of the maintaining power.

But the beſt method, in practice as well as in theory, for correcting the effects of heat and cold in pendulums, conſiſts in oppoſing the expanſions and contractions of different kinds of metal to each other. The celebrated George Graham was, as far as I am informed, the firſt who applied this expedient to clocks. His account is in ſome of the earlier volumes of the Philoſophical Tranſactions, which I have not at hand, and therefore make reference to it from memory. Having aſcertained that the expanſion of mercury greatly exceeds that of iron, he formed a pendulum conſiſting of an iron tube filled with mercury to a certain height. In this compound pendulum it is clear that, if the whole had been iron, the centre of oſcillation would have deſcended by heat; and it is equally clear that, if the iron tube could be deprived of all expanſibility whatever, the mercury ſupported beneath would be lengthened upwards by increaſe of temperature; and in this aſſumed caſe, the centre of oſcillation would aſcend by heat. But, as the expanſion of mercury is about fifteen times greater than that of iron, if the tube had been nearly filled, the aſcent of the mercurial column would be only one-fifteenth part leſs than if the iron had not expanded at all. When the mercurial column is ſhortened by pouring out ſome of the fluid, the effect of its expanſion will alſo be diminiſhed. There is, conſequently, a ſtate in which theſe two contrary expanſions ſhall counteract each other, and the centre of oſcillation continue invariable. When a clock thus fitted up gains by heat, the quantity of mercury muſt be diminiſhed; and the contrary if it loſes. I have ſeen an improvement of this pendulum by Mr. Troughton of Fleet-ſtreet, in which a bulb and tube of glaſs, reſembling the common thermometer, was ſubſtituted inſtead of the ſimple tube of Graham. The variable ſurface of the mercury was brought into that part of the rod where its changes of elevation were beſt calculated to counteract the oppoſite expanſion of the whole apparatus.

John Harriſon is admitted to be the firſt who applied the oppoſite expanſions of braſs and ſteel to correct each other, in the apparatus called the gridiron pendulum *. Since the increaſe by expanſion in braſs and ſteel is as 113 to 68, and the expanſion in any piece of metal is proportionate to its length, it will follow that theſe increments will be equal in two rods of theſe metals reſpectively, when their lengths are inverſely as thoſe numbers. That is to ſay, if a rod of braſs 68 inches long be laid upon a rod of ſteel 113 inches long, ſo that one end of each may coincide, and theſe ends be faſtened together; then, if the oppoſite end of the ſteel be fixed to ſome invariable ſupport, the unconfined extremity of the braſs rod will alſo keep its poſition invariably in all temperatures. For the expanſion of the ſteel will in every caſe carry the whole bar of braſs as much in one direction as the expanſion of this laſt bar would have carried its inner extremity in the oppoſite direction. We may imagine the ball of a pendulum to be fixed at this invariable point, and the centre of ſuſpenſion to be at the ſingle end of the ſteel bar.

But it is inconvenient to have nearly half the pendulum rod beneath the ball; for which reaſon Mr. Harriſon cut the ſteel bar into three parts, and the braſs into two. The firſt ſteel bar hung downwards from the centre of ſuſpenſion. To its lower extremity was attached the firſt braſs bar leading upwards. To the upper extremity of this was attached the ſecond ſteel bar, which in like manner ſupported by its lower extremity the ſecond braſs bar leading upwards. A double combination, of the nature of the ſimple pair of bars firſt deſcribed, was thus formed, in which the predominant expanſion was upwards, becauſe the braſs and ſteel were nearly equal in length. But the addition of the third ſteel bar leading downwards from the upper end of the ſecond braſs bar, anſwered the purpoſe of making the deſired compenſation. To its lower extremity then the ball of the pendulum was attached.

Pendulums of this conſtruction have been made, though it is certain that ſuch a combination of bars, inſtead of remaining parallel, would tend, by their own weight and that of the ball, to open like the legs of a pair of compaſſes, and that the remedy of bracing to keep them together would certainly impede their regular action. Mr. Harriſon had recourſe to another expedient. Inſtead of a ſingle ſteel bar, in the firſt inſtance, he made uſe of

---

* Elements of Clock and Watch Work, by Alexander Cumming, London, 1766, page 92.

two, connected at ſome diſtance aſunder by a croſs piece above and below, at their extremities, forming a parallelogram. Within and nearly contiguous to theſe were placed two braſs bars, pinned to the lower croſs piece, and proceeding upwards to another croſs piece which connected their ſuperior extremities. From this laſt piece hung a ſecond pair of ſteel bars, within and nearly contiguous to thoſe of braſs. This laſt pair was alſo connected below by a ſeparate croſs piece, like the firſt, from which a ſecond pair of braſs bars proceeded upwards, as before, and were connected at top, like the former pair of the ſame metal. And laſtly, from the upper connecting piece of theſe braſs bars deſcended one ſingle bar of ſteel, which paſſed clear through the lower framing, and ſerved to ſuſpend the lens or ball. In this ingenious contrivance it is evident that the bars of the ſame metal, which were uſed in pairs, performed the office of ſingle rods, excepting that they mutually ſupported each other, ſo as to need no bracing. It is likewiſe clear that, ſince the pairs of braſs and ſteel bars compared with each other are nearly equal in length, the repetition muſt be more frequent, in order to counteract the expanſion of the ſingle ſteel bar in the middle, in proportion as the difference of their expanſions is leſs. Thus Harriſon and the earlier artiſts cut the ſteel bar into three parts, and the braſs into two, which, with the doubling of the three outer bars, formed a gridiron of ſeven bars. But at preſent, by ſubſtituting a mixture of zinc and ſilver in the place of the braſs, the expanſion of the compound rod is found to be ſo great, that two pieces of ſteel and one of the zinc and ſilver may be conveniently uſed, which contrivance affords a compound bar of five pieces only. This is the form generally uſed.

The gridiron pendulum deſervedly poſſeſſes a higher degree of reputation with aſtronomers than any other contrivance upon the ſame principle. But a conſiderable number of pendulums has been made upon another conſtruction, known by the name of its inventor, Mr. John Ellicott. This artiſt formed his pendulum-rod of iron; but inſtead of attaching the ball to this rod, he cauſed it to reſt upon the outer arms of two ſhort levers, moveable upon centre-pins fixed in the iron bar. The inner ends of theſe levers were acted upon by the lower extremity of a bar of braſs of the ſame figure as the pendulum-rod againſt which it was applied, and to which its upper extremity was attached. From this arrangement it followed that, upon an increaſe of temperature, the braſs bar, being lengthened more than the iron, would cauſe the inner tails of the levers to deſcend more than the fulcrum-pins, and conſequently would raiſe the outer tails, and along with them the ball. By the ingenious application of ſcrews to adjuſt the bearing parts of the ball upon the levers, the quantity of aſcent in the ball itſelf was ſo regulated as to counteract the effect of lengthening in the iron bar, which is the true pendulum-rod.

The objections made by curious reaſoners to this contrivance are, that the levers may be prevented by friction from obeying minute alterations of temperature; and that the two bars may be bended during the increaſe of temperature. Mr. Cumming, in his work before quoted, has conſiderably improved this invention, though, after all, it is liable to more objections than the gridiron. Its chief merit appears to conſiſt in the facility with which it may be adjuſted to temperature, with little or no alteration of the adjuſtment for time.

Several writers on this ſubject have ſtrongly inſiſted, that every compenſation for temperature, whether in a balance or pendulum, ought to move through the air along with it, in order that the ſeveral parts may be heated or cooled together. To this it has been anſwered, that, from the neceſſary circulation of heated air, through its difference of denſity, and the ſpeed with which metals conduct heat, it is not probable that any appreciable difference of effect can ariſe from the pendulum itſelf becoming heated or cooled more ſpeedily than any fixed part of the apparatus which may be applied to obviate its change of figure; and ſtill more it is remarked, that the ſmall variations would correct each other in their return. Theſe arguments appear to poſſeſs conſiderable practical force in favour of fixed compenſations, which in ſome inſtances are ſimpler and cheaper than ſuch as move with the pendulum.

The ordinary principle of theſe combinations is nevertheleſs ſuch as to imply the uſe of a lever. If we ſuppoſe an inflexible bar of braſs, ſtanding upright upon a ſteady ſupport, to afford the fulcrum to an horizontal lever extending each way; and that a vertical bar of ſteel be attached to one extremity of the lever, and faſtened beneath to the ſame ſupport, it is certain that the other extremity of the lever will aſcend by heat, and deſcend by cold, accordingly as the fulcrum riſes and falls through a greater ſpace than the extremity of the ſteel bar attached to the oppoſite end of the lever. If the pendulum, therefore, be ſuſpended by a flexible piece of ſpring to that end of the lever which is fartheſt from the ſteel bar, and the ſpring be ſlightly confined by a notch in a metallic cock, as uſual, the aſcent and deſcent of the whole pendulum, by the operation of this contrivance, will counteract the deſcent and aſcent of the ball by heat and cold. In order that it may accurately do this, it is requiſite that the proportion of the arms of the lever ſhould be variable by adjuſtment.

The direct oppoſition of the expanſion of metals is certainly the moſt ſimple and effectual; and that by levers is evidently leſs ſo, though capable of being magnified at pleaſure. This direct action has been applied in watches to carry a curb or piece of metal which ſerved to lengthen or ſhorten the effective part of the pendulum-ſpring, accordingly as the difference of temperature, by altering the diameter of the balance, might render it more or leſs eaſy to be moved with a determinate velocity. But I have not heard of any ſimple contrivance of this nature having been applied to the balance itſelf. It might not be difficult to conſtruct a balance upon the principle of the gridiron pendulum; but the difficulties of adjuſtment for temperature, poſition, and rate would either prove exceedingly great, or call for expedients, hitherto unthought of, to ſimplify the proceſſes. A very curious contrivance is at preſent uſed in our beſt portable time-pieces, which differs very much from thoſe before deſcribed. I do not know that it has ever been ſcientifically treated, nor even who was the artiſt that firſt applied it to watch-work. One artiſt in a looſe uncertain manner informed me, that he underſtood the contrivance to belong to M. Berthoud at Paris; another poſitively aſſured me, that the well-known Pinchbeck in Cockſpur-ſtreet exhibited a metallic ſolid thermometer on this principle many years ago. Cumming appears to have known nothing of it in 1766, when he publiſhed his Elements; neither do I find it in the French Encyclopédie publiſhed in 1779. I ſhould be happy to do juſtice to the inventors by the aſſiſtance of communications from correſpondents. The principle of this contrivance is, ſimply, that if the faces of two ſtrait bars of metal, differing in their expanſibility, be faſtened together by riveting or ſoldering, they will continue ſtrait only ſo long as the original temperature remains; but if the temperature be augmented, the moſt expanſible metal will become the longer, and a flexure will take place, ſo as to produce convexity on that ſide and concavity on the other. But if the temperature be diminiſhed, the moſt variable of the two metals, by contracting more than the other, will form a concavity on its ſide.

It might afford data for intereſting mathematical ſpeculation to aſcertain what would be the nature of the curves formed by ſuch a combination of materials in all the aſſumable varieties of figure, tenacity, flexibility, and temperature. The conſiderations which preſent themſelves in a looſe proſpect of the ſubject may be exemplified in two uniform bars of braſs and ſteel, differing only in thickneſs, meaſured from the common face of contact.

Metals are capable of being lengthened or ſhortened by mechanical force, as well as by altering their temperature. Hence, if the temperature of a piece of metal be increaſed at the ſame time that its expanſion is prevented, the conſequence muſt be a contraction or change in the texture, of the nature of what workmen call hammer hardening. And the contrary effect may be ſuppoſed to take place if a mechanical obſtacle be oppoſed to prevent the contraction which would take place by cold. It is known from experiment, that the dilatations and contractions from change of temperature are made with an extremely great force; but from other experiments it is alſo known, that the tenacities of bodies are ſufficient to oppoſe and greatly diminiſh the effect of thoſe forces.

If a thin bar of ſteel be ſoldered to a very thick bar of braſs, it may be inferred that the greater expanſions and contractions of the braſs will alternately rarefy and condenſe the adherent ſteel, at the ſame time that the face of the braſs will probably undergo an oppoſite change of the ſame nature, in the vicinity of the ſteel. It may be imagined that the immediate properties of the metals would in the courſe of time become changed by this continual effort; that is to ſay, they might become more or leſs flexible, tenacious, hard, expanſible, denſe, &c. Thus bell-wires become hard by pulling; gold, in the hands of the gold-beaters, becomes ſoft by bending backward and forward; brittleneſs is produced by twiſting, and ſo forth. The power of the braſs to alter the texture of the ſteel muſt be derived from two of its qualities; namely, its reſiſtance to flexure, and its ſtrength or reſiſtance to change, of the ſame kind which was inferred to take place chiefly in the ſmaller bar. Whenever, therefore, it is required that the expanſions and contractions from temperature ſhould be exerted chiefly in bending the compound bar, it is neceſſary that the ſtrengths and flexibilities of the two bars ſhould be ſo proportioned that the texture of each ſhould ſuffer alike.

The increaſe of dimenſions in braſs and ſteel in like elevations of temperature is nearly as two to one. The weight required to break a wire of ſteel is about double that required to produce the ſame effect in braſs. If a bar of braſs be ſoldered to a bar of ſteel of half the thickneſs, their ſtrengths will then be nearly equal; and the yielding, except ſo far as depends on the mechanical difficulty of bending a thicker bar, will be nearly the ſame in both. Steel, being leſs flexible than braſs, diminiſhes the effect of the mechanical advantage of the latter from its thickneſs, and renders it leſs neceſſary to take it into conſideration.

When the ſtrengths of the two bars are nearly equal, and the flexibilities great, there will moſt probably be ſome line within each bar which will poſſeſs the ſame length as that of the unconfined bar at the ſame temperature.

If a right line be divided into any number of equal parts, and a number of equal perpendiculars be erected at the ſeveral points of diviſion, the diſtances between the upper extremities of the ſeveral adjacent perpendiculars will be equal. But if theſe intervals be every one alike increaſed while the lines joining the lower extremities continue ſtrait and unaltered in length, it may eaſily be ſhewn that the right line will become regularly polygonal: and if the number of diviſions were indefinitely great, this polygonal line would become circular. The ſteel and braſs, in our experiments, are referable to this theorem; for, while they continue ſtrait, an indefinite number of perpendiculars may be conceived to be drawn from the ſurface of the ſteel to the ſurface of the braſs, at right angles to the plane of contact. Change of temperature will render one bar longer than the other; the perpendiculars will diverge, and the right line will become a circle.

The ſhorter the perpendiculars, that is to ſay, the thinner the bars, the greater will be the divergency from a given difference of length, and conſequently the ſmaller the circle; that is to ſay, the diameters will be nearly in proportion to the thickneſſes. Accordingly it is found that very thin bars of this conſtruction are much more bended by the ſame change of temperature than ſuch as are thicker. If a narrow piece of the thin tinned iron called ſheet-tin, about eight or ten inches long, be rubbed bright, and wetted with a ſolution of ſal ammoniac, and upon this there be applied an equal thin ſlip of ſheet braſs, alſo brightened, and wetted with the ſame ſolution, and the faces be preſſed firmly together by confining them between two thick ſtrait bars of metal, bound together with wire (while in the vice), the whole apparatus may be then gently heated, till the coating of the tin is fuſed, and ſolders the faces together. This period is known by placing a ſmall piece of tin upon the outſide, which will flow when the heat is ſufficiently elevated. When the compound bar thus obtained is bended up in the figure of a pair of tea-tongs, the change of poſition of its extremities is very perceptible, and is ſuch that (the iron being outſide) it not only opens viſibly when held to the fire or touched by the hand, but even when breathed upon.

The practical advantage of this conſtruction is, that the range of motion greatly exceeds that of the mere expanſion or contraction. The diſadvantage to be ſuſpected is, that the properties of the combination may change, ſo that the original motion produced from a known alteration of temperature may not take place after ſome years heating and cooling.

When a thin bar of braſs is attached to another of ſteel of nearly the ſame tenacity and flexibility, and both become expanded by heat, the form of a circular arc will be aſſumed, of which the radius may be diſcovered by admeaſurement; and the difference in length of the two bars aſcertained by an eaſy calculation. Or converſely, if their expanſion be known at any given temperature, it will be eaſy to determine the curve. In order to aſcertain from direct experiment, without reference to pyrometrical tables, the proportion between the lengthening of the braſs beyond the ſteel, and the ſpace moved through by the extremity of a bar of this kind, I had recourſe to experiment. A bar of plate ſteel, 0.033 inch thick, 0.55 inch wide, and 6.4 inches long, was riveted to a perfectly ſimilar and equal bar of plate braſs by ſmall iron rivets, as in Figure 1, Plate V. It was then pinned at one end againſt a ſtrait bar of copper, which it touched through its whole length, from the pin to the other end that was left at liberty. The diſtance from the pin to the free end was 6.35 inches. Immediately oppoſite the laſt-mentioned end a pin was fixed upright in the copper, very near the end, but not touching it. On the pin was ſlided a ſpring clip, with a ſhort tail as at C in Figure 2, where AB repreſents the compound bar and DE the pin. The braſs ſide of the bar was next the copper. When the temperature was 40°

of Fahrenheit, both bars touched; but upon immerſing the whole in boiling water, the compound bar bended, and puſhed the clip through the ſpace of 0.16 inch. This ſpace is the verſed ſine of the circular arc AC, produced by raiſing the temperature from 40° to 212°, namely 172°. But as the verſed ſine 0.16 is to the ſine 6. 35, ſo is the ſine 6.35 to a fourth number 252; which added to the verſed ſine gives the diameter 252.23 inches of the circle into which the upper face of the ſteel bar had bended, and its circumference 792 inches. But the difference between the diameter of the circle, meaſured on the braſs convex outer face, and that bounded by the interior or concave face of the ſteel, was twice the thickneſs of the bar, or 0.132 inch. But it ſeems fair to meaſure the real lengths of the bars through the middle of their ſolidities. The difference of diameter of two circles paſſing through the bars in this manner would conſequently be no more than 0.066 inch; and of their circumferences 0.207, which is the expanſion of the whole circle of braſs beyond the ſteel. Whence the whole periphery 792 inches is to the length of the bar (neglecting the expanſion endways) 6.35 as the whole difference of the circumferences 0.207 inch is to the exceſs of expanſion in the braſs beyond the ſteel 0.00166 inch. Now as the motion of the end of the compound bar by flexure was 0.16 inch, it was nearly one hundred times as great as the linear difference of expanſion in the two metals.

Since the verſed ſines of very ſmall arcs may be taken to be as the ſquares of the lengths of the arcs themſelves, and in ſimilar arcs they are as the diameters, the quantities of deflection meaſured by the verſed ſines of equal ſmall arcs in circles of different diameters will be inverſely as the ſquares of the diameters, and directly as the diameters, or more ſimply in the inverſe ratio of the diameters themſelves. And as the ſteel made uſe of in this experiment was about five times the thickneſs of ordinary watch-ſpring, it will follow, that a compound bar made of watch-ſpring and braſs of the ſame thickneſs would have been deflected five hundred times as much as the mean exceſs of expanſion of the braſs beyond the ſteel.

Fig. 10, Plate V, repreſents the balance conſtructed on this principle, and known by the name of the expanſion balance. The outer part of the rim is braſs, and the inner ſteel. After this compound rim is brought to its figure by turning, it is cut through in three places A, B, C, which ſets one end of each third part of the periphery at liberty to move outwards when the temperature is diminiſhed, or inwards when it is increaſed. D, E, F are three ſimilar and equal maſſes of metal fitted upon the circular bars in a proper manner to admit of their being fixed at any required diſtance from the extremity, where the motion is moſt conſiderable. G, H, I are three ſcrews, the heads of which may be ſet nearer to or farther from the centre, and ſerve as weights to effect the adjuſtments for poſition and rate. The peculiar advantage of this balance may be explained as follows: When an increaſe of heat diminiſhes the elaſtic force of the pendulum-ſpring K, the outer braſs rim being lengthened more than the ſteel, muſt throw the weights DEF nearer to the axis, and diminiſh the effect of the inertia of the balance, which conſequently is as ſpeedily carried through its vibration as before. And on the contrary, when cold weather adds to the elaſtic force of the ſpring, the ſame weights are alſo thrown farther out, and prevent the acceleration which would have followed. The exact adjuſtment of the weights is found by trial of the going of the machine. If it gain by heat, the weights compenſate too much, and muſt be moved farther from the extreme ends of the circular compound bars;

but if the gain be produced by cold, the ſpring predominates, and the weights will accordingly require to be ſet farther out.

I have not met with any application of this principle to pendulums, though it may be applied to them in a variety of methods. Fig. 3 repreſents an apparatus by which I have ſuſpended the ſimple pendulum of a clock in my poſſeſſion. Its chief advantage conſiſts in the facility of adjuſting for temperature and rate, independently of each other, without ſtopping the clock. It is peculiarly applicable to the pendulum for obtaining an univerſal meaſure, contrived by Hatton and improved by Whitehurſt and by Dr. George Fordyce *.

In the Figures 3, 4, 5, 6, 7, 8, 9, Plate V, the ſame letters denote the ſame things. Fig. 3 exhibits the entire apparatus together, and the other figures ſhew the parts ſeparately taken. II is a braſs platform, which is to be firmly ſcrewed in the horizontal poſition upon the ſteady ſupport intended to ſuſtain the pendulum. At OO are two ſlits through which a pair of ſprings paſs, and ſerve in the uſual manner to afford the centres of ſuſpenſion of the pendulum. Between two edge bars KK is included the tapped part of a piece of turned ſteel FF, of which nearly one half L is tapped with a left-hand ſcrew, and the other half R with a right-hand ſcrew of the ſame thread, a ſmall portion near the middle being reduced to afford a complete termination to the ſcrews. The outer tails at the extremities FF are ſquared to receive a milled head G for the purpoſe of moving it round. Two bridge-pieces HH are tapped to ſerve as nuts to the ſcrews, and are ſo carefully filed at bottom, together with the edge bars, that when the bars are fixed to the platform the bridge-pieces move ſtiffly along the face when the ſcrew is turned. The motion, from the nature of the ſcrews, is in each equal, and contrary to the other. At the outer corner above of each bridge-piece an edge is prominent, which is reduced at top, all but the corners, to afford a ſteady lodgement for a compound bar EE, without ſuffering it to touch the reſt of the ſurface of thoſe pieces, or to move at all horizontally. The bar EE is compoſed of braſs and ſteel, the braſs being laid uppermoſt. It is perforated in the middle at Q, Fig. 8, and properly opened, to afford a ſteady bearing for the lower extremity of the ſcrew AB, Fig. 3, of which the figure may be ſeen at S, in Figure 7. The ſcrew is provided with a double milled head, and graduated circle, each diviſion of which anſwers to a difference of one ſecond in the daily rate. The branches MM ſupport a face, bearing either a nonius, or, as I have it, a ſingle ſtroke, for the purpoſe of ſetting the ſcrew AB. The nut C receives the ſprings of the pendulum in a ſlit P, terminating above in grooves VV, Fig. 7; and the extremities of the legs TT are ſufficiently below S to allow the weight of the pendulum itſelf to preſerve the vertical ſituation of the ſcrew. From the nut C proceeds a flat plate DD, which fills the whole ſpace between the edge bars KK, and prevents the nut from moving horizontally, while it is left at liberty to riſe or fall in the vertical direction.

The effect of this apparatus will be underſtood without difficulty after this deſcription.

---

* I do not pretend to inſinuate that the practical ſolution of this problem has yet been cleared of the many difficulties which attend it. The pendulum here mentioned conſiſts of a flat ſteel wire ſuſpending a weight, which vibrates from a notch in a plate adjuſtable at different heights to meaſure the linear difference between the ſeconds and half-ſeconds pendulum. Mr. Hatton, as I am informed, applied many years ago to the Society for the Encouragement of Arts for a premium in favour of this invention. Mr. Whitehurſt purſued the ſame object afterwards; and at his death the apparatus was purchaſed by Dr. George Fordyce, who has given an account of it, with drawings, in the Philoſophical Tranſactions for the year 1794, page 2.

When the pendulum N, Fig. 3, becomes lengthened by heat, it is evident that it would go ſlower if the greater expanſion of the braſs in the compound bar EE did not at the ſame time render the whole bar more convex above, and raiſe the ſcrew and its nut, and with it the pendulum itſelf, which is by that means correſpondently ſhortened at O. If the common rate of going from day to day differ from mean time, the adjuſtment muſt be made by the ſcrew A. But if this rate, being ſteady while the temperature continues unaltered, ſhould be found to change when the temperature is changed, the adjuſtment muſt be made by the ſcrew FF. That is to ſay, the bridges HH are to be moved further aſunder, if the pendulum loſes by heat; but if, on the contrary, heat cauſes it to gain, the effect of the compenſation in the bar EE is too conſiderable, and the remedy will conſiſt in moving the bridges nearer to each other.

Thus it is ſeen that an adjuſtable compenſation for temperature may be applied by this method with the ſame facility as the adjuſtment for rate. It only remains to be obſerved, for the inſtruction of actual workmen, by what method it is aſcertained that the bar EE ſhall poſſeſs ſufficient variation in its flexure to produce the deſired effect within the limits of the bridge-pieces. For this purpoſe it is adviſable to make a compound bar of braſs and ſteel, faſtened together by riveting, ſoft-ſoldering, or brazing; the latter method of which is moſt adviſable. The length of this bar may be ſix or eight inches, and the thickneſs of each metal may be between the twentieth and thirtieth part of an inch. After reducing the two bars in their conjoined ſtate to an uniform thickneſs by filing and calipering as much as may be requiſite, the whole flexure of the bar, by a conſiderable number of degrees alteration of temperature, may be aſcertained in the method deſcribed by reference to Fig. 2. The ſpace run through by the clip C will be the verſed ſine of the arc of flexure; whence the verſed ſines or deviations from the right line may be deduced in ſhorter lengths; for theſe are very nearly in the preſent caſe as the ſquares of the lengths of the bar. The effective length of the compound bar EE muſt be meaſured from one of the bridges E to the middle point; that is to ſay, it will be equal to half the diſtance between the bearing edges when drawn as far as poſſible from each other. The proportion will therefore be: as the ſquare of the whole bar is to the ſpace moved through by the clip, ſo is the ſquare of the effective bar of the apparatus to its flexure by expanſion. From the length of the intended pendulum, and the nature of the metal of its rod, its direct expanſion for the number of degrees in queſtion may be known from various tables, or by experiments. The rates of expanſion in braſs and ſteel have already been quoted in this paper. It is taken for granted that the compound bar here ſpoken of has been made ſufficiently thick, and that the deviation by flexure in the effective bar, deduced from the proportion of the ſquares, will prove leſs than the direct expanſion of the pendulum. If it ſhould prove greater, the bar is too thin, and there is no remedy but to make a thicker. We may however proceed on the ſuppoſition that the quantity of deviation in the effective bar is conſiderably leſs. It will be moſt convenient that this deviation ſhould be about one-third leſs. If the difference do not exceed this quantity, a piece of the long bar may be cut, and fitted to the apparatus. If it do exceed, the flexibility of the bar from temperature will be increaſed by reducing it on both ſides with the file. The quantity of deviation will be nearly in the inverſe proportion of the thickneſs, as has already been ſtated. Whence it is obvious, that the requiſite thickneſs may be deduced and acquired from the original experiment of flexure by change of temperature. But it is ſafer and better to take the ſmall additional trouble of aſcertaining the rate of flexure from time to time during the filing, by means of the ſimple apparatus, Fig. 2.

III.

Obſervations and Experiments on the Light, Expence and Conſtruction of Lamps and Candles, and the Probability of rendering Tallow a Subſtitute for Wax.

If we diſtribute the catalogue of human wants in the order of the neceſſity of each, food will occupy the firſt place; and, next to this, in moſt climates of the earth, the articles of fuel, lodging, and clothing will immediately preſent themſelves. The total want of any of theſe neceſſarily implies extreme diſtreſs; and if ſuch a privation be applied even in fancy to an entire ſociety, every notion of comfort and civilization ſeems at once to diſappear. Inferior only to theſe in its urgency is the neceſſity of artificial light during the abſence of the ſun. We might indeed exiſt without it; but how large a portion of our lives would in that caſe be condemned to a ſtate little ſuperior in efficacy to that of the animals around us! It might be a curious ſpeculation to enquire how far and in what reſpects the morals of man would become degraded by the want of ſo important an art; ſince every uſeful art muſt ſurely in its conſequences affect the moral principles of ſociety. But it is ſufficient on the preſent occaſion that, previous to entering upon a diſſertation reſpecting the art of illumination, a train of ideas has ſlightly been pointed out, which cannot fail to ſhew its magnitude and importance.

We are acquainted with no means, unleſs we may except electricity, of producing light but by combuſtion, and this is moſt probably of the ſame nature. The rude method of illumination conſiſts in ſucceſſively burning certain maſſes of fuel in the ſolid ſtate. Common fires anſwer this purpoſe in the apartments of houſes, and in ſome light-houſes: ſmall pieces of reſinous wood, and the bituminous coal called kannel-coal, are in ſome countries applied to the ſame uſe; but the moſt general and uſeful method is that in which fat oil, of an animal or vegetable kind, is burned by means of a wick. Theſe inſtruments of illumination are either lamps or candles. In the lamp, the oil muſt be one of thoſe which retains its fluidity in the ordinary temperature of the atmoſphere. The candle is formed of an oil, or other material, which is not fuſible but at a temperature conſiderably elevated.

The method of meaſuring the comparative intenſities of light is one of the firſt requiſites in an enquiry concerning the art of illumination. Two methods of conſiderable accuracy are deſcribed in the Traité d'Optique of Bouguer, of which an abridged account is given by Dr. Prieſtley in his Optics [*], page 540. The firſt of theſe two methods has been uſed by others ſince that time, and probably before, from its very obvious nature, but particularly by Count Rumford, who has given a deſcription and drawings of an inſtrument called the Photometer, in the Philoſophical Tranſactions for 1794. The principle it is grounded upon is, that if two lights ſhine upon the ſame ſurface at equal obliquities, and an opake body be interpoſed, the two ſhadows it will produce muſt differ in blackneſs or intenſity in the ſame degree. For the ſhadow formed by intercepting the greater light

[*] The title of this well-known work is: *The Hiſtory and preſent State of Diſcoveries relating to Viſion, Light, and Colours.* By Joſeph Prieſtley, LL. D. F. R. S. London. Johnſon, 1772.

will be illuminated by the ſmaller light only, and reverſely the other ſhadow will be illuminated by the greater light. That is to ſay, in ſhort, the ſtronger light will be attended with the deeper ſhadow. But it is eaſy, by removing the greater light to a greater diſtance, to render the illumination it produces at the common ſurface equal to that afforded by the leſs. Experiments of this kind may be conveniently made by faſtening a ſheet of white paper againſt the wall of a room. The two lights or candles intended to be compared, muſt then be placed ſo that the ray of light from each ſhall fall with nearly the ſame angle of incidence upon the middle of the paper. In this ſituation, if a book or other object be held to intercept part of the light which would have fallen on the paper, the two ſhadows may be made to appear as in Figure 10, Plate V, where A repreſents the ſurface illuminated by one of the lights only; B, the ſurface illuminated by the other light; C, the perfect ſhadow from which both lights are excluded. It will eaſily be underſtood that the lights about D and E, near the angle F, will fall with equal incidences when the double ſhadow is made to occupy the middle of the paper; and conſequently, if one or both of the lights be removed directly towards or from the paper, as the appearances may require, until the two ſhadows at E and D have the ſame intenſity, the quantities of light emitted by each will be as the ſquares of the diſtances from the paper. By ſome experiments made in this way in the year 1785, I was ſatisfied that the degree of illumination could be thus aſcertained to the eightieth or ninetieth part of the whole.

By experiments of this kind many uſeful particulars may be ſhewn. Thus, for example, the light of a candle, which is ſo exceedingly brilliant when firſt ſnuffed, is very ſpeedily diminiſhed to one-half, and is uſually not more than one-fifth or one-ſixth before the uneaſineſs of the eye induces us to ſnuff it. Whence it follows, that if candles could be made ſo as not to require ſnuffing, the average quantity of light afforded by the ſame quantity of combuſtible matter would be more than doubled. In the ſame way, likewiſe, ſince the coſt and duration of candles, and the conſumption of oil in lamps, are eaſily aſcertainable, it may be ſhewn whether more or leſs of light is obtained at the ſame expence during a given time, by burning a number of ſmall candles inſtead of one of greater thickneſs. From a few experiments already made out of the numerous and uſeful ſeries that preſents itſelf, I have reaſon to think that there is very much waſte in this expenſive article of accommodation.

In the lamp there are three articles which demand our attention, the oil, the wick, and the ſupply of air. It is required that the oil ſhould be readily inflammable, without containing any fetid ſubſtance which may prove offenſive, or mucilage, or other matter, to obſtruct the channels of the wick. I do not know of any proceſs for meliorating oils for this purpoſe, excepting that of waſhing with water containing acid or alkali. Either of theſe is ſaid to render the mucilage of animal oils more ſoluble in the water; but acid is preferred, becauſe it is leſs diſpoſed to combine with the oil itſelf. The office of the wick appears to be chiefly, if not ſolely, to convey the oil by capillary attraction to the place of combuſtion. As the oil is conſumed and flies off, other oil ſucceeds, and in this way a continued current of oil and maintenance of the flame are effected. But as the wicks of lamps are commonly formed of combuſtible matter, it appears to be of ſome conſequence what the nature and ſtructure of this material may be. It is certain that the flame afforded by a wick of ruſh differs very conſiderably from that afforded by cotton; though perhaps this difference may, in a great meaſure, depend on the relative dimenſions of each. And if we may judge from the different odour in blowing out a candle of each ſort, there is ſome reaſon to ſuſpect that the decompoſition of the oil is not effected preciſely in the ſame manner in each. We have alſo ſome obſcure accounts of prepared wicks for lamps, which are ſtated to poſſeſs the property of facilitating the combuſtion of very impure oils, ſo that they ſhall burn for many hours without ſmoke or ſmell [*].

The acceſs of air is of the laſt importance in every proceſs of combuſtion. When a lamp is fitted up with a very ſlender wick, the flame is ſmall, and of a brilliant white colour: if the wick be larger, the combuſtion is leſs perfect, and the flame is brown: a ſtill larger wick not only exhibits a brown flame, but the lower internal part appears dark, and is occupied by a portion of volatiliſed matter, which does not become ignited until it has aſcended towards the point. When the wick is either very large or very long, part of this matter eſcapes combuſtion, and ſhews itſelf in the form of coal or ſmoke. The different intenſity of the ignition of flame, according to the greater or leſs ſupply of air, is remarkably ſeen by placing a lamp with a ſmall wick beneath a ſhade of glaſs not perfectly cloſed below, and more or leſs covered above. While the current of air through the glaſs ſhade is perfectly free, the flame is white; but in proportion as the aperture above is diminiſhed, the flame becomes brown, long, wavering, and ſmoky; it inſtantly recovers its original whiteneſs when the opening is again enlarged. The inconvenience of a thick wick has been long ſince obſerved, and attempts made to remove it; in ſome inſtances by ſubſtituting a number of ſmall wicks inſtead of a larger; and in others, by making the wick flat inſtead of cylindrical. The moſt ſcientific improvement of this kind, though perhaps leſs ſimple than the ordinary purpoſes of life demand, is the well-known lamp of Argand. In this the wick forms a hollow cylinder or tube, which ſlides over another tube of metal, ſo as to afford an adjuſtment with regard to its length. When this wick is lighted, the flame itſelf has the figure of a thin tube, to the inner as well as the outer ſurface of which the air has acceſs from below. And a cylindrical ſhade of glaſs ſerves to keep the flame ſteady, and in a certain degree to accelerate the current of air. In this very ingenious apparatus many experiments may be made with the greateſt facility. The inconvenience of a long wick, which ſupplies more oil than the volume of flame is capable of burning, and which conſequently emits ſmoke, is ſeen at once by raiſing the wick; and on the other hand, the effect of a ſhort wick, which affords a diminutive flame merely for want of a ſufficient ſupply of combuſtible matter, is obſervable by the contrary proceſs.

The moſt obvious inconvenience of lamps in general, ariſes from the fluidity of the combuſtible material, which requires a veſſel adapted to contain it, and even in the beſt conſtructed lamps is more or leſs liable to be ſpilled. When the wick of a lamp is once adjuſted as to its length, the flame continues nearly in the ſame ſtate for a very conſiderable time.

---

[*] The economical wicks of M. Leger, concerning which a Report was preſented to the Academy at Paris in 1782 by Condorcet, Lavoiſier, and De Milly, were compoſed of cotton of different ſizes and forms, namely, round and flat, according to the uſe they were intended to ſerve. They were covered with a fat ſubſtance, of a ſmell not diſagreeable, but feebly aromatic. From the trials of theſe commiſſaries it was aſcertained: 1. That they afforded a clearer flame, with leſs undulation. 2. That they conſumed ſomewhat leſs oil; and 3. That they poſſeſſed the remarkable property of affording neither ſmell nor ſmoke, however common the oil made uſe of. I find a difficulty reſpecting theſe experiments, where it is ſaid that the flame with the prepared wick was only ten lines long, and very ſteady; while that of the unprepared wick of compariſon was four inches and a quarter long, of a conical figure, and vibrated much. It ſeems as if this laſt had been diſadvantageouſly trimmed, or too much raiſed, in order to have produced ſuch an extravagant flame. It ſoon blackened a ſilver plate at upwards of a foot diſtance.

It is almoſt unneceſſary to deſcribe a thing ſo univerſally known as a candle. This article is formed of a conſiſtent oil, which envelopes a porous wick of fibrous vegetable matter. The cylindrical form and dimenſions of the oil are given either by caſting it in a mould, or by repeatedly dipping the wick into the fuſed ingredient. Upon comparing a candle with a lamp, two very remarkable particulars are immediately ſeen. In the firſt place, the tallow itſelf, which remains in the unfuſed ſtate, affords a cup or cavity to hold that portion of melted tallow which is ready to flow into the lighted part of the wick. In the ſecond place, the combuſtion, inſtead of being confined, as in the lamp, to a certain determinate portion of the fibrous matter, is carried, by a flow ſucceſſion, through the whole length. Hence ariſes the greater neceſſity for frequent ſnuffing the candle; and hence alſo the ſtation of the freezing point of the fat oil becomes of great conſequence. For it has been ſhewn that the brilliancy of the flame depends very much on the diameter of the wick being as ſmall as poſſible; and this requiſite will be moſt attainable in candles formed of a material that requires a higher degree of heat to fuſe it. The wick of a tallow candle muſt be made thicker in proportion to the greater fuſibility of the material, which would otherwiſe melt the ſides of the cup, and run over in ſtreams. The flame will therefore be yellow, ſmoky, and obſcure, excepting for a ſhort time immediately after ſnuffing. Tallow melts at the 92d degree of Fahrenheit's thermometer; ſpermaceti at the 133d degree; the fatty matter formed of fleſh after long immerſion in water melts at 127°; the *pela*[*] of the Chineſe, at 145°; bees-wax at 142°; and bleached wax at 155°. Two of theſe materials are well known in the fabrication of candles. Wax in particular does not afford ſo brilliant a flame as tallow: but, on account of its leſs fuſibility, the wick can be made ſmaller; which not only affords the advantage of a clear perfect flame, but from its flexibility it is diſpoſed to turn on one ſide, and come in contact with the external air, which completely burns the extremity of the wick to white aſhes, and thus performs the office of ſnuffing. We ſee, therefore, that the important object to ſociety of rendering tallow candles equal to thoſe of wax, does not at all depend on the combuſtibility of the reſpective materials, but upon a mechanical advantage in the cup, which is afforded by the inferior degree of fuſibility in the wax; and that, to obtain this valuable object, one of the following effects muſt be produced: Either the tallow muſt be burned in a lamp, to avoid the gradual progreſſion of the flame along the wick; or ſome means muſt be deviſed to enable the candle to ſnuff itſelf, as the wax candle does; or, laſtly, the tallow itſelf muſt be rendered leſs fuſible by ſome chemical proceſs. I have no great reaſon to boaſt of ſucceſs in the endeavour to effect theſe; but my hope is, that the facts and obſervations here preſented may conſiderably abridge the labour of others in the ſame purſuit.

The makers of thermometers and other ſmall articles with the blow-pipe and lamp, give the preference to tallow inſtead of oil, becauſe its combuſtion is more complete, and does not blacken the glaſs. In this operation, the heat of the lamp melts the tallow which is occaſionally brought into its vicinity by the workman. But for the uſual purpoſes of illumination, it cannot be ſuppoſed that a perſon can attend to ſupply the combuſtible matter. Conſiderable difficulties

---

[*] Pearſon, in the Philoſophical Tranſactions for 1794.

arife

ariſe in the project for affording this gradual ſupply as it may be wanted. A cylindrical piece of tallow was inſerted into a metallic tube, the upper aperture of which was partly cloſed by a ring, and the central part occupied by a metallic piece nearly reſembling that part of the common lamp which carries the wick. In this apparatus the piece laſt deſcribed was intended to anſwer the ſame purpoſe, and was provided with a ſhort wick. The cylinder of tallow was ſupported beneath in ſuch a manner that the metallic tube and other part of this lamp were left to reſt with their whole weight upon the tallow at the ring or contraction of the upper aperture. In this ſituation the lamp was lighted. It burned for ſome time with a very bright clear flame, which, when compared with that of a candle, poſſeſſed the advantage of uniform intenſity, and was much ſuperior to the ordinary flame of a lamp in its colour, and the perfect abſence of ſmell. After ſome minutes it began to decay, and very ſoon afterwards went out. Upon examination it was found, that the metallic piece which carried the wick had fuſed a ſufficient quantity of tallow for the ſupply during the combuſtion; that part of this tallow had flowed beneath the ring, and to other remote parts of the apparatus, beyond the influence of the flame; in conſequence of which, the tube, and the cylinder of tallow were faſtened together, and the expected progreſſion of ſupply prevented. It ſeems probable, that in every lamp for burning conſiſtent oils, the material ought to be ſo diſpoſed that it may deſcend to the flame upon the principle of the fountain reſervoir. I ſhall not here ſtate the obſtacles which preſent themſelves in the proſpect of this conſtruction, but ſhall diſmiſs the ſubject by remarking, that a contrivance of this nature would be of the greateſt public utility.

The wick of a candle, being ſurrounded by the flame, is nearly in the ſituation of a body expoſed to deſtructive diſtillation in a cloſe veſſel. After loſing its volatile products, the carbonaceous reſidue retains its figure, until, by the deſcent of the flame, the external air can have acceſs to its upper extremity. But, in this caſe, the requiſite combuſtion, which might ſnuff it, is not effected. For the portion of oil emitted by the long wick is not only too large to be perfectly burned, but alſo carries off much of the heat of the flame while it aſſumes the elaſtic ſtate. By this diminiſhed combuſtion and increaſed efflux of half-decompoſed oil, a portion of coal or ſoot is depoſited on the upper part of the wick, which gradually accumulates, and at length aſſumes the appearance of a fungus. The candle does not then give more than one-tenth of the light emitted in its beſt ſtate. Hence it is that a candle of tallow cannot ſpontaneouſly ſnuff itſelf. It was not probable that the addition of a ſubſtance containing vital air or oxygene would ſupply that principle at the preciſe period of time required; but, as experiment is the teſt of every probability of this nature, I ſoaked a wick of cotton in a ſolution of nitre, then dried it, and made a candle. When this came to be lighted, nothing remarkable happened for a ſhort time; at the expiration of which a decrepitation followed at the lower extremity of the flame, which completely divided the wick where the blackened part commences. The whole of the matter in combuſtion therefore fell off, and the candle was of courſe inſtantly extinguiſhed. Whether this would have happened in all proportions of the ſalt or conſtructions of the candle I did not try, becauſe the ſmell of azote was ſufficiently ſtrong and unpleaſant to forbid the uſe of nitre in the purſuit. From various conſiderations I am diſpoſed to think that the ſpontaneous ſnuffing of candles made of tallow, or other fuſible materials, will ſcarcely be effected but by the diſcovery of ſome material for the wick which ſhall be voluminous enough to abſorb the tallow, and at the ſame time ſufficiently flexible to bend on one ſide.

The moſt promiſing ſpeculation reſpecting this moſt uſeful article, ſeems to direct itſelf to the cup which contains the melted tallow. The imperfection of this part has already been noticed, namely, that it breaks down by fuſion, and ſuffers its fluid contents to eſcape. The Chineſe have a kind of candle about half an inch in diameter, which, in the harbour of Canton, is called a *lobchock;* but whether the name be Chineſe, or the corruption of ſome European word, I am ignorant. The wick is of cotton, wrapped round a ſmall ſtick or match of the bamboo cane. The body of the candle is white tallow; but the external part to the thickneſs of perhaps one-thirtieth of an inch, conſiſts of a waxy matter coloured red. This covering gives a conſiderable degree of ſolidity to the candle, and prevents its guttering, becauſe leſs fuſible than the tallow itſelf. I did not obſerve that the ſtick in the middle was either advantageous or the contrary; and, as I now write from the recollection of this object at ſo remote a period as twenty-five years ago, I can only conjecture that it might be of advantage in throwing up a leſs quantity of oil into the flame than would have been conveyed by a wick of cotton ſufficiently ſtout to have occupied its place unſupported in the axis of the candle.

Many years ago I made a candle in imitation of the *lobchock*. The expedient to which I had recourſe conſiſted in adapting the wick in the uſual pewter mould: wax was then poured in, and immediately afterwards poured out: the film of wax which adhered to the inner ſurface of the mould ſoon became cool; and the candle was completed by filling the mould with tallow. When it was drawn out, it was found to be cracked longitudinally on its ſurface, which I attributed to the contraction of the wax, by cooling, being greater than that of the tallow. At preſent I think it equally probable that the cracking might have been occaſioned by too ſudden cooling of the wax before the tallow was poured in; but other avocations prevented the experiments from being varied and repeated. It is probable that the Chineſe external coating may not be formed of pure hard bleached wax.

But the moſt deciſive remedy for the imperfection of this cheapeſt, and in other reſpects beſt material for candles, would undoubtedly be to diminiſh its fuſibility. Various ſubſtances may be combined with tallow, either in the direct or indirect method. In the latter way, by the decompoſition of ſoap, a number of experiments were made by Berthollet, of which an account is inſerted in the Memoirs of the Academy at Paris for the year 1780, and copied into the 26th volume of the Journal de Phyſique. None of theſe point directly to the preſent object; beſides which, it is probable that the ſoap made uſe of by that eminent chemiſt was formed not of tallow, but oil. I am not aware of any regular ſeries of experiments concerning the mutual action of fat oils and other chemical agents, more eſpecially ſuch as may be directed to this important object of diminiſhing its ſolubility; for which reaſon I ſhall mention a few experiments made with this view.

1. Tallow was melted in a ſmall ſilver veſſel. Solid tallow ſinks in the fluid, and diſſolves without any remarkable appearance. 2. Gum ſandarach in tears was not diſſolved, but emitted bubbles, ſwelled up, became brown, emitted fumes, and became criſp or friable. No ſolution nor improvement of the tallow. 3. Shell-lac ſwelled up with bubbles, and was more perfectly fuſed than the gum ſandarach in the former experiment. When the tallow was poured off, it was thought to congeal rather more ſpeedily. The lac did not

appear to be altered. 4. Benzoin bubbled without much ſwelling, was fuſed, and emitted fumes of an agreeable ſmell, though not reſembling the flowers of benzoin. A ſlight or partial ſolution ſeemed to take place. The benzoin was ſofter and of a darker colour than before, and the tallow leſs conſiſtent. 5. Common reſin unites very readily with melted tallow, and forms a more fuſible compound than the tallow itſelf. 6. Camphor melts eaſily in tallow, without altering its appearance. When the tallow is near boiling, camphoric fumes fly off. The compound appeared more fuſible than tallow. 7. The acid or flowers of benzoin diſſolves in great quantities without any ebullition or commotion. Much ſmoke ariſes from the compound, which does not ſmell like the acid of benzoin. Tallow alone does not fume at a low heat, though it emits a ſmell ſomething like that of oil-olive. When the proportion of the acid was conſiderable, ſmall needled cryſtals appeared as the temperature diminiſhed. The appearances of ſeparation are different according to the quantity of acid. The compound has the hardneſs and conſiſtence of firm ſoap, and is partially tranſparent. 8. Vitriolated tartar, nitre, white ſugar, cream of tartar, cryſtallized borax, and the ſalt ſold in the markets under the name of ſalt of lemons, but which is ſuppoſed to be the eſſential ſalt of ſorrel, or vegetable alkali ſuperſaturated with acid of ſugar, were reſpectively tried without any obvious mutual action or change of properties in the tallow. 9. Calcined magneſia rendered tallow opake and turbid, but did not ſeem to diſſolve. Its effect reſembled that of lime.

It is propoſed to try the oxigenated acetous acid, or radical vinegar; the acid of ants, of ſugar, of borax, of galls, the tanning principle, the ſerous and gelatinous animal matter, the fecula of vegetables, vegetable gluten, bird-lime, and other principles, either by direct or indirect application. The object, in a commercial point of view, is entitled to an extenſive and aſſiduous inveſtigation. Chemiſts in general ſuppoſe the hardneſs or leſs fuſibility of wax to ariſe from oxigen, and to this object it may perhaps be advantageous to direct a certain portion of the enquiry. The metallic ſalts and calces are the combinations from which this principle is moſt commonly obtained; but the combinations of theſe with fat oils have hitherto afforded little promiſe of the improvement here ſought. The ſubject is however ſo little known, that experiments of the looſeſt and moſt conjectural kind are by no means to be deſpiſed.

IV.

A Memoir upon the Diſcovery of America. By M. Otto.*

It has always been looked upon as a piece of injuſtice, not to have given the name of Columbus to that valuable part of the world which he diſcovered; and that Americanus Veſpucius, who did nothing but follow his footſteps, has had the good fortune of having his name handed down to the moſt diſtant poſterity, to the prejudice of his predeceſſor. What then will be ſaid, if it ſhall be proved that neither of thoſe celebrated navigators was the firſt diſcoverer of this immenſe country; and that this honour belongs to a man ſcarcely known in the republic of letters? This, however, is what I ſhall attempt in the following paper; and if the obſcurity of cotemporary writers, and the diſtance of time, do not afford arguments ſufficient for an abſolute demonſtration, there will, however, be enough to call in queſtion the pretenſions of Chriſtopher Columbus.

---

* From the American Tranſactions, Vol. II. in French and Engliſh. The former appears to be the original; for which reaſon I have compared this with it, and corrected ſeveral paſſages.

I ſhall not here enter into an examination of the reveries of ſome hiſtorians, on the voyages of the Carthaginians, the Atlantis of Plato, the bold expedition of Mador Prince of Wales, and ſon of Owen Guinnedd, of which Hakluyt has preſerved ſome account, nor on the voyages of Bacchus, or the land Ophir of Solomon. Conjectures of this kind, whether true or falſe, cannot leſſen the glory of Columbus, were there not proof that he received, juſt before his expedition, the charts and journal of a learned aſtronomer who had been in America.

Garcilaſſo de la Vega, born at Cuſco in Peru, has given us an hiſtory of his country, in which, to take from Columbus the merit of the diſcovery of America, and to give the honour of it to the Spaniards, he aſſures us, that this navigator had been informed of the exiſtence of another continent by Alonzo Sanchez de Huelva, who, in his voyage to the Canaries, had been driven by a gale of wind to the Antilles; but that his chief information was procured from a celebrated geographer of the name of Martin Behenira. Garcilaſſo ſays nothing more of this Behenira: and ſince we know of no Spaniſh geographer of this name, Garcilaſſo has been ſuſpected of making a ſacrifice of truth to the deſire of wreſting from a Genoeſe the glory of diſcovering the New World.

On looking over with attention a liſt of all the learned men of the fifteenth century, I find the name of Martin Behem, a famous geographer and navigator. The chriſtian name is the ſame with that mentioned by Garcilaſſo, and I find that the ſyllables *ira* added to his name are owing to a particular circumſtance, namely, the honour conferred on him by John II. King of Portugal. It is then poſſible that this Martin Behem is the ſame perſon as Martin Behenira mentioned by Garcilaſſo; but this vague conjecture will receive the ſtamp of truth by the following detail:

The literary hiſtory of Germany gives an account of a Martin Behem, Beheim or Behin, who was born at Nurenberg, an Imperial city of the circle of Franconia, of a noble family, ſome branches of which are yet extant. He was much addicted to the ſtudy of geography, aſtronomy, and navigation from his infancy. At a more mature age, he often thought on the poſſibility of the exiſtence of the Antipodes, and of a weſtern continent. Filled with this great idea, he paid a viſit in 1459 to Iſabella, daughter of John I. King of Portugal, and Regent of the duchy of Burgundy and Flanders. Having informed her of his deſigns, he procured a veſſel, in which he made the diſcovery of the iſland of Fayal in 1460. He there eſtabliſhed a colony of Flemings, whoſe deſcendants yet exiſt in the Azores, which were for ſome time called the Flemiſh Iſlands. This circumſtance is proved, not only by the writings of cotemporary authors, but alſo by the manuſcripts preſerved in the records of Nurenberg, from the Latin of which the following is tranſlated: "Martin Behem tendered his ſervices to the daughter of John King of Luſitania, who reigned after the death of Philip of Burgundy, ſurnamed the Good; and from her procured a ſhip, by means of which, having ſailed beyond all the then known limits of the Weſtern Ocean, he was the firſt who in the memory of man diſcovered the iſland of Fayal, abounding with beech trees, which the people of Luſitania call Faye; whence it derived its name. After this he diſcovered the neighbouring iſlands, called by one general name, the Azores, from the multitude of hawks which build their neſts there (for the Luſitanians uſe this term for hawks, and the French too uſe the word eſſos or eſores in their purſuit of this game); and left colonies of the Flemiſh on them, when they began to be called Flemiſh Iſlands, &c." Although this record is contrary to the generally received opinion that the Azores were diſcovered by Gonſalva Velho, a Portugueſe, yet its authenticity cannot be doubted: it is confirmed by ſeveral cotemporary

temporary writers, and eſpecially by Wagenſeil, one of the moſt learned men of the laſt century; who, after having travelled into Africa, and throughout all Europe, was made Doctor of Laws at Orleans, and choſen Fellow of the Academies of Turin and Padua, although he was a German by birth. The particulars are to be found in his Univerſal Hiſtory and Geography. I have moreover received from the records of Nurenberg a note written in German, on parchment, which contains the following facts: "Martin Beham, Eſquire, ſon of Mr. Martin Beham, of Scoperin, lived in the reign of John II. King of Portugal, in an iſland which he diſcovered, and called the Iſland of Fayal, one of the Azores, lying in the Weſtern Ocean."

After having obtained from the Regent Iſabella a grant of Fayal, and reſided there about twenty years, during which time he was buſied in making freſh diſcoveries in geography, by ſmall excurſions which need not be mentioned, Behem applied in 1484 (which was eight years before Columbus's expedition) to John II. King of Portugal, to procure the means of undertaking a great expedition towards the ſouth-weſt. This prince gave him ſome ſhips, with which he diſcovered that part of America which is now called Brazil; and he even ſailed to the Straits of Magellan, or to the country of ſome ſavage tribes whom he called Patagonians, from the extremities of their bodies being covered with a ſkin more like a bear's paws than human hands and feet. This fact is proved by authentic records, preſerved in the archives of Nurenberg, one of which in particular deſerves attention: "Martin Behem, traverſing the Atlantic Ocean for ſeveral years, examined the American iſlands, and diſcovered the Strait which bears the name of Magellan, before either Chriſtopher Columbus or Magellan ſailed thoſe ſeas; whence he mathematically delineated on a geographical chart, for the King of Luſitania, the ſituation of the coaſt around every part of that famous and renowned Strait long before Magellan thought of his expedition." This aſſertion is ſupported by Behem's own letters, written in German, and preſerved in the archives of Nurenberg, in a book which contains the birth and illuſtrious actions of the nobility of that city. Theſe letters are dated in 1486, that is, ſix years before the expedition of Columbus. This wonderful diſcovery has not eſcaped the notice of cotemporary writers. The following paſſage is tranſlated from the Latin chronicle of Hartman Schedl: "In the year 1485, John II. King of Portugal, a man of a magnanimous ſpirit, furniſhed ſome galleys with proviſions, and ſent them to the ſouthward, beyond the Straits of Gibraltar. He gave the command of this ſquadron to James Canus, a Portugueſe, and Martin Behem, a German of Nurenberg in Upper Germany, deſcended of the family of Bonna: a man very well acquainted with the ſituation of the globe; bleſſed with a conſtitution able to bear the fatigues of the ſea; and who, by actual experiments and long ſailing, had made himſelf perfectly maſter with regard to the longitudes and latitudes of Ptolemy in the weſt. Theſe two, by the bounty of Heaven, coaſting along the Southern Ocean, and having croſſed the Equator, got into the other hemiſphere, where, facing to the eaſtward, their ſhadows projected towards the ſouth and right hand. Thus, by their induſtry, they have opened to us another world, hitherto unknown, and for many years attempted by none but the Genoeſe, and by them in vain. Having finiſhed this cruiſe in the ſpace of twenty-ſix months, they returned to Portugal with the loſs of many of their ſeamen by the violence of the climate."

This paſſage becomes more intereſting from being quoted in a book on the ſtate of Europe during the reign of the Emperor Frederick III. by the learned hiſtorian Æneas Sylvius, afterwards Pope Pius II.. This hiſtorian died before the diſcoveries of Behem were made; but the publiſhers of his works thought the paſſage in Hartman Schedl ſo important, that they inſerted it in the hiſtory. We alſo find the following particulars in the remarks made by Petrus Mateus on the canon law, two years before the expedition of Columbus: "*Prima navigationes*, &c.—The firſt Chriſtian voyages to the newly diſcovered iſlands became frequent under the reign of Henry, ſon of John, King of Luſitania. After his death Alphonſus V. proſecuted the deſign; and John, who ſucceeded him, followed the plan of Alphonſus, by the aſſiſtance of Martin Bohem, a very ſkilful navigator; ſo that in a ſhort time the name of Luſitania became famous over the whole world." Cellarius, one of the moſt learned men of his age, ſays expreſsly, "*Behaimeus non modo*, &c.—Bœhm did not think it enough to ſurvey the iſland of Fayal, which he firſt diſcovered, or the other adjacent iſlands which the Luſitanians call Azores, and we, after the example of Bœhm's companions, call Flemiſh iſlands, but advanced ſtill farther and farther ſouth, until he arrived at the remoteſt ſtrait, through which, Ferdinand Magellan, following his track, afterwards ſailed, and called it after his own name." All theſe quotations, which cannot be thought tedious, ſince they ſerve to prove a fact almoſt unknown, ſeem to demonſtrate, that the firſt diſcovery of America is due to the Portugueſe, and not to the Spaniards; and that the chief merit belongs to a German aſtronomer. The expedition of Ferdinand Magellan, which did not take place before the year 1519, aroſe from the following fortunate circumſtance: This perſon, being in the apartment of the King of Portugal, ſaw there a chart of the coaſt of America, drawn by Behem, and at once conceived the bold project of following the ſteps of this great navigator. Jerome Benzon, who publiſhed a deſcription of America in 1550, ſpeaks of this chart; a copy of which, ſent by Behem himſelf, is preſerved in the archives of Nurenberg. The celebrated aſtronomer Riccioli, though an Italian, yet does not ſeem willing to give his countryman the honour of this important diſcovery. In his Geographia Reformata, book iii. p. 90, he ſays, "Chriſtopher Columbus never thought of an expedition to the Weſt Indies until his arrival in the iſland of Madeira, where, amuſing himſelf in forming and delineating geographical charts, he obtained information from Martin Bœhm, or, as the Spaniards ſay, from Alphonſus Sanchez de Huelva, a pilot, who had chanced to fall in with the iſland afterwards called Dominica." And in another place: "Let Bœhm and Columbus have each their praiſe; they were both excellent navigators; but Columbus would never have thought of his expedition to America, had not Bœhm gone there before him. His name is not ſo much celebrated as that of Columbus, Americus, or Magellan, although he is ſuperior to them all."

But the moſt poſitive proof of the great ſervices rendered to the crown of Portugal by Behem, is the recompenſe beſtowed on him by King John, who in 1485 knighted him in the moſt ſolemn manner, in the preſence of all his court. I have before me a German paper, extracted from the archives of Nurenberg, to the following purport: "In the year 1485, on the 18th of February, in Portugal, in the city of Allaſavas, and in the church of St. Salvador, after the maſs, Martin Behem of Nurenberg was made a knight, by the hands of the moſt puiſſant Lord John, the Second, King of Portugal, Algarve, Africa, and Guinea; and his chief Squire was the King himſelf, who put the ſword in his belt; and the Duke of Begia was his ſecond Squire, who put on his right ſpur; and his third Squire was Count Chriſtopher de Mela, the King's couſin, who put on his left ſpur; and his fourth Squire was Count Martini Marbarinis, who put on his iron helmet; and the King himſelf gave him the blow on the ſhoulder; which was done in the preſence of all the princes, lords, and knights of the kingdom; and he eſpouſed the daughter of a great lord, in conſideration of the important ſervices he had performed; and he was made governor of the iſland of Fayal." Theſe marks of diſtinction, conferred on a ſtranger, could not be meant as a recompenſe for the diſcovery of the Azores, which was made twenty years before, but as a reward for the diſcovery of Congo, from whence the Chevalier Behem had brought gold and different kinds of precious wares. This diſcovery made much greater impreſſion than that of a weſtern world, made at the ſame time, but which neither increaſed the wealth of the royal treaſury, nor ſatisfied the avarice of the merchants.

In 1492 the Chevalier Behem, crowned with honours and riches, undertook a journey to Nurenberg, to viſit his native country and his family. He there made a terreſtrial globe, which is looked on as a maſter-piece for that time, and which is ſtill preſerved in the library of that city. The outline of his diſcoveries may there be ſeen, under the name of weſtern lands; and from their ſituation it cannot be doubted that they are the preſent Coaſts of Brazil, and the environs of the Straits of Magellan. This globe was made in the ſame year that Columbus ſet out on his expedition; therefore it is impoſſible that Behem could have profited by the works of this navigator, who beſides went a much more northerly courſe.

[*To be concluded in the next Number.*]

V.

Analyſis of the Oriental Lapis Lazuli[*]. *By M.* KLAPROTH.

THE analyſis of lapis lazuli made by Margraff has ſhewn that the blue colour of this ſtone is not owing to copper, as has commonly been thought, but that it ariſes from iron. But as we have not hitherto poſſeſſed any accurate analyſis of this ſtone, I have thought it might be uſeful to examine it anew. In fact, Margraff informs us, that lime, gypſum, and ſilex, together with iron, are its component parts; but he does not determine their proportions; and his analyſis is incomplete, as he does not mention the alumine which this ſtone alſo contains.

According to Rinmann, the lapis lazuli contains lime, quartz, iron, and the acid of fluor, I have not found the latter; and it is probable that Rinmann's opinion was grounded upon the phoſphoreſcence of this ſtone when it is heated.

Cronſtedt and ſome others have affirmed that the lapis contains a quantity of ſilver, after the rate of two ounces per quintal. But my eſſays have afforded no certain indication of the preſence of this metal.

I ſelected for experiment a ſpecies of the lapis of a beautiful deep blue colour, and carefully ſeparated the white and pyritous ſpecks.

A. One hundred parts of lapis lazuli in thin flakes were kept in a ſtate of ignition for half an hour in a porcelain crucible. They loſt two parts of their weight; the colour was not at all changed. This permanence at a great heat induced me to think that the ſtone might be of advantage in enamelling; to which opinion I was the more inclined, from

---

[*] From the Annales de Chimie, XXI. 150. The French tranſlation is made from the German, by Citizen Taſſaert, but whence taken is not ſaid.

Bergmann having preſumed that the Chineſe and Japaneſe make-uſe of it for the blue colour of their porcelain *. To aſcertain the truth, I mixed ſome of the very fine powder of lapis lazuli with a proper flux, and diſpoſed it upon porcelain, which I afterwards placed in the enameller's furnace. My expectations were not realiſed, for the colour changed to a blueiſh grey.

B. By expoſure to a more violent fire the lapis was deprived of twelve centenaries of its weight, and was vitrified. I apprehend that the two parts loſt by the firſt ignition conſiſted merely of water, and that the additional ten in the ſecond eſſay conſiſted for the moſt part of carbonic acid. This opinion is ſupported by the efferveſcence of the ſtone, which takes place when an acid is poured upon it, and indicates, though very feebly, that part of the calcareous earth is united to carbonic acid.

C. Upon two hundred grains of the lapis in impalpable powder I poured muriatic acid diluted with an equal meaſure of water (étendu de moitié eau), and digeſted the whole together by a progreſſive heat. The colour became gradually changed to a grey aſh-colour; and when the ebullition commenced the powder was more ſtrongly attacked, and at length became converted into a thick jelly. This was diluted with water; after which nitric acid was added, and the whole boiled until the reſidue had become white. The filtered ſolution was of a pale yellow colour.

D. The product which remained upon the filter had the appearance of ſand, and weighed one hundred and thirty-eight grains. When this was mixed with three parts of cauſtic pot-aſh, and ignited, it afforded a greeniſh maſs. The ſolution of the maſs in water was perfectly colourleſs. By an exceſs of muriatic acid I ſeparated the ſilex, which after ignition weighed 57 grains.

E. The ſolutions C and D, decompoſed by the carbonate of pot-aſh, afforded a yellow white precipitate, which, when dried, amounted to 221 grains, and was re-diſſolved in the muriatic acid.

F. Ammoniac ſeparated from the ſolution E a gelatinous precipitate. This was thrown ſtill wet into a cauſtic lixivium, in which I digeſted it. It was not totally diſſolved, but left a yellowiſh reſidue weighing 113 grains.

G. The fluid which remained after the precipitation by ammoniac was treated with carbonate of pot-aſh, and afforded 59 grains of the carbonate of lime.

H. Upon the 113 grains which were inſoluble in the cauſtic pot-aſh, I poured ſulphuric acid diluted with water. This mixture, after having been heated, aſſumed the form of a jelly. It was diluted with a large quantity of water, and afforded a precipitate of ſilex which after ignition weighed 29 grains.

I. After the ſeparation of the ſilex I poured ammoniac into the ſolution. The precipitate, ſtill humid, being mixed with a cauſtic lixivium, depoſited brown flocks in the liquor, which when dry weighed 13 grains. I diſſolved theſe in the muriatic acid; and this ſolution treated with ammoniac let fall the oxide of iron, which after ignition weighed ſix grains. The carbonate of ammoniac likewiſe precipitated five grains of calcareous earth.

K. The ſolutions F and I by the cauſtic alkalis were treated with the muriatic acid. The precipitate

---

* Opuſcules Phyſiques et Chymiques de Bergmann, Vol. IV. page 32. From the good quality of the cobalt at preſent uſed for this purpoſe in England and elſewhere, there is no reaſon to think that any other material is uſed for full deep blues in the Eaſt. N.

precipitate which was formed and re-diſſolved by exceſs of acid, and afterwards formed again by carbonate of pot-aſh, was diſſolved again by ſulphuric acid. A new precipitate of ſilex was thus obtained, which after ignition weighed ſix grains. The ſilex was ſeparated, and pot-aſh being then added, cryſtals of alum were obtained. Theſe were diſſolved in water, and the alumine, after precipitation, drying, and ignition, weighed 29 grains.

L. I had aſcertained by a previous experiment, that all the calcareous earth contained in the lapis was not ſaturated by the carbonic acid, but that part was combined with the ſulphuric acid. I had boiled in a large quantity of water a portion of the pulveriſed ſtone; the water, after filtration, did not appear very tranſparent. I poured in the muriate of barytes, and a precipitate was immediately formed conſiſting of ſulphate of barytes. In order to aſcertain the proportion of ſulphate of lime, I ſuperſaturated with muriatic acid the liquor which remained from the precipitate C and the water of edulcoration; after which I poured in muriate of barytes, and obtained a precipitate of ſulphate of barytes, which when perfectly dried weighed 19½ grains.

I ſuſpected that the alkalis made uſe of in the experiments D and E might contain a ſmall portion of ſulphate of pot-aſh, and by that means have contributed to the precipitate of the ſulphate of barytes. To aſcertain this fact, I diſſolved an equal quantity of alkali; and having ſuperſaturated it with muriatic acid, and treated it with the muriate of barytes, the ſulphate of barytes which fell down was carefully collected and dried, and weighed 1½ grain, a quantity to be deducted from the foregoing, and leaves 18 grains of ſulphate of barytes to determine the proportion of the ſulphate of each; and by computation I find that 200 grains of the lapis contain 8.18 of ſulphuric acid, of the ſpecific gravity 1.850, or in combination with calcareous earth 13 grains of ſulphate of lime. This calculation is founded upon my experiments, which ſhew that with the difference of a very ſmall fractional quantity, 100 parts of ſulphuric acid of the ſpecific gravity 1.850 form 220 of ſulphate of barytes. The ſame quantity of acid for its ſaturation with calcareous earth demands either 100 parts of carbonate of lime, or 55 of pure lime, and forms 160 of ſulphate of lime.

The 200 grains of lapis lazuli contain therefore as component parts:

				Grains.
Siliceous earth	D 57 H 29 K 6			92
Calcareous earth	G 59 I 5	64	but ignited	35
Alumine	K			29
Oxide of iron	I			6
Sulphuric acid	L			8
Carbonic acid	B			20
Water	A			4
				194
			Loſs	6
				200

But becauſe the calcareous earth is combined partly with the ſulphuric acid and partly with the carbonic acid, it follows that the combination of the lapis is

	Grains.	
Silex	46	100
Alumine	14.50	
Calcareous carbonate	28	
Calcareous ſulphate	6.50	
Oxide of iron	3	
Water	2	

The ſum is here complete, becauſe I have conſidered the calcareous earth as perfectly ſaturated with the carbonic acid; which does not however appear in fact to be the caſe.

VI.

Uſeful Notices reſpecting various Objects.—Roſe-Water—Eau de Luce—Soap of Wool—Sea Sickneſs.

1. *Roſe-Water.*

THE ſimple diſtilled water from roſe-leaves, which is ſold by the name of roſe-water, has the diſadvantage of loſing its fragrance, by a ſpontaneous change which ſeems to be of the nature of the putrid fermentation. This happens in much leſs time than muſt elapſe between the annual ſeaſons when freſh roſe-leaves are to be had. The article is nevertheleſs to be purchaſed at any time of the year; from which circumſtance it has been ſuppoſed that the manufacturing perfumers were in poſſeſſion of ſome method of preventing the proceſs by which it is changed. A philoſophical friend aſſures me that this is not the caſe, but that they diſtil only ſo much roſe-water at a time as they know will keep during the period of the regular demand for that quantity; at the end of which they diſtil the ſame quantity from other roſe-leaves. Their management for inſuring a regular ſupply conſiſts in packing the freſh roſe-leaves with common ſalt in a maſs, to a portion of which, when required, they add water, and diſtil from the mixture.

2. *Eau de Luce.*

THE ſame intelligent friend informs me, that the uſual recipes in the London Pharmacopœia and other books, for making the fragrant alkaline liquor called eau de luce, the leading perfection of which is, that it ſhall poſſeſs and retain a milky opacity, do not ſucceed, but that a ſeparation takes place, and the fluid becomes more or leſs clear by keeping. The uſe of maſtic in this compoſition has hitherto been kept a ſecret. Upon his aſſurance that this is the chief ingredient, I made the following trials. One dram of the rectified oil of amber was diſſolved in four ounces of the ſtrongeſt ardent ſpirit of the ſhops; its ſpecific gravity being .840 at 60 degrees of Fahrenheit. This is the oily ſpirit which is to be added to volatile alkali to form eau de luce, according to Macquer, in his Dictionary, who ſpeaks highly of a recipe to this effect, but with the addition of ten or twelve grains of white ſoap to the ſpirit, previous to the oil. The purpoſes of my experiment did not require the ſoap. A portion of the clear ſpirit was poured upon a larger quantity of fine powdered maſtic, than it was judged could be taken up. This was occaſionally agitated without heat, by which means the gum reſin was for the moſt part gradually diſſolved. One part of the oily ſolution was poured into a phial, and to this was added one part of the ſolution of maſtic. No opacity or other change appeared. Four parts of ſtrong cauſtic volatile alkali were then poured in, and immediately ſhaken. The fluid was of a denſe opake white colour, affording a ſlight ruddy tinge when the light was ſeen through a thin portion of it.

In a ſecond mixture four parts of the alkali were added to one of the ſolution of maſtic; it appeared of a leſs denſe and more yellowiſh white than the former mixture. More of the gum reſinous ſolution was then poured in, but it ſtill appeared leſs opake than that mixture. It was ruddy by tranſmitted light.

The laſt experiment was repeated with the oily ſolution inſtead of that of maſtic. The white was much leſs denſe than either of the foregoing compounds, and the requiſite opacity was not given by augmenting the doſe of the oily ſolution. No ruddineſs nor other remarkable appearance was ſeen by tranſmitted light.

Theſe mixtures were left at repoſe for two days; no ſeparation appeared in either of the compounds containing maſtic; the compound conſiſting of the oily ſolution and alkali became paler by the ſeparation of a cream at the top.

It appears, therefore, that the firſt of theſe three mixtures, ſubject to variation of the quantity of its ingredients, and the odorant additions which may be made, is a good eau de luce.

Chemical writers ſpeak of a milky fluid, under the name of *lac virginale*, made by pouring tincture of benzoin into ſome perfumed water. As this fragrant balſam promiſed to be in ſome reſpects ſuperior to maſtic, I was induced to try it. A ſpirituous ſolution was made of the brown benzoin, which happened to be at hand. To one part of the oily ſolution firſt mentioned was added one part of the clear filtered ſolution of benzoin. Theſe mixed uniformly. Four parts of the cauſtic volatile alkali were then added. The mixture became opake, fawn-coloured and curdled, with much leſs pungency of ſmell than was afforded by the other alkaline compounds. The next day the opake part had conſiderably ſubſided, and left a browniſh tranſparent though turbid fluid above. It appears probable, therefore, that the acid of the benzoin had not only united with part of the volatile alkali, but that the oily and reſinous parts had likewiſe been diſpoſed to coagulate in this arrangement. It is undoubtedly of no value in the preſent point of view.

3. *Soap of Wool.*

I HAVE made a few experiments on the ſaponaceous combination of wool and alkali*. A cauſtic lixivium was made by mixing a ſolution of the cryſtals of ſoda with a due portion of lime and water. To the clear ley, in a ſtate of ebullition, wool was added, a little at a time. It was ſpeedily diſſolved by ſtirring; but on account of the lixivium being too ſtrong, the compound appeared thick, and was poured out before, as it afterwards appeared, the ſaturation of the alkali was effected. The compound, after being left in an earthen veſſel for ſeveral days, was found to be ſcarcely more conſiſtent than treacle. Its ſmell was offenſive, though not ſtrong; it readily diſſolved in water, but ſcarcely lathered at all; and by its action when ſmeared on the hands it appeared to poſſeſs a conſiderable portion of diſengaged alkali. The whole was then diſſolved in water, and left for ſeveral days on account of other avocations. In the mean time a weaker lixivium was boiled with the addition of wool in ſmall portions, till the laſt quantity added remained for a conſiderable time undiſſolved, and was taken out. The fluid poured into cups had the conſiſtence of treacle. It lathered ſcarcely at all with water, in which, however, it readily diſſolved; and upon being uſed with a piece of flannel, it ſeemed admirably adapted for ſcouring that cloth. Four ſucceſſive rinſings in different clear waters ſeemed to have waſhed off the ſoap; but the ſmell ſtill remained very ſtrong in the flannel. It went off in ſix or ſeven hours completely.

---

* Philoſ. Journal, I. 40.

I was deſirous of aſcertaining the ſtate of the wool when it ſhould be again ſeparated by means of an acid. For this purpoſe I added diluted vitriolic acid to the aqueous ſolution of the firſt imperfect ſoap, which contained about half a pound of wool. A ſeparation inſtantly followed; and, to my ſurpriſe, upon ſtirring the liquor, it lathered very well. The next day the conſiſtent part had ſettled to the bottom. The clear liquor, which was conſiderably acid, and had a ſulphureous ſmell, lathered with agitation. The conſiſtent part had the appearance as if moſtly compoſed of broken fibres. The whole was poured into a coarſe cloth, and, after ſtraining away the acid, water was ſuffered to run from a cock into the bag of the cloth, and fell clear into a ſmall baſon which it overflowed below. It was remarkable that this clear taſteleſs water bore a ſtrong bead or head by agitation. The edulcorated matter, when dry, weighed leſs than half an ounce. Hence it ſhould ſeem in this looſe experiment that the acid had not only ſaturated the diſengaged alkali, but had either combined with the wool of the ſoap, or formed a triple compound with the ſoap itſelf, which was moſtly carried off in the edulcoration. The reſidue was probably wool, which had not been completely deprived of its organization in the firſt boiling. But I mean to examine the perfect ſoap with greater attention to quantities.

4. *Sea Sickneſs.*

IN an account of the iſlands of Goree and Senegal, by Citizen Prélong, printed in the xviiith volume of the Annales de Chimie, the author mentions that he ſuffered prodigiouſly and for a long time by the ſea ſickneſs, but was greatly, and afterwards habitually, relieved by taking ten drops of vitriolic ether in a ſpoonful of water. I have alſo been aſſured by the commander of a packet conſtantly paſſing between Harwich and Helvoetſluys, that he always found this diſtreſſing illneſs greatly relieved in his paſſengers by a ſmall quantity of red wine heated with ſpices. The ſea ſickneſs ſeems to be a ſpaſmodic affection of the ſtomach, produced by the alternate preſſure and receſs of the contents of that viſcus againſt its lower internal ſurface, accordingly as the riſe and fall of the ſhip oppoſes or recedes from the action of gravity. Hence it is relieved by change from the erect to the prone poſture, or by removing from the extremity of the veſſel to the vicinity of axis of the pitching motion, near the mainmaſt; and hence alſo, when the organs have become habituated to a regular vibration of one kind in a ſhip for ſeveral months, the ſickneſs may nevertheleſs be again generated by a different vibration in a boat. As it is a habit which requires ſome time to be generated, and comes on gradually, it is not difficult to oppoſe it by mental effort or diverſion; but, to ſuch as have not acquired this facility, it may be acceptable to know that the above ſpirituous ſtimuli have been found of ſervice in counteracting it. The ſtimulus of food, taken even againſt the inclination, has alſo been frequently found to be beneficial.

VII.

A Compariſon between Electrical Machines with a Cylinder, and thoſe which produce their Effect by means of a Circular Plate of Glaſs. With a Deſcription of a Machine of great Simplicity and Power, invented by DR. MARTINUS VAN MARUM.

IT is a remarkable circumſtance that the plate-machine for electricity, firſt invented and publiſhed in this country by Dr. Ingenhouſz, has never been much uſed here, though it has been well received on the Continent, and almoſt univerſally preferred to the machine with a cylinder. This may in ſome meaſure be owing to the improvements in our manufacture of blown glaſs, by which we have been ſupplied with cylinders of conſiderable dimenſions at a moderate price, inſtead of the globes which were originally uſed for this purpoſe. Some years ago (1787) I improved the cylinder-machine by a contrivance for changing the electricity of the conductor from plus to minus almoſt inſtantly, which is deſcribed in the Philoſophical Tranſactions for 1789. From a compariſon of the quantities of electricity accumulated by the friction of a ſquare foot of glaſs, I was induced to adopt the opinion, that cylinders were preferable to plates in every reſpect, excepting the great quantity of ſurface afforded by the latter in machines conſtructed without regard to expence. The labours of the celebrated Dr. Van Marum, together with ſome obſervations of my own, have ſince that time tended to alter my opinion: for which reaſon I ſhall, in the firſt place, enumerate a few general facts, and then proceed to deſcribe his excellent improvement of my contrivance.

I. Electrical machines were formerly made to revolve with conſiderable velocity by a multiplying wheel. This has ſince been rejected in conſequence of the ſtrong excitation and increaſed friction produced by a more advantageous application of the amalgam of zinc and mercury. The machines with a ſingle winch demand the ſame labour as before to work them. They exhibit much more fire in the form of flaſhes and ſparks. But, as far as my experience ſhews, the ſpark from the old machine was denſer and more pungent, the excitation more ſteady, and the time employed in charging ſomewhat ſhorter.

II. A cylinder with a ſingle winch requires larger terminations of its metallic parts to prevent the fire from flaſhing out, than are required either in the old machine, or one of thoſe conſtructed with a flat plate.

III. It frequently happens that the ſimple machine will be in a ſtate to throw out ramifications to the table, to the face of the operator, and into the air; though the actual ſpark is not very denſe, nor the power great, when examined by the time required to charge a battery or jar.

IV. From theſe circumſtances it appeared probable that the electric matter in a charged conductor may be thrown into a ſtate of undulation by an irregular ſupply from the cylinder, and that in this ſtate it will fly off more readily than when ſupplied by a more uniform ſtream. Thus, when the cylinder is of an irregular figure, the action of the cuſhion will be ſtronger on the one ſide than the other, and this irregularity may be increaſed alſo from other cauſes. The irregular ſupply will therefore be made at more diſtant intervals by the ſimple machine, at more frequent reiterations in the machine with the multiplying wheel, and perhaps uniformly from a plate.

V. The effect of this undulation may be ſhewn from various facts. 1. A ſmall wire of many yards in length, communicating from a ball to the ground, will be rendered luminous through its whole length, by ſparks of poſitive electricity given to the ball; but it will not be at all luminous when the ſame quantity of electricity is given in a more continued ſtream by placing the ball in contact with the prime conductor. 2. If a metallic rod be inſulated with a ball at one end, of a diameter ſufficient to prevent the ſpontaneous flaſhing of electricity when it forms part of the prime conductor, and at the other end of the ſtem be fixed another ball of a proper ſize to draw the ſpark; when this laſt ball is placed in contact with the prime conductor, the other ball will not throw out flaſhes; but if it be withdrawn ſo as to receive ſparks, the external ball, though certainly no more electrified than before, will throw out a flaſh every time the ſpark is emitted. 3. A braſs ball of four inches diameter was connected by a metallic ſtem to the end of the prime conductor of an electrical machine in the poſitive ſtate, ſufficiently vigorous to throw a flaſh into the air now and then. The metallic ſtem, which was about ſix inches long, was then changed for another of the ſame dimenſions of deal wood. In this ſtate the ball threw out continual flaſhes into the air. The experiment was frequently repeated, and the laſt reſult may naturally be ſuppoſed to depend on the imperfection or diſcontinuity of the conducting matter in the wooden ſtem. 4. A pointed wire was inſerted in the poſitive conductor of Nairne's electrical machine, with the point upwards. It was then covered with a clean Florence flaſk. The point of the wire occupied the centre of the bottle. Whenever the poſitive ſpark was drawn from the conductor, the point of the wire exhibited the luminous negative ſign. But when the experiment was repeated on the negative conductor and the ſpark drawn, the point emitted the poſitive flaſh ſo as to fill the whole capacity of the bottle with ramified light. In theſe experiments it may be preſumed, that the eſcape at the point was occaſioned by undulation. 5. The lateral ſpark in diſcharging a jar may be urged as an inſtance of the ſame kind.

Theſe obſervations ſeem to give the advantage in favour of plate-machines as far as relates to the eſcape. I ſhall therefore proceed to deſcribe that of Dr. Van Marum[*], and afterwards ſtate from his report, compared with my own experiments, what may be the proportions of electricity collected from each ſquare foot, which paſſes the cuſhion in machines of both kinds.

Plate III exhibits a perſpective view of the machine, and Plate IV a ſection, excluſive of the cuſhions. In the view it may be obſerved that the cuſhions are each ſeparately inſulated upon pillars of glaſs, and are applied nearly in the direction of the horizontal diameter of the plate, inſtead of the vertical diameter as heretofore. The ball diametrically oppoſite to the handle is the prime conductor, and the ſemicircular piece with two cylindrical ends ſerves, in the poſition of the drawing, to receive the electricity from the plate. By the happy contrivance of altering the poſition of this ſemicircular branch from vertical to nearly horizontal, the cylindrical ends may be placed in contact with the cuſhions, and the prime conductor inſtantly exhibits negative electricity. But as it is neceſſary that the cuſhions ſhould communicate with the ground when the poſitive power is wanted, and that they ſhould be inſulated when the negative power is required, there is another ſemicircular branch applied to the oppoſite ſide of the plate nearly at right angles to the firſt. That is to ſay, when poſitive electricity is wanted, this ſecond branch denoted by I. I in the ſection Fig. 1, Pl IV, is placed nearly horizontal, and forms a communication from the cuſhions to the ground through a metallic rod from K behind the mahogany pillar which ſupports the axis; but when on the contrary the negative power is wanted, and the branch from the prime conductor is placed in contact with the cuſhions, this other branch from the axis is put into the vertical ſituation, and carries off the electricity emitted from the plate of glaſs.

---

[*] Abridged from the *Seconde Continuation des Expériences faites par le Moyen de la Machine Electrique Teylerienne.*

The axis of the plate Bh, Fig. 1, Plate IV, is ſupported by a ſingle column A, which for that purpoſe is provided with a bearing-piece K, on which two braſs collar-pieces DD, repreſented more at large and in face in Fig. 3, are fixed, and carry the axis itſelf. The whole of Fig. 1, is reduced to one-eighth of its real dimenſions, unleſs contracted by the ſhrinking of the paper after printing; to obviate which, it may be remarked that the diameter of the plate is 31 Engliſh inches. The axis has a counterpoiſe O, of lead, to prevent too great friction in the collar D neareſt the handle. The arc of the conductor EE, which carries the two ſmall receiving conductors FF, is fixed to the axis G, which turns in the ball H. On the other ſide of the plate is ſeen the other arc II, of braſs wire, half an inch in diameter, fixed to the extremity of the bearing-piece K, ſo that it may be turned in the ſame manner as the arc EE. The two receiving conductors FF are ſix inches long, and two and a half inches in diameter. The double line P repreſents a copper tube terminating in a ball Q. It moves like a radius upon the ſtem R of the ball S, which being ſcrewed into the conductor H, ſerves to confine the arm P in any poſition which may be required. The diameter of the ball S is only two inches, which, together with certain other leſs rounded parts of this apparatus, may ſerve to ſhew that the conſiderable electricity from this machine is leſs diſpoſed to eſcape than if it had proceeded from a cylinder. The diſſipation of electricity along the glaſs ſupports is prevented by a kind of cap T, of mahogany, which affords an electrical well or cavity underneath, and likewiſe effectually covers the metallic caps into which the glaſs is cemented. The lower extremity of the cap is guarded in the ſame manner by a hollow piece or ring V, of mahogany, which covers the metallic ſocket into which the glaſs is cemented. The three glaſs pillars are ſet in ſliding-pieces, as marked on the platform of Plate III, which are 9 inches long.

The rubbers of this machine differ in no eſſential particular from thoſe deſcribed by the inventor in the Journal de Phyſique for February 1791; and the apparatus for applying them is deſcribed in the ſame work for April 1789. Fig. 2. repreſents a ſection of this judicious piece of mechaniſm ſeen from above, and one-fourth of the real ſize. A metallic ſliding-piece bb, is ſlided into a correſpondent face, on the ball Z, which is one of thoſe fixed on the top of the glaſs pillars near the circumference of the glaſs plate in Plate III. To this is affixed the piece dd, which terminates in two hinges gg, that allow the ſprings ee to move in the plane of the horizon. The pieces gg repreſent the wood-work of the cuſhions attached to the extremities of the ſprings by the hinges hh. The ſprings are regulated by the bolt and ſcrew ii. The two cuſhions are thus made to apply to the plate equally through their whole length; the actions on the oppoſite ſides of the plate are accurately the ſame; and the play of the hinges gg, prevents the plate from being endangered by any ſtrain in the direction of its axis. It is certain that, before this adequate proviſion was made to ſecure thoſe eſſential requiſites, it was impracticable to apply the cuſhions to a plate with the ſame ſafety and effect as to cylinders, which poſſeſs much ſtrength from their figure. An ingenious workman will probably find little difficulty in conſtructing theſe rubbers from this deſcription and drawing; but the moſt preciſe information reſpecting every circumſtance and dimenſions is to be found in the letters above quoted.

The inner extremities of the cuſhions are defended by the plates of gum-lac YY, which cover the three ſides or edges, and prevent their attracting the electric matter from the ends of the receiving conductor.

That part of the axis which moves between the collars is made of ſteel. The middle of the non-conducting part of the axis is a cylinder of walnut-tree wood aaaa, baked until its inſulating power is equal to that of glaſs, and then ſoaked in amber varniſh, while the wood ſtill remains hot. The two extremities of this cylinder, which are of a leſs diameter, are forced, by ſtrong blows, with a mallet, into the ſtout braſs caps b and c, in which they are retained by three iron ſcrews dd. The cylinder aa, and the braſs caps are covered with a layer of gum-lac eeee, to preſerve the inſulating ſtate of the wooden cylinder more perfectly, and to prevent the cap b from throwing flaſhes to the rubbers. The bottom of the cap b is ſcrewed home on the tapped extremity of the ſteel axis b. The baſe of the cap c, which is four inches in diameter, terminates in an axis one inch thick, and two in length; the extremity of which is formed into a ſcrew. The glaſs plate is put on this projecting part, and ſecured in its place by a nut of box-wood, forced home by a key, applied in the holes ii. Two rings of felt are applied on each ſide of the glaſs, to defend its ſurface from the contact of the wood and the metal; and the central hole in the glaſs, which is two inches in diameter, contains a ring of box-wood, which prevents its immediate application to the axis.

As it is neceſſary that the axis G ſhould be parallel to the axis of the plate, in order that the conductors FF may move parallel to the plate itſelf, the pillar M is rendered adjuſtable by three bearing ſcrews RR at the bottom, which re-act againſt the ſtrong central ſcrew T, and this is drawn downwards by its nut. The conductors FF are alſo adjuſtable by the ſliding-pieces vv, and the binding-ſcrews ww, which alſo afford an adjuſtment to bring the axis of each ſmall conductor parallel to the face of the glaſs plate. A ſimilar adjuſtment may be obſerved at the extremities of the arc II.

Fig. 4. repreſents a ſection of the moving part of the branch II, one-half of its real ſize. A braſs plate *αα* is ſcrewed to the face of the capital K by three iron ſcrews *β*. To this is ſcrewed another ring δδ, which affords a groove for the moveable ring *γγ*, into which the arms II are fixed. This is accordingly applied in its place before the ring δδ is fixed.

The wooden part of the rubbers GG, Fig. 2, Plate IV, is covered with thin plates of iron, excepting the ſurface neareſt to the glaſs. The intention of this is to maintain a more perfect communication between the rubbed part of the cuſhion and the earth or negative conductor, as the caſe may be.

The plates of gum-lac YY, are applied to the rubbers, each by means of a thin plate of braſs, to which they are affixed by heat. There are two wires riveted in theſe plates, which are thruſt into correſpondent holes in the wooden part of the cuſhion.

The mahogany column A ends in a ſquare ζζ, upon which the piece K is fitted and firmly applied, by means of the ſcrew and nut exhibited in the ſection.

To aſcertain the power of this machine, Dr. Van Marum relates an experiment made before the Directors of the Teylerian Eſtabliſhment, and other philoſophical gentlemen, in circumſtances not very favourable to the apparatus; but to which he gives the preference, on account of the reſpectability of the aſſiſtants. A battery of ninety jars, each containing upwards of a ſquare foot of coated glaſs, was charged to the higheſt degree by 150 turns of the plate, ſo that it diſcharged itſelf. The great Teylerian machine with two plates of ſixty-five inches diameter in its original ſtate, before Dr. Van Marum's improved rubbers were applied to it, never charged the ſame battery, in the moſt favourable circumſtances, in leſs than 66 turns. It follows, therefore, that this ſmall and ſimple machine exhibited $\frac{66}{150}$ths, or about $\frac{4}{9}$ths of the power of that great machine in its firſt ſtate; and probably, if the circumſtances had been alike favourable in each, it would have amounted to one half. The Doctor has grounded a calculation upon theſe facts.; but as he ſtates the rubbed ſurfaces of theſe two machines, probably by ſome miſtake in calculation, to be 1243 and 9636 ſquare inches reſpectively, I ſhall repeat the calculation in this place.

The diameter of the plate is 31 inches, and the length of the cuſhion 9 inches. Then $31\ .7854-\overline{31-18}|^2.7854=522$ ſquare inches rubbed by one cuſhion on one ſide. And $522\times4=2088$ ſquare inches rubbed by the four cuſhions. Again in the great machine, the two plates having a diameter of 65 inches, and eight cuſhions of $15\frac{1}{2}$ inches long, $\overline{65}|^2.7854-\overline{65-31}|^2.7854=2410.4$. And $2410.4\times8=19283$ ſquare inches rubbed. But the intenſity of the electric power of a machine will be in the compound ratio inverſely of the ſurfaces and number of turns when the charge is the ſame; Or $150\times2088:66\times19283::1=$ the intenſity of the larger machine $:4=$ the intenſity of the ſmaller.

To have increaſed the power of ſteady excitation four-fold, is certainly an aſtoniſhing acquiſition. This expreſſion, however, of the intenſities appears to be leſs generally uſeful than that of the ratio of the ſurface rubbed, to that which is charged. This laſt expreſſion becomes very ſimple when the latter quantity is reduced to 1, or unity. Thus, in the two machines here mentioned, the rubbed ſurfaces in inches for the battery are $\frac{19283\times66}{90\times144}$ and $\frac{2088\times150}{90\times144}$, which are equal to the ſimple numbers 90.5 and 24.0, which reſpectively denote the number of inches rubbed to charge one inch of coated glaſs.

The great machine charged a ſingle foot of glaſs, by rubbing 66.6 feet; and a battery of 224 feet at the rate of 94.8 feet rubbed per foot. If the gradual decay of excitation be ſuppoſed the ſame in the ſmall machine here deſcribed, it muſt have commenced with an intenſity of 17.6. In the Philoſophical Tranſactions, already quoted, I have ſtated the commencing intenſity of a cylinder, excited by the amalgam of zinc, as tried by a jar of $2\frac{1}{2}$ feet to be 18.03 and 19.34. But, from my notes, I find that this jar was charged with leſs than 15.0 when the hand was conſtantly preſſed againſt the ſilk-flap; and alſo, that this preſſure increaſed the intenſity as 49 to 39 in ſome few trials, not enough varied and repeated. The labour of turning was very great; much more than, from various circumſtances, I am inclined to ſuppoſe Mr. Van Marum's method requires. From this conſideration, as well as from the numbers, and the probability that, on account of the leſs undulation, the charges by a plate may be higher before they explode than thoſe by a cylinder, and likewiſe from the large ſurface expoſed to friction, I conclude, that the machine deſcribed in this paper is at leaſt equal in ſteady intenſity, and much ſuperior in power of charging, to any cylinder-machine which has ever been made.

VIII.

The Proceſs for giving a beautiful White Colour to Raw Silk without Scouring. By M. BAUME *.

[Concluded from page 32.]

TO complete the deſcription of M. Baumé's proceſs for bleaching ſilk, nothing more remains, than to ſhew in what manner he recovers the ardent ſpirit, and enſures the purity of the acids made uſe of. Theſe circumſtances are of eſſential importance to the art: for the proceſs would be much too expenſive if the ſpirit were loſt, and it could not be made to ſucceed at all if the acid were impure.

The alcohol which has been uſed in bleaching ſilk, is acid, and loaded with colouring matter. In this ſtate it cannot be again uſed. There are two methods of diſtilling it; which have their reſpective advantages and inconveniences. By the firſt, the acid is loſt; which is ſaturated with pot-aſh, in order that the diſtillation may be afterwards performed in a copper alembic. The ſecond is performed by diſtilling with glaſs retorts, or an alembic of ſilver. In either of theſe veſſels, which are not acted upon by the marine acid, the diſtillation may be performed, and the greater part of the acid recovered. The inventor moſt generally practiſed the ſaturation of the acid from reaſons of convenience; but recommends the uſe of a ſilver alembic, as being moſt economical upon the whole, in a manufactory.

A ſolution of pot-aſh is to be poured into the acid ſpirit and ſtirred about to promote the ſaturation. Carbonic acid is diſengaged with ſtrong effervescence from the alkali, and the point of ſaturation is known by the uſual teſt, that the fluid does not redden the tincture of turnſol. The diſtillation is then to be made in the copper alembic, and the alcohol reſerved in proper veſſels, as mentioned at the beginning of this Memoir.

If too much alkali ſhould have been added, the liquor remaining in the alembic may be uſed in another ſaturation. The alkali in this proceſs being an expenſive article, Mr. Baumé endeavoured to ſupply its place by chalk, quick-lime, and lime which had been ſlaked by expoſure to the air. But he found that the action of the ſpirit upon the calcareous earth, or perhaps the abſence of water, prevented the acid from uniting with that ſubſtance. The union does not take place to perfect ſaturation in leſs than five or ſix weeks, even when the alcohol is diluted with upwards of fifty times its bulk of water.

In the ſecond proceſs for diſtilling without alkali, the acid ſpirit is diſtributed into a great number of glaſs retorts, placed in the ſand-bath, on the gallery of a furnace. The firſt product is ſcarcely acid; but what follows is more and more ſo, and muſt be kept in veſſels of glaſs or ſtone ware, which become embarraſſing on account of their number. The fluid which remains in the retorts has the colour of beer ſlightly turbid, and contains the greateſt part of the marine acid. It muſt be poured into one or more retorts, and concentrated by heat.

---

* The Editors of the Journal de Phyſique, to which reference was made at the beginning of this Abſtract, omitted to mention how they obtained it. I find in the Annales de Chimie, XVII. 156, that it was read at the Public Meeting of the Academy at Paris, April 10, 1793.

heat gradually applied. The firſt liquor which comes over is ſlightly red, turbid, and ſcarcely acid. This is to be thrown away, and the receivers changed. The ſucceeding product is the colourleſs marine acid, of an aromatic ſmell reſembling the buds of poplar. The reſin of the ſilk remains in the retort decompoſed by the acid. The marine acid thus obtained is weaker than it originally was; which is in fact of little conſequence, as it is pure, and may be ſafely uſed, either by increaſing the doſe proportional to its diminiſhed ſtrength, or by concentrating it, if required, in the uſual way.

If this diſtillation be made in a ſilver alembic, inſtead of retorts of glaſs, and a capital and worm of pure tin be annexed, the alcohol will be obtained ſo ſlightly acid as ſcarcely to redden the tincture of turnſol; but it is ſufficiently acid to receive injury if preſerved in a copper veſſel.

If a cucurbit of ſilver be prepared, of the capacity of three or four quarts, with a glaſs head, the reſidues of the firſt diſtillation may be treated in this veſſel in the ſame manner as has been directed for glaſs retorts. M. Baumé affirms that he has practiſed all theſe operations with glaſs retorts and a ſmall ſilver alembic, with the moſt perfect ſucceſs; but that he made uſe of pot-aſh to ſaturate the marine acid, becauſe he had not a ſilver veſſel of ſufficient capacity. From the danger of diſtilling large quantities of ardent ſpirit in glaſs veſſels, he is of opinion that no motives of economy are ſufficient to juſtify the riſk attending this method. In the uſe of tin, it is neceſſary to be careful that it contains no adulteration of lead, becauſe the vapours of marine acid have ſufficient power to alter this laſt metal very conſiderably.

Upon the firſt intimation of this new proceſs in France, manufactories were immediately eſtabliſhed, to the number of twenty or more, without the concurrence of Mr. Baumé, by perſons who conſequently were not aware of the apparently minute but very important circumſtances neceſſary to inſure its ſucceſs. In particular, the inventor ſtates that the marine acid of commerce is unfit for this purpoſe.

This acid was formerly prepared with the marine ſalt of the ſaltpetre manufacturers; and even when it is made with good ſalt, the decompoſition is effected with common vitriolic acid which contains nitrous acid. Marine acid mixed with a ſmall quantity of nitrous acid does not prevent the ſilk from being beautifully whitened: it even accelerates the proceſs conſiderably, and in the moſt ſatisfactory manner. But the alcohol, every time it is uſed and rectified, becomes charged with the acid and gas of nitre, which aſſume the characters of the nitrous anodyne liquor. In this ſtate, neither diſtillations nor repeated rectifications from alkali are ſufficient to ſeparate the nitrous matter from the alcohol. Then it is that the ſucceſs of the operator vaniſhes, with a degree of rapidity equal to the advances which encouraged his hopes at the commencement. The ſame diſappointments befel M. Baumé at the beginning of his labours; to prevent which, he directs the preparation of the vitriolic and marine acids to the following effect.

The vitriolic acid of commerce is obtained by burning ſulphur in chambers of lead, with the addition of ſaltpetre, either crude or of the ſecond cryſtallization, and a ſmall portion of flax. This acid is concentrated and rectified in France, at the place of its fabrication, to 66 degrees of Baumé's hydrometer, or ſpecific gravity in the uſual form 1.848. It contains ſulphur, lead, vitriolated tartar, Glauber's ſalt, alum, ſelenite, and particularly the nitrous and marine acid.

To

To purify it, one hundred pounds of this vitriolic acid is to be mixed in a large baſon of copper with the ſame quantity of river water, and ſtirred with a wooden ſpatula. The mixture inſtantly becomes heated to the boiling-water point, and a great quantity of red vapour is diſengaged, which has the ſmell of aqua-regia, and ariſes from the nitrous and marine acids. When this mixture is made, it is proper to immerſe the baſon to a ſuitable depth in a large veſſel of water, to haſten the cooling. As ſoon as it is ſufficiently cooled it is to be drawn off into bottles, and left to become clear during ſeveral days. Great part of the ſulphur falls down. The author obtained from four to ſix drams.

A gallery muſt be provided, on which two rows of iron pots of eleven or twelve inches in diameter are to be properly placed for ſeparate ſand-baths, as M. Baumé always practiſed in the ſublimation of ſal-ammoniac. By this means the retorts are iſolated, and if one breaks, the acid cannot diffuſe itſelf and break the others in its vicinity. An empty retort is then to be placed in each pot, and covered with ſand. In this way they are much more convenient to arrange, and are attended with no riſk.

The acid is in the next place to be decanted and conveyed into the retorts by a ſyphon funnel, and the rectification proceeded upon until it becomes perfectly white. Towards the end of the operation a ſmall quantity of ſulphur ſublimes in the neck of the retort. Inſtead of receivers a ſmall glaſs cup is placed beneath the aperture of each retort, in order to facilitate the diſſipation of the nitrous and marine acids.

When the acid in the retorts is ſufficiently cooled, it is poured a ſecond time into the copper baſon, and mixed with 100 pounds of river water, as at firſt, and again concentrated in the retorts till it becomes perfectly clear. Sulphur has been afforded in ſome inſtances by the ſecond rectification. The liquor which diſtills is received in the cups as before, and the acid in the retorts is then ſufficiently pure: that is to ſay, it is purified from all volatile matter. The lead and neutral ſalts ſtill remain combined with the acid, but fortunately they can in no reſpect injure the purity of the marine acid.

This concentrated acid exhibits 68 degrees by the hydrometer, or ſpecific gravity 1.896. It ſtill contains a portion of gas, but ſo ſmall in quantity as not to injure the purity of the marine acid, to which it only gives the property of cryſtalliſing when the temperature of the air is near the freezing point.

During the rectification of this acid, what firſt comes over is mere water, and muſt be thrown away; but that which ſucceeds is the aqueous acid. If this be ſet apart, and concentrated, a conſiderable quantity of vitriolic acid is obtained of the greateſt purity. As it has been carried over in diſtillation, it contains no foreign matter.

The author attempted, but in vain, to diſſipate the nitrous acid from the acid of vitriol by ebullition in an open veſſel without concentration. The experiment was made with 50 pounds of common vitriolic acid and 60 of river water. This was kept boiling in the copper baſon for four days, water being added from time to time to ſupply the loſs by evaporation. The copper baſon, by weighing before and after the operation, had loſt by ſolution no more than ten drams of copper. The acid was blue, but became white as uſual during the rectification in the retorts. From this experiment, as the author obſerves, it is ſeen not only that the nitrous acid cannot be diſſipated by ſimple ebullition without concentration, but that the action of the vitriolic acid upon copper is extremely ſlight.

The marine acid is to be diſengaged from common ſalt by the application of this vitriolic acid in the uſual manner. But as M. Baumé's experience led him to various ſimple manipulations and remarks of importance, and more eſpecially as he conſiders the deſcription of this proceſs as part of the new art of bleaching ſilk, he has annexed it to his memoir.

The vitriolic acid obtained by the foregoing proceſs being too concentrated, muſt be diluted in the copper baſon as before with river water. It is convenient to add 18 ounces of water to each pound of the acid, becauſe the marine acid is not wanted in a ſtate of high concentration. This mixture ought to give 35 or 36 degrees by Baumé's hydrometer; which laſt anſwers to a ſpecific gravity of 1.333. When it is cold it may be preſerved in bottles for uſe.

In the next place, four pounds of marine ſalt dried, becauſe in that ſtate it pours beſt, is to be put into a retort of the capacity of five or ſix French pints, or Engliſh quarts. This may be done by means of a paper funnel, or a long-necked funnel of glaſs, which muſt enter the body of the retort in order that the neck may remain clean. A number of theſe muſt be diſpoſed on a gallery in two oppoſite rows, with the necks properly encloſed and enveloped in ſand as uſual.

A bottle or gauge being provided of ſuch a ſize as by previous experiment is known to hold four pounds of the vitriolic acid before mentioned; this quantity of the acid muſt be meaſured into each of the retorts by means of a curved funnel, the tube of which may paſs into the body, to prevent the acid being ſpilled in the neck. If nevertheleſs a few drops ſhould fall, no inconvenience will follow, as this pure acid is not detrimental to the bleaching proceſs.

The ſupports for the receivers are then to be placed, and the receivers applied, each being pierced with a ſmall hole. The junctures are to be made good with paſted paper, and the diſtillation begun. A gradual heat is to be applied until the fluid boils gently. The marine acid which firſt riſes is volatile and expanſible *, and requires the ſmall holes of the receiver to be occaſionally opened; but after one fourth part of the time of diſtillation the acid comes over freely, and the vapours ceaſe to be elaſtic.

This diſtillation laſts two days; but it is practicable to avoid fitting up the intermediate night. The fire muſt be ſo managed that the contents of the retort may be very liquid in the evening: if it begins to thicken, there is reaſon to apprehend that it may be too hard the next day; in which caſe the heat will dilate the concrete matter before it liquefies, and break the containing veſſel.

Towards the cloſe of the diſtillation the matter ſwells up conſiderably. When this happens, it is proper to empty the receivers, and raiſe the retorts, that more ſand may flow in beneath them. When the matter is dry, and nothing more comes over, the operation is finiſhed.

Each retort affords five pounds of marine acid, of the ſtrength of 14 or 15 degrees; ſpecific gravity 1.114. When the retorts are half cooled, one pound of hot river water is to be poured into each, and the diſtillation being reſumed affords 24 ounces of the ſame marine acid from each retort.

It is remarkable, that in this proceſs ſome of the retorts afford the colourleſs and ſome the yellow acid; which is an object of no conſequence with regard to the bleaching. The author thinks the yellow colour is owing to a portion of ſulphur ſtill remaining in the vitriolic

---

* It might be of advantage, even in the large way, to adapt a ſimple pneumatic apparatus to condenſe the marine acid air in water, as is uſual in philoſophical proceſſes. N.

vitriolic acid. And if from curioſity the products of ſeveral of the retorts be received in eight different parts, it will be ſeen that in ſome the acid which paſſes firſt is the moſt concentrated, and gives 20 degrees by the hydrometer; that the products diminiſh ſucceſſively in the progreſs of concentration, till the laſt exhibits ſometimes no more than eight degrees; but that others afford the moſt concentrated acid at the beginning and end, while that in the middle of the diſtillation is the weakeſt. All theſe products mixed together afford a mean reſult of 14, 15, or 16 degrees*.

The hard compact ſaline matter in the retort conſiſts of much Glauber's ſalt, and a ſmall quantity of undecompoſed common ſalt. M. Baumé's method of extracting it is ſimple and ingenious. He fills the retort with water, corks it, and inverts it in an open veſſel alſo containing water, over which is fixed a board with holes for receiving the neck of the retort. It is proper that the ſaline maſs ſhould be detached, which ſoon happens in the filled retort, and ſuffered to ſlide down towards the neck before it is placed in the hole of the board. The cork is then taken out, and by that means the water in the retort communicates with that of the open veſſel. As the ſalt diſſolves, the brine flows down, and is replaced by purer water from below, which from its leſs denſity riſes to the uppermoſt place in the retort. In this way the evacuation is made in two days without trouble, which could not ſafely be effected in eight or ten days by ſucceſſive filling and emptying the retorts.

M. Baumé concludes his memoir by deſcribing the method of giving a bright yellow to ſilk, whether raw or bleached. For this purpoſe ten gros or drams of nitrous acid are to be mixed with one pound of alcohol, and into this a few ounces of ſilk are to be immerſed, and kept on the water bath at between 30 and 40 degrees of Reaumur, or 100° and 130° of Fahrenheit. The ſilk acquires a tarniſhed brown colour, and muſt be cleared of its acid by waſhing in ſeveral waters, and afterwards ſcoured with ſoap in the uſual manner. When thus cleaned and dry, it has the appearance of gold threads when ſeen in the ſun's light. Different ſhades may be given by keeping it a ſhorter time in the acidulated ſpirit; all which are equally permanent, and reſiſt waſhing and every other teſt. The author propoſes them to be uſed in articles of furniture wrought in deſigns which require light and ſhade.

MATHEMATICAL CORRESPONDENCE.

IN order to accommodate ſuch mathematical correſpondents as may reſide at a diſtance from the metropolis, and are diſpoſed to contribute to this part of the work, the ſolutions to the queſtions propoſed in any number of the Journal will be uniformly given the ſecond month after their publication; but it is requeſted that they may be ſent as early as poſſible, that proper time may be allowed to prepare them for inſertion.

QUESTIONS propoſed for Solution.

QUESTION III. *By ANALYTICUS.*

IT is required to determine the odds againſt the dealer, at the game of whiſt, having all the thirteen trumps in his own hand.

---

* For the correſpondent ſpecific gravities ſee p. 39 of this Journal.

QUESTION IV. *By J. B.*

GIVEN the time in which mercury is raiſed to the boiling point by the heat of a furnace, and the rate of cooling per minute, after it has been removed, to determine the heat of the furnace, or that to which the mercury has been expoſed.

SCIENTIFIC NEWS.

THE particular memoirs contained in the four firſt cahiers of the Journal of the Polytechnic School at Paris are thirteen in number *: two on ſtereotomy; one on fortification; ſix on chemiſtry; three on general phyſics; and one on the application to the arts.

The memoirs of ſtereotomy treat: 1. On the determination of tints in deſigns, by ſeveral pupils of the ſchool; and 2. On the curve lines of the ſurface of the ellipſoid, by Monge.

The ſolution of the intereſting problem of the determination of tints is deduced from the leſſons which Monge has given upon perſpective. This ſolution depends on the fundamental principle, that all bodies reflect the white rays; and that the quantity of rays of this colour, reflected from each point of a body, depends on its poliſh, and the angles formed by the incident and reflected rays which come to the eye; that the more white rays are reflected, the more luminous the body will appear; and on the contrary, the leſs of theſe rays are reflected, the more obſcure it will be.

All poliſhed bodies preſent a white ſpot or line; the line is viſible upon cylinders, cones, &c.; the point, upon ſurfaces of double curvature.

When bodies are perfectly poliſhed, a white point or determinate ſurface is perceived; but when they are obſcure, a ſucceſſive degradation of tints is ſeen, which depends on the form of the body.

From the point, the line, or the moſt enlightened ſurface, it is poſſible to trace a ſucceſſion of curves of equal tint: theſe are the curves of which the authors of this memoir have in the firſt place determined the equation, and afterwards ſought to aſcertain the law of the degradation of the tints.

If it be aſſumed that any tint laid on white paper will poſſeſs an intenſity dependent on the quantity of colour ſpread, and the proportion of white points left uncovered, the authors of the memoir have ſought to determine whether the law of the application of tints upon each other ought to depend on their degradation: by this means they have ſucceeded in giving the theory of waſhed tints, properly ſo called.

The ſolution of this problem, ſo intereſting to thoſe who cultivate the art of deſign, and are capable of purſuing the calculation it demands, proves the extended knowledge to which the pupils have arrived, and the progreſs they daily make, under their Inſtitutor.

The memoir on the curve lines of the ſurface of the ellipſoid, which Monge has given in the ſecond cahier of the Journal Polytechnique, contains an application of the property of curve ſurfaces with regard to their curvature, to the configuration of ſtones for arches.

The joints of arch-work ought to be made agreeable to a number of conditions; the chief of which are: " 1. They muſt be everywhere perpendicular to the arch, in order that the angles of two contiguous key-ſtones being reſpectively equal, they may alike reſiſt diſunion by the action they exerciſe upon each other. 2. They muſt be perpendicular between the ſtones, for the ſame reaſon. 3. They muſt be generated by the motion of a right line; for the ſurfaces generated in this manner are alone ſuſceptible of being accurately wrought; and it is neceſſary that the joints of the contiguous ſtones ſhould be perfectly well executed, becauſe very ſlight irregularities would produce a rupture of the arch."

---

* This Analyſis is tranſlated from the Journal des Sçavans, p. 117.

All theſe conditions may be obtained by dividing the curved ſurfaces of a vault by lines drawn from the one to the other of the two curves; and formed by the movement of a point of the ſurface which meets a ſeries of normals placed in the ſame direction. The diſtance between theſe lines muſt be a finite quantity, dependent on the nature of the materials.

"The operations of artiſts having been conſtantly directed to this general ſolution, they have obtained it only for the moſt eaſy caſes of cylindric and conical ſurfaces, and ſuch as are generated by the revolution of a plane. But with regard to curved ſurfaces of which they know not the lines of curvature, they almoſt generally excluded them from the conſtruction of arches, even when circumſtances urgently demanded them; and it is principally to this that we are to attribute the bad effect generally produced in architecture by leaning arches, or portions of arch-work (les morceaux de trait de coupe des pierres), becauſe, in order to render this practicable, a ſurface or curvature is choſen for the arch which is not always ſuch as the nature of things demands."

The example choſen by Monge for the application of his principles, is apparently the moſt happy he could have ſelected in the preſent ſituation of the French government, in which architects are buſied in the conſtruction of halls deſtined for the ſittings of the Legiſlative Council. Now the form moſt favourable to the diſtribution of the members of a deliberative aſſembly, and the ſituation of the orator, is the elliptic curve commonly called an oval. From the projection of theſe curves, traced on the deſigns at the end of this memoir, it may be ſeen what agreeable and elegant forms are thus produced; and how eminently the architect may avail himſelf of the principles here eſtabliſhed, even for the objects of decoration. Students in architecture will find this memoir highly deſerving of their peruſal. It will more and more convince them of the immenſe reſources afforded by the ſtudy of ſtereotomy for the ſolidity of conſtruction, and even for beauty of new forms, afforded by this theory to the ſyſtem of ornament.

The art of ſecuring thoſe who defend a rampart from the ſtroke of balls and ſhot is known in French by the term *defilement*. Say, aſſiſtant profeſſor in the Polytechnic School, has given a memoir on this ſubject in the fourth cahier of the Journal of that inſtitution.

He ſhews in the firſt place, that the outline of the defilement is the ſame as that of a ſhadow afforded by a row of luminous points ſituated in the ſpace from which the defenders of a fortification may be fired on.

He divides his ground into two parts: the exterior ſpace in which the attacking party may be placed, and the interior ſpace which is to be defended.

He examines the art of ſecuring a work (de filer) in two different caſes: 1. That in which the parapet is determined. 2. That in which the ground plan is given, but not abſolutely the relief or profile. To theſe two caſes he adds certain rules for tracing fortifications, deduced ſingly from conſiderations relative to their defilement.

Theſe three queſtions are preceded by ſome preliminary notions; and by the method in which Say has treated his ſubject, we have acquired in this memoir a more complete treatiſe than any which has yet appeared on defilements. He has the merit of having fixed with clearneſs and preciſion, a ſeries of principles which hitherto have been only tranſmitted in a fugitive and as it were traditional manner, in the School de Mezières, appropriated to the inſtruction of the *élèves du génie*, and deſtined again to receive them, according to the meſſage long ago ſent from the Government to the Legiſlative Body.

The ſix memoirs of chemiſtry are: 1. Deſcription and uſe of an eudiometer of ſulphate of pot-aſh (liver of ſulphur), by Guyton. 2. Obſervations on the eudiometric properties of phoſphorus, by Berthollet. 3. Analyſis of the calcedony of Creuſot, by Guyton. 4. Experiments on the fuſibility of earths, and their habitudes with ſaline fluxes, together with the ſolvent action they exert on each other, by Guyton. 5. Experiments on the formation of the colouring pruſſic principle, by Bonjour. 6. The properties of the ſulphureous acid, and its combination with earths and alkaline baſes, by Fourcroy and Vauquelin.

[*To be continued.*]

NEW PUBLICATIONS.

Travels in Hungary, with a ſhort Account of Vienna in the Year 1793. By Robert Townſon, LL. D. F. R. S. Edin. &c. &c. Illuſtrated with a map and ſixteen other copper-plates. 4to. 506 pages including the Index. Printed for Robinſon. Price 1l. 1s.

To thoſe who attend chiefly to diplomatical politics, or the balance of power, the kingdom of Hungary will probably afford an object of ſubordinate value; but on the larger, more extended and important conſiderations of internal government, political economy, and the ſtate of man with regard to ſcience and manners in the progreſs of civilization, it will prove highly intereſting. Mr. Townſon has publiſhed the preſent work from the corrected notes of a five-months' tour; in which theſe and other objects of utility and entertainment have engaged his attention. As I hope ſhortly to give a fuller account of this work, the preſent notice is intended only to announce the publication of a valuable and entertaining book.

The Hiſtoire Naturelle of Valmont de Bomare, rangée par ordre de matieres par L. Blondelin of the Univerſity of Bâle, ornamented with coloured plates engraved by J. J. de Méchel, was announced for publication in the middle of February laſt, in the Decade Philoſophique, &c. It will be printed at Bâle; and the Quadrupeds were then ready for the preſs. It will amount to about 150 ſheets, or 6 or 7 volumes in octavo, with at leaſt the ſame number of plates. One number, containing ſeven ſheets and eight plates, will appear every ſecond decade, or one volume of three numbers every two months. The price four livres per number, with the figures plain; or ſix, if coloured. The ſubſcribers to the firſt 500 copies will have the advantage of a deduction of one-fourth. The plates may be had at the price of two livres, plain; or ſix, coloured.

Subſcribers pay in advance for one number or volume; and the reſt, on delivery, to Cit.

Fuchs, bookſeller, rue des Mathurins, hotel de Cluny, à Paris ; and at Bâle, by Cit. L. Blondelu (perhaps Blondelin as above), agrégé à l'Univerſité.

Refutation de la Théorie Pneumatique, &c. A Refutation of the Pneumatic Theory, or the New Doctrine of the Modern Chemiſts, preſented Article by Article, in a Series of Replies to the Principles collected and publiſhed by Cit. Fourcroy in his Philoſophie Chimique: to which is prefixed, a Complementary Supplement to the Theory exhibited in a Work entitled *Recherches ſur les Cauſes des Principaux Faits Phyſiques ;* or, Reſearches into the Cauſes of the Principal Phyſical Facts, to which this forms a neceſſary Continuation. By J. B. Lamarck, of the National Inſtitute of France. Octavo, 481 pages. Publiſhed at Paris, by the Author, at the Muſeum of Natural Hiſtory ; and by Agaſſe, rue des Poitevins. L'an 4.

I have not yet procured the *Recherches* here mentioned ; but, when in poſſeſſion of both, it will become an object of careful enquiry, whether an abſtract of the leading points of difference in this author's theory and that which he attempts to refute may be of value to the ſcientific world.

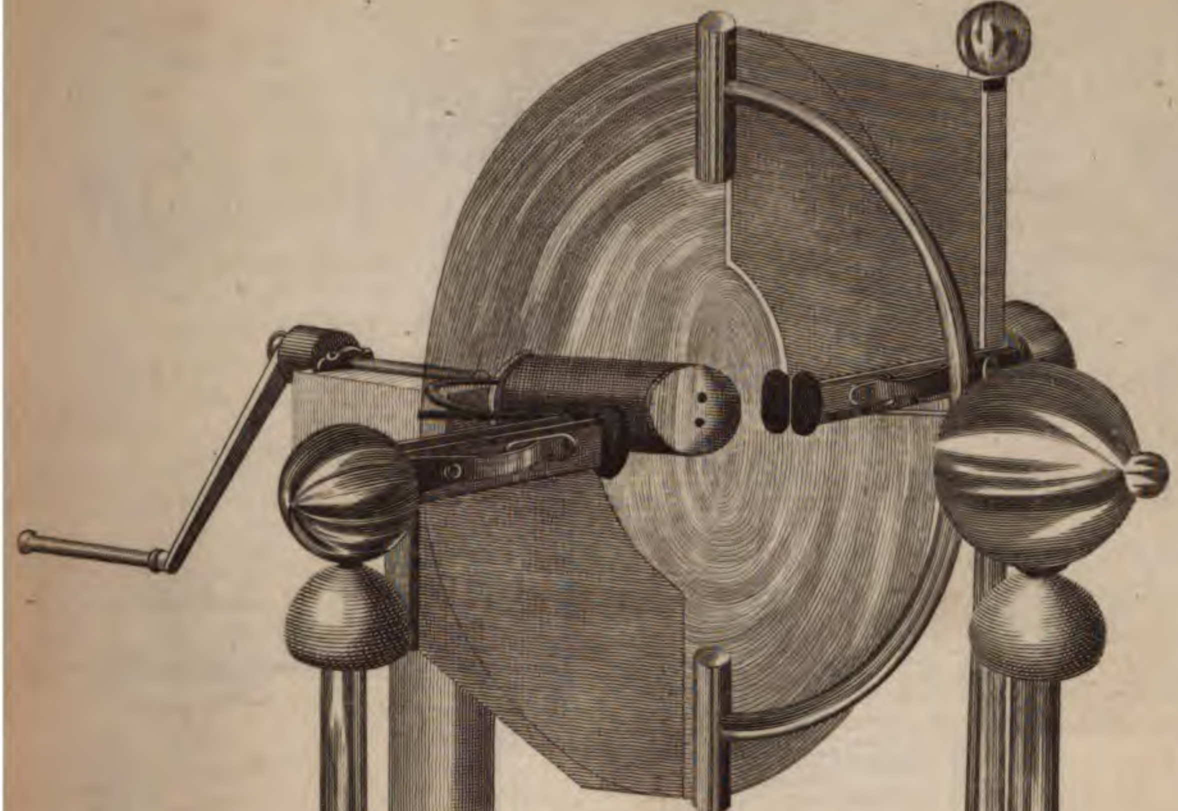
Specimens of Britiſh Minerals ſelected from the Cabinet of Philip Raſhleigh, of Menabilly, in the county of Cornwall, Eſq. M. P. F. R. S. and F. A. S. with general deſcriptions of each article : 4to—56 pages—33 plates coloured. Nicol and White. Price 2l. 12s. 6d.

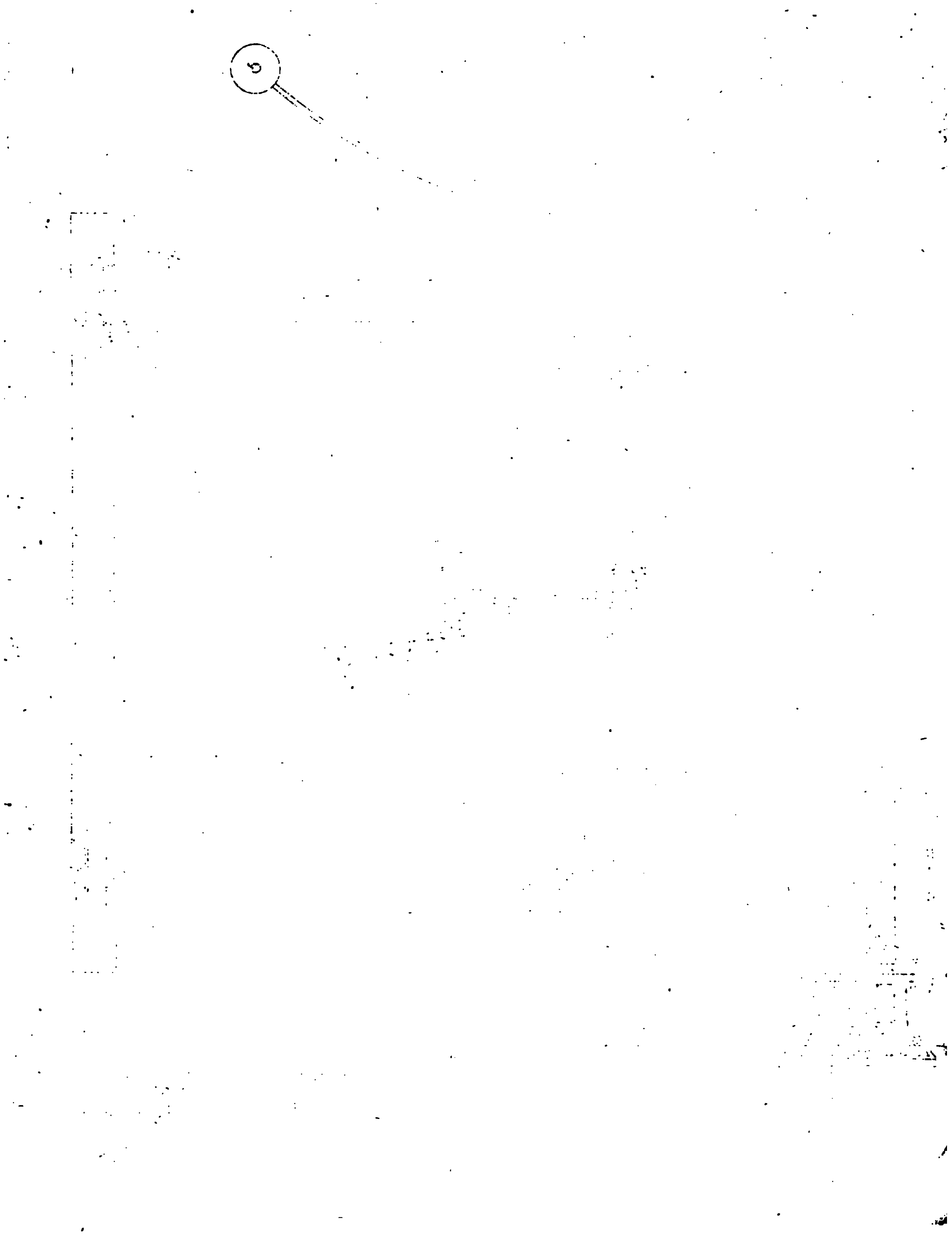
This magnificent work diſplays with great effect the ſpecimens, chiefly conſiſting of tin, copper and lead ores, with calcareous ſpars and quartz. The deſcriptions are intended to complete the knowledge, in part and very ſtrikingly, conveyed by the plates. Little is ſaid in general concerning analyſis, or the component parts.

Cit. Prony, of the French National Inſtitute, has publiſhed the ſecond part of the *Nouvelle Architecture Hydraulique*. This part contains the deſcription of ſteam engines. It is ſold by Firmin Didot, No. 116, rue de Thionville. Quarto, 240 pages, with 40 plates. Price 40 liv.

M. Lalande, in the Journal des Sçavans page 125, gives the following account of this work: " Since the appearance of the firſt part of the Architecture Hydraulique in 1790, the public has waited with impatience for the continuation. This part is a complete Treatiſe on Steam Engines. It contains new experiments on the expanſive power of heat ; an account of pumps which have the piſton acted on by the ſteam alternately on both ſides (pompes à double effet), and thoſe which are ſimple ; inſtructions for diſpoſing the latter to act in the manner of the former ; the theory of the right-lined motion of the piſton by a combination of circular movements ; and a method of interpolation applicable to phenomena which depend on elaſtic fluids. The analytic part of this work is no leſs curious than the mechanical and experimental parts. No one but a geometer could have treated theſe ſubjects in a manner ſo perſpicuous for the learned ; nor could a leſs able engineer have rendered them ſo ſatisfactory to practical men."

Citizen Garnier, Profeſſor of Mathematics, has added ſeveral uſeful explanations of parts of the firſt volume of Citizen Prony, which render this work ſtill more complete.




6

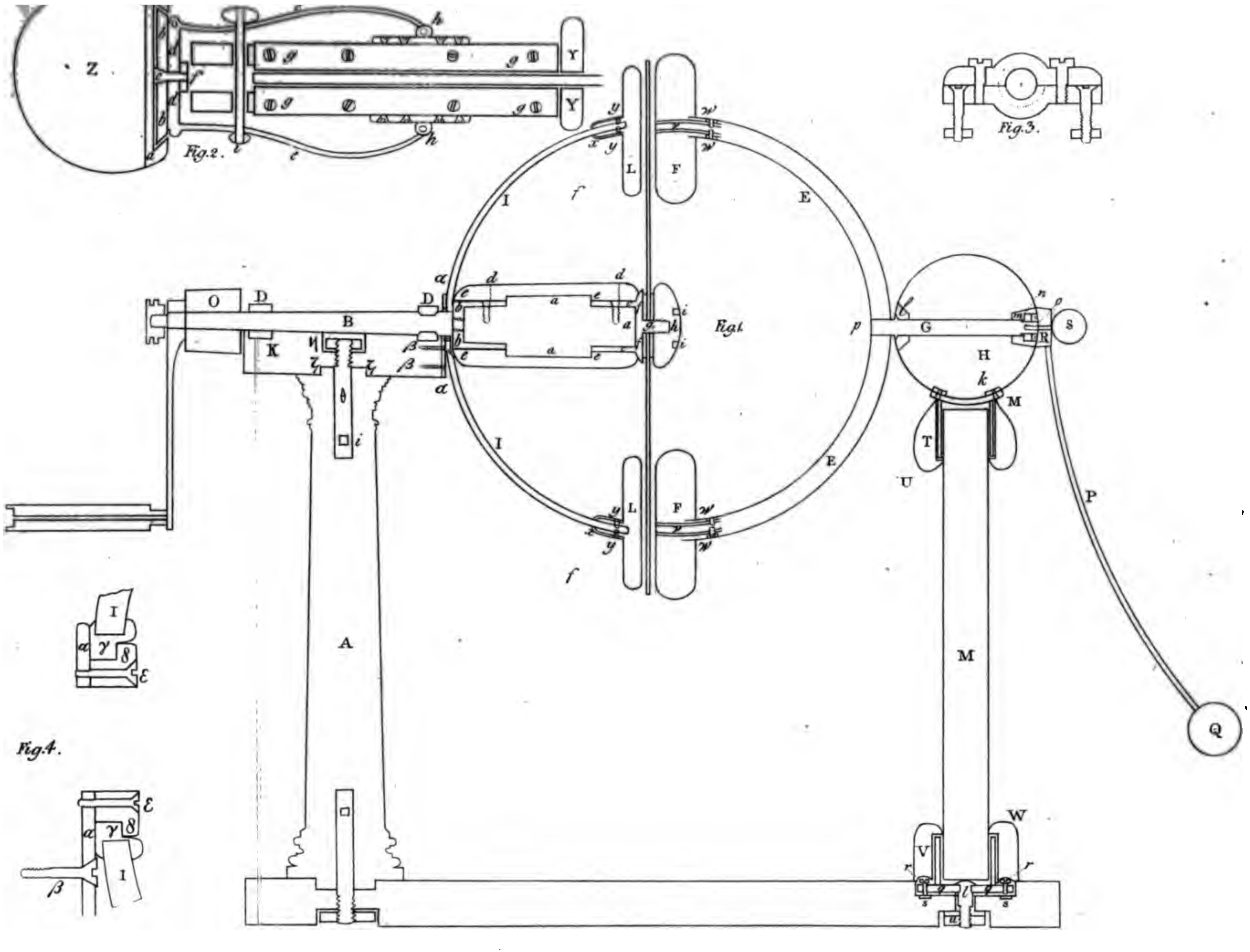
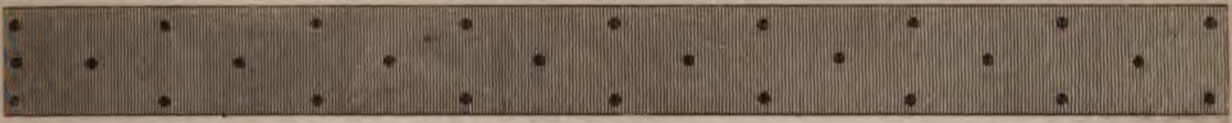


Fig.1.
Fig.2.
Fig.3.
Fig.4.
A
B
D
K
O
E
F
G
H
I
L
M
P
Q
R
S
T
U
V
W
Y
Z
a
b
d
e
f
g
h
i
k
l
n
o
p
r
s
t
u
w
x
y
α
β
γ
δ
ε
ζ
η
θ

Fig. 1.



Flexure of metallic bars by Heat.

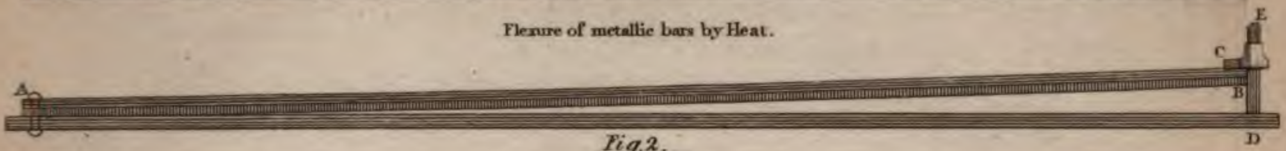


Fig. 2.

Expansion Balance.

Fig. 3.

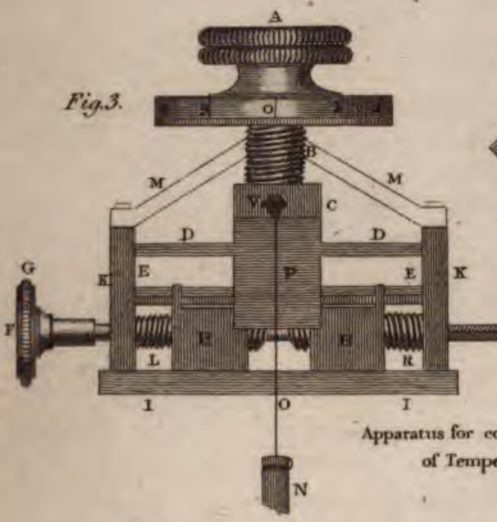


Fig. 10.

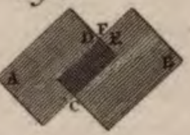
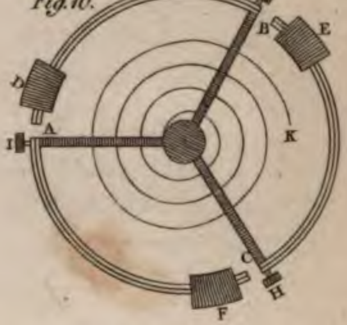


Fig. 10.



Apparatus for correcting the effects of change of Temperature in Pendulums.

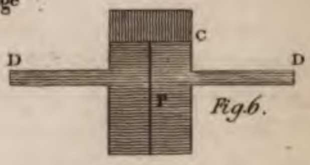


Fig. 6.

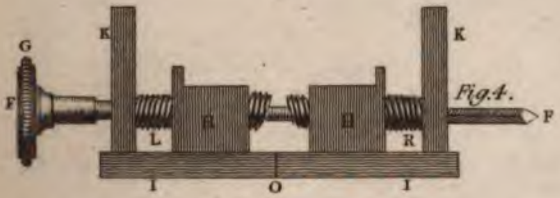


Fig. 4.

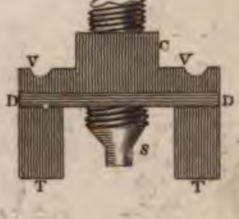


Fig. 7.

Compound bar

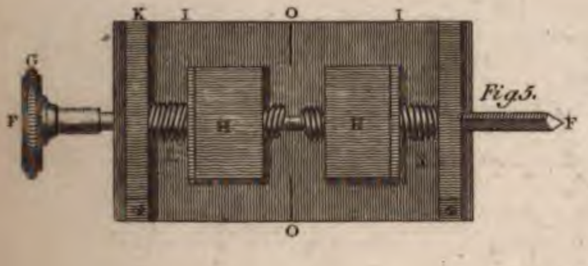


Fig. 5.

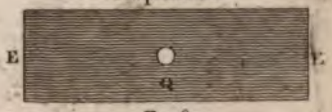


Fig. 8.

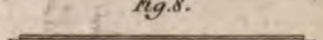


Fig. 9.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JUNE 1797.

ARTICLE I.

A Letter from M. de HUMBOLDT *to M.* PICTET, *on the Magnetic Polarity of a Mountain of Serpentine* [*].

AT the beginning of the eighteenth century the attention of natural philoſophers was entirely fixed on the phenomena of magnetiſm. The progreſs which has ſince been made in the theory of electricity, and the preponderance which chemiſtry has acquired over all the other branches of natural hiſtory, have diminiſhed the intereſt with which enquiries into the nature of the magnetic fluid ought to have been purſued. It is true that your celebrated countrymen Meſſrs. de Sauſſure and Prevôt have given vigour to this purſuit by diſcoveries worthy of their ſagacity: the firſt, by inventing an inſtrument capable of meaſuring the comparative intenſity of the magnetic forces in different regions of the globe; and the other, by reducing the laws of polarity to the ſimple laws of attraction. But theſe diſcoveries have not afforded inducement ſufficient to lead philoſophers into a path ſo honourably explored. The moſt valuable work on the origin of magnetic forces has been neglected, together with the calculations of the ingenious Coulomb, and his experiments with the balance of Torſion.

Having traverſed with the compaſs in my hand great part of the mountains of Europe, I became convinced that declinations cauſed by maſſes of iron in beds or in veins are infinitely leſs frequent than naturaliſts affirm. The obſervations which Meſſrs. de Sauſſure and Trembley have made on the ſummit of Cramont [†], appear to me the more curious, as it ſtands alone, and preſents to our knowledge a very extended image of the dimenſions of magnetic ſpheres. It is among the Alps of Sweden and Norway, thoſe northern regions which nature has enriched with an enormous depoſit of iron leſs oxided than in our country, that we were entitled to expect ſimilar phenomena.

---

[*] I received this communication in manuſcript from the Right Hon. Sir Joſeph Banks, Bart. P. R. S. &c. It is written in French, under the title of "Lettre quatrième de M. de Humboldt à M. Pictet, ſur la Polarité Magnetique d'une Montagne de Serpentine." The ſame liberal promoter of ſcience has favoured me with a ſpecimen of this rock, with permiſſion to make experiments upon it. A few obſervations on this ſpecimen are added at the end of this memoir. N.

[†] Voyage dans les Alpes, T. I. p. 375.—T. II. p. 343.

I haſten to communicate to you a diſcovery I made in the month of November, and which appears to me of conſiderable importance in the progreſs of geology. You are acquainted, Sir, with the laws and the harmony which I have obſerved in the direction and inclination of the primitive ſtrata, from the banks of the Mediterranean to thoſe of the Baltic Sea. You have even condeſcended, jointly with our friend Dolomieu, to expreſs an intereſt with regard to this laborious undertaking; which, in more ſkilful hands than mine, would, I am well aſſured, throw great light on the conſtruction of the globe. I traverſed the chain of mountains of the High Palatinate and the margraviate of Bayreuth; and I found, in the bottom of the Fichtelgebirge, between Munichberg and Goldcronach, an iſolated hill, which riſes to the elevation of fifty toiſes above the ſurrounding plain. Its height above the level of the ſea may be eſtimated at two hundred and eighty, or three hundred, toiſes. This hill extends in length from weſt to eaſt, and forms a pyramid extremely obtuſe. The rocks which crown the ſummit or ridge are compoſed of ſerpentine of conſiderable purity, which, by its colour and foliated fracture, approaches in various parts to the chlorithſchiefer of Werner *(ſchiſtous chlorite)*. This ſerpentine is divided into ſtrata rather diſtinct, of which the inclination to the north-weſt preſents an angle between 60 and 65 degrees. It repoſes on a foliated granite, mixed with hornblende; a mixture which we diſtinguiſh by the name of ſyenite. I approached this ſerpentine with the compaſs, in order to determine more accurately the angle it formed with the meridian. The magnetic needle was in a ſtate of continual agitation. I advanced two ſteps farther, and beheld that the north pole was entirely turned to the ſouth. I called two friends, Meſſrs. Godeking and Killinger, who aſſiſted me in my geological purſuits; and we were alike penetrated with that joy which the contemplation of intereſting phenomena produces in the minds of thinking men. I ſhall not detain your attention by a full recital of our obſervations; but ſhall merely preſent the reſults, to which I may hereafter make additions, if my occupations ſhould not lead me from this part of Germany.

The action of this mountain of ſerpentine upon the magnet ſhews itſelf in a very curious manner. The uncovered rocks which are ſeen on the northern ſlope, and thoſe on the declivity towards the ſouth, have poles directly oppoſite. The former exhibit only ſouth poles, and the latter north poles. The whole maſs of foliated ſerpentine does not therefore poſſeſs a ſingle magnetical axis, but preſents an infinity of different axes perfectly parallel to each other. This paralleliſm alſo agrees with the magnetic axis of the globe, though the poles of the ſerpentine are inverted; ſo that the northern pole of the hill is oppoſed to the ſouth pole of the earth. The eaſt and weſtern ſlopes preſent what in the theory of magnetiſm would be called points of indifference. The magnet does not at this part appear to be in any reſpect affected, though the ſubſtance of the rock differs in no external character from the other parts. It is the ſame on the ſouth ſide of the ſummit *. I have obſerved not only that the magnetic axes are not diſpoſed in the ſame horizontal plane; but I have likewiſe remarked, that two points, of which the action is very ſtrong, are joined by rocks which do not exert the leaſt attraction. The chemical analyſis of theſe compounds affords the ſame reſults; and it would be no leſs difficult to diſcover any difference of aggregation between them, than between iron which has received the touch, and other iron which had never acquired the magnetic power.

---

* I ſuſpect an error of the copyiſt, as the words "Il en eſt de même du côté meridional de la ſommité" contradict the general deſcription immediately preceding. N.

On this occaſion a queſtion preſents itſelf which cannot be reſolved in leſs than half a century. The tables founded on the obſervations of Picard, La Hire, Maraldi, Caſſini, and Le Monnier, ſhew that the needle has declined ſince 1660 towards the weſt; and that this declination continues to increaſe, though the oſcillations cauſed by the heats of the ſouth, and the temperature of the ſeaſons, often produce a retrograde courſe. If the magnetic axis of our mountain were aſtronomically determined by the culmination of the ſtars, whether its direction would remain the ſame until the year 1850, or whether its ſouth-pole would turn towards the weſt, in connection with the variation of the magnetic needle? From our profound ignorance of the cauſes of the declination, as well as of moſt geological phenomena, it is not in our power to reſolve ſo complicated a problem.

Other obſervations equally intereſting may be made on the identity of magnetic forces. I have diſcovered a maſs of rocks which affect the needle at the diſtance of twenty-two feet. With an apparatus ſimilar to the magnetometer of M. de Sauſſure, we might obſerve whether the intenſity of the forces of magnetic action remains the ſame in winter and in ſummer; whether it be ſtronger in the morning, at noon, at the ſolſtices, during the aurora borealis, or in an atmoſphere loaded with electric fluid? I ſuppoſe that theſe ſame rocks might act on the needle ſometimes at 16, and ſometimes at 28 feet diſtance.

It has been obſerved, that metals expoſed to the air gradually imbibe the magnetic fluid. A ſlight oxidation of the iron ſeems to favour this effect. I have myſelf obſerved, that in a magnetic bed of iron thoſe parts only which were in contact with the air affected the needle. This phenomenon is conſidered as the effect of atmoſpheric electricity. I am aware that lightning converts a bar of iron into a magnet; that the diſcharge of the Leyden vial ſometimes increaſes the intenſity of magnetic forces; but I do not ſee why the atmoſpheric electricity ſhould act ſimply on the external ſurface of a bed of magnetic iron, which is a good conductor of the electric fluid. Does not the oxygene of the atmoſphere rather act a part in this operation? Without wandering in the ſphere of probabilities, I have choſen to adhere to enquiries reſpecting facts. I have obſerved the rocks which were covered with turf, from which I detached pieces that had not been in contact with the air. I found that the magnetic force was conſtantly the ſame.

The mountains of the Harz preſent a granite rock called the ſchnarcher, which is elevated in the form of a tower, or broken pyramid. This granite likewiſe affects the needle; but it acts only in the maſs, and in a ſingle band or perpendicular vein. Detached pieces ſhew no action upon the needle. It is to Mr. de Trebra, celebrated for his reſearches concerning the internal parts of mountains, that we are indebted for this important diſcovery.

Some philoſophers pretend that the ſchnarcher contain in their bowels a maſs of magnetic iron; others preſume that a ſtroke of lightning has cauſed the magnetic vein in theſe mountains.

The nature of the rocks which I have the honour to preſent to your notice in this paper does not admit of ſimilar explanations. The ſerpentine not only acts in a maſs, in its natural

tural ſituation, but all the pieces, when broken, to infinity, ſtill exhibit two very diſtinct poles. Pieces of five inches diameter act on the needle at the diſtance of half a foot. The examination of the magnetic axes affords an object of curious enquiry. They are moſtly found in a direction parallel to that of the foliated grain; nevertheleſs, I have found ſome which croſs it perpendicularly. Fragments extremely ſmall, of the magnitude of 0.01 of a cubic line, ſhew a very ſtrong polarity in proportion to their maſſes. You ſee them turn very ſuddenly when the poles of the weakeſt magnet are ſucceſſively preſented to them. It is a very ſtriking phenomenon, that a ſtone poſſeſſed of ſo high a degree of polarity ſhould exhibit no attraction for iron which is not magnetiſed. I have never obſerved the ſmalleſt particle of filings of iron adhere to the ſerpentine; but the ſerpentine, reduced to powder, attaches itſelf very readily to the magnet.

You will enquire with impatience, if it be well proved that my ſerpentine is not mixed with magnetic iron; whether this mixture may not be ſufficiently intimate to enter into the compoſition of each particle of the rock? I can aſſure you, that I have made the moſt aſſiduous enquiries in this reſpect. All my experiments were made in conjunction with Mr. Godeking, whoſe knowledge and abilities are a ſufficient aſſurance againſt error; but we were decidedly convinced, that if the magnetic force cannot adhere to the earthy ſubſtances which form the baſe of the ſerpentine, it can be attributed only to the oxide of iron with which it is coloured. Theſe are our reaſons: The rock has no mixture of metallic ſubſtances. It preſents only here and there a few fragments of talc or amianthus; but neither pyrites, nor ſchoerl, nor octahedrons of magnetic iron. When reduced to very fine powder, it reſembles pounded chalk. The microſcope diſcovers only earthy parts, of a clear whitiſh green. The ſpecific gravity of this ſerpentine is very ſmall. I find it only from 1.901 to 2.04 aſſuming water to be 1.0. There are not conſequently any minerals but pumice-ſtone, mountain-leather, and ſome varieties of the opal, which do not equal our ſerpentine in denſity. The chemical experiments we have hitherto made, prove that it contains, like the jade or lapis ollaris, oxided iron; but not iron capable of attraction by the magnet. The ſolutions in muriatic acid, mixed with the nitric acid, are yellow, and not green like thoſe which the micaceous iron, and all the ores which contain pure or metallic iron, afford.

Here then is a very ſtriking phenomenon, namely, the polarity of ſuper-oxigenated iron. We learn by the valuable experiments of my celebrated countrymen Klaproth and Wenzel, that pure nickel and cobalt are attracted by the magnet; we know that iron ſlightly oxided (the black oxide) is alſo affected; but how great the difference between this ſtate of oxidation, and that of the iron which colours the ſerpentine, various calcareous ſtones, and perhaps even certain vegetable matters! What difference between a ſubſtance which acts alike on the two extremities of the needle, and a ſtone of which the ſmalleſt portions exerciſe a ſpontaneous polarity! Let us purſue the path of obſervation; let us collect indubitable facts. By this method the theories of natural philoſophy will be eſtabliſhed on ſolid and durable foundations.

Obſervations on the Stone which was forwarded to Sir JOSEPH BANKS *with the preceding Memoir.*

DESCRIPTION.—IT is of a blueiſh opake black colour, every where interſperſed with minute particles of a yellowiſh rather ſilky white, and of no regular figure. No appearance of ſymmetry or cryſtallization preſents itſelf, except on one ſide, where a rough indication of laminæ is ſeen. This is in the direction of the magnetic poles. The tenacity is very conſiderable, as it was not broken but by a violent blow on a round flint. Hardneſs between 6 and 7 of Kirwan; that is to ſay, it yields to the knife and file, but greatly injures thoſe inſtruments. Powder white, or greeniſh-white. Fracture coarſe, earthy. Fragments rather angular. No ſmell, nor adheſion to the tongue; but when two pieces are ſtruck together its ſmell is the ſame as is given in like circumſtances by rock cryſtal. Under the hammer it gives fire, but rarely with the ſteel. Specific gravity 2.91. Does not imbibe water. When touched with nitrous acid, a very ſlight effervescence took place, which was general and diſtinct, but ſcarcely viſible without the magnifier. When ſtrongly urged with the blow-pipe, it acquired an irregular light-brown colour, and was much leſs tenacious, but ſuffered no other change. Acid of borax and microcoſmic ſalt diſſolve it with little effervescence, and very ſlowly. The glaſs of the former is clear green, the latter clear white.

Its whole weight was 8238 grains; and that of the piece broken off for experiment, without the contact or uſe of iron, was 614 grains. It weighed 403 grains in water, and conſequently loſt 211 grains. Hence $\frac{614}{211} = 2.91$, or the ſpecific gravity to water aſſumed as 1.00. This differs from the reſults found by Mr. Humboldt.

The fracture was made acroſs the line of magnetical direction. The ſurfaces which had been thus ſeparated, exhibited oppoſite poles, in the ſame manner as when any other natural magnet is broken. The ſmaller piece was then broken into many fragments, and in part pulveriſed. All the pieces were poſſeſſed of polarity. A ſmall five-bar horſe-ſhoe magnet took up the ſmaller fragments, and the powder, though weakly, as they were eaſily ſhaken off. I could not ſatisfactorily aſcertain that any part of the ſtone did either attract the fineſt iron filings, or influence their arrangements when laid upon paper.

A very delicate magnetic needle, 1¾ inches long, moving on an agate ſocket, was ſuffered to diſpoſe itſelf in the magnetic meridian. The larger piece was preſented due eaſt from the centre, and then moved upon its own centre, to find the poſitions of the greateſt attraction and repulſion. At the diſtance of 18 inches from the middle of the ſtone, the needle was perceptibly affected; but its greateſt deviation did not exceed ten minutes of a degree. At 12 inches the deviation was one degree, and at ſix inches it was about 14 degrees. By application of the ſtone cloſe to the compaſs-box, the needle was led into any poſition at pleaſure.

It was found on compariſon with various pieces of natural magnet, that its directive force or effect on the needle is much weaker than that of any of the pieces. But whether a natural magnet as weak in directive power as this ſtone would be equally inactive with regard to iron filings, does not appear from the facts I have hitherto obſerved.

II.

An Account of ſome Experiments upon Coloured Shadows. By Lieutenant General Sir BENJAMIN THOMPSON, *Count of Rumford, F. R. S. In a Letter to Sir* JOSEPH BANKS, *Bart. P. R. S.*[*]

DEAR SIR,

SINCE my laſt letter, being employed in the proſecution of my experiments upon light, I was ſtruck with a very beautiful, and what to me appeared to be a new appearance. Deſirous of comparing the intenſity of the light of a clear ſky by day with that of a common wax-candle, I darkened my room, and letting the day-light from the north, coming through a hole near the top of the window-ſhutter, fall at an angle of about 70° upon a ſheet of very fine white paper, I placed a burning wax-candle in ſuch a poſition that its rays fell upon the ſame paper, and, as near as I could gueſs, in the line of reflection of the rays of day-light from without; when, interpoſing a cylinder of wood, about half an inch in diameter, before the centre of the paper, and at the diſtance of about two inches from its ſurface, I was much ſurpriſed to find that the two ſhadows projected by the cylinder upon the paper, inſtead of being merely ſhades without colour, as I expected, the one of them, that which, correſponding with the beam of day-light, was illuminated by the candle, was yellow; while the other, correſponding to the light of the candle, and conſequently illuminated by the light of the heavens, was of the moſt beautiful blue that it is poſſible to imagine. This appearance, which was not only unexpected, but was really in itſelf in the higheſt degree ſtriking and beautiful, I found upon repeated trials, and after varying the experiment in every way I could think of, to be ſo perfectly permanent, that it is abſolutely impoſſible to produce two ſhadows at the ſame time, from the ſame body, the one anſwering to a beam of day-light and the other to the light of a candle or lamp, without theſe ſhadows being coloured, the one yellow, and the other blue.

---

[*] From the Philoſophical Tranſactions, 1794, p. 107.

The experiment may very eaſily be made at any time by day, and almoſt in any place, and even by a perſon not in the leaſt degree verſed in experimental reſearches. Nothing more is neceſſary for that purpoſe than to take a burning candle into a darkened room, in the day-time, and open one of the window-ſhutters a little, about half or three-quarters of an inch for inſtance; when, the candle being placed upon a table or ſtand, or given to an aſſiſtant to hold, in ſuch a ſituation that the rays from the candle may meet thoſe of day-light from without at an angle of about 40° at the ſurface of a ſheet of white paper, held in a proper poſition to receive them, any ſolid opake body, a cylinder, or even a finger, held before the paper, at the diſtance of two or three inches, will project two ſhadows upon the paper, the one blue and the other yellow.

If the candle be brought nearer to the paper, the blue ſhadow will become of a deeper hue, and the yellow ſhadow will gradually grow fainter; but if it be removed farther off, the yellow ſhadow will become of a deeper colour, and the blue ſhadow will become fainter; and the candle remaining ſtationary in the ſame place, the ſame varieties in the ſtrength of the tints of the coloured ſhadows may be produced merely by opening the window-ſhutter a little more or leſs, and rendering the illumination of the paper, by the light from without, ſtronger or weaker. By either of theſe means, the coloured ſhadows may be made to paſs through all the gradations of ſhade, from the deepeſt to the lighteſt, and *vice verſa*; and it is not a little amuſing to ſee ſhadows thus glowing with all the brilliancy of the pureſt and moſt intenſe priſmatic colours, then paſſing ſuddenly through all the varieties of ſhade, preſerving in all the moſt perfect purity of tint, growing ſtronger and fainter, and vaniſhing and returning, at command.

With reſpect to the cauſes of the colours of theſe ſhadows, there is no doubt but they ariſe from the different qualities of the light by which they are illuminated; but how they are produced, does not appear to me ſo evident. That the ſhadow correſponding to the beam of day-light which is illuminated by the yellow light of a candle, ſhould be of a yellowiſh hue, is not ſurpriſing; but why is the ſhadow correſponding to the light of the candle, and which is illuminated by no other light than the apparently white light of the heavens, blue? I at firſt thought that it might ariſe from the blueneſs of the ſky; but finding that the broad day-light reflected from the roof of a neighbouring houſe covered with the whiteſt new-fallen ſnow produced the ſame blue colour, and, if poſſible, of a ſtill more beautiful tint, I was obliged to abandon that opinion.

To aſcertain with ſome degree of preciſion the real colour of the light emitted by a candle, I placed a lighted wax-candle, well trimmed, in the open air, at mid-day, at a time when the ground was deeply covered with new-fallen ſnow, and the heavens were overſpread with white clouds; when the flame of the candle, far from being white, as it appears to be when viewed by night, was evidently of a very decided yellow colour, not even approaching to whiteneſs. The flame of an Argand's lamp, expoſed at the ſame time in the open air, appeared to be of the ſame yellow hue. But the moſt ſtriking manner of ſhewing the yellow hue of the light emitted by lamps and candles, is by expoſing them in the direct rays of a bright meridian ſun. In that ſituation the flame of an Argand's lamp, burning with its greateſt brilliancy, appears in the form of a dead yellow ſemi-tranſparent ſmoke. How tranſcendently pure and inconceivably bright the rays of the ſun are when compared to the light of any of our artificial illuminators, may be gathered from the reſult of this experiment.

It appearing to me very probable that the difference in the whiteneſs of the two kinds of light which were the ſubjects of the foregoing experiments, might ſomehow or other be the occaſion of the different colours of the ſhadows, I attempted to produce the ſame effects by employing two artificial lights of different colours; and in this I ſucceeded completely.

In a room previouſly darkened, the light from two burning wax-candles being made to fall upon the white paper, at a proper angle, in order to form two diſtinct ſhadows of the cylinder, theſe ſhadows were found not to be in the leaſt coloured; but upon interpoſing a pane of yellow glaſs, approaching to a faint orange colour, before one of the candles, one of the ſhadows immediately became yellow, and the other blue. When two Argand's lamps were made uſe of, inſtead of the candles, the reſult was the ſame: the ſhadows were conſtantly and very deeply coloured, the one yellow, approaching to orange, and the other blue, approaching to green. I imagined that the greeniſh caſt of this blue colour was owing either to the want of whiteneſs of the one light, or to the orange hue of the other, which it acquired from the glaſs.

When equal panes of the ſame yellow glaſs were interpoſed before both the lights, the white paper took an orange hue; but the ſhadows were to all appearance without the leaſt tinge of colour; but two panes of the yellow glaſs being afterwards interpoſed before one of the lights, while only one pane remained before the other, the colours of the ſhadows immediately returned.

The reſult of theſe experiments having confirmed my ſuſpicions that the colours of the ſhadows aroſe from the different degrees of whiteneſs of the two lights, I now endeavoured, by bringing day-light to be of the ſame yellow tinge with candle-light by the interpoſition of ſheets of coloured glaſs, to prevent the ſhadows being coloured when day-light and candle-light were together the ſubjects of the experiment; and in this I ſucceeded. I was even able to reverſe the colours of the ſhadows, by cauſing the day-light to be of a deeper yellow than the candle-light. In the courſe of theſe experiments I obſerved that different ſhades of yellow given to the day-light produced very different and often quite unexpected effects: thus one ſheet of the yellow glaſs interpoſed before the beam of day-light changed the yellow ſhadow to a lively violet colour, and the blue ſhadow to a light green; two ſheets of the ſame glaſs nearly deſtroyed the colours of both the ſhadows; and three ſheets changed the ſhadow which was originally yellow to blue, and that which was blue to a purpliſh yellow colour.

When the beam of day-light was made to paſs through a ſheet of blue glaſs, the colours of the ſhadows, the yellow as well as the blue, were improved and rendered in the higheſt degree clear and brilliant; but when the blue glaſs was placed before the candle, the colours of the ſhadows were very much impaired.

In order to ſee what would be the conſequence of rendering the candle-light of a ſtill deeper yellow, I interpoſed before it a ſheet of yellow or rather orange-coloured glaſs; when a very unexpected and moſt beautiful appearance took place. The colour of the yellow ſhadow was changed to orange, the blue ſhadow remained unchanged, and the whole ſurface of the paper appeared to be tinged of a moſt beautiful violet colour, approaching to a light crimſon or pink; almoſt exactly the ſame hue as I have often obſerved the diſtant ſnowy mountains and valleys of the Alps to take about ſun-ſet. Is it not more than probable that this hue is in both caſes produced by nearly the ſame combinations of coloured light? In the one caſe it is the white ſnow illuminated at the ſame time by the pureſt light of the heavens, and by the deep yellow rays from the weſt; and in the other it is the white paper illuminated by broad day-light, and by the rays from a burning candle, rendered ſtill more yellow by being tranſmitted through the yellow glaſs. The beautiful violet colour which ſpreads itſelf over the ſurface of the paper will appear to the greateſt advantage if the pane of orange-coloured glaſs be held in ſuch a manner before the candle, that only a part of the paper, half of it, for inſtance, be affected by it, the other half remaining white.

To make theſe experiments with more convenience, the paper, which may be about eight or ten inches ſquare, ſhould be paſted or glued down upon a flat piece of board, furniſhed with a ball and ſocket upon the hinder ſide of it, and mounted upon a ſtand, and the cylinder ſhould be faſtened to a ſmall arm of wood or of metal, projecting forward from the bottom of the board for that purpoſe. A ſmall ſtand, capable of being made higher or lower as the occaſion requires, ſhould likewiſe be provided for ſupporting the candle; and if the board with the paper faſtened upon it be ſurrounded with a broad black frame, the experiments will be ſo much the more ſtriking and beautiful. For ſtill greater convenience, I have added two other ſtands for holding the coloured glaſs through which the light is occaſionally made to paſs in its way to the white ſurface upon which the ſhadows are projected. It will be hardly neceſſary to add, that, in order to the experiments appearing to the greateſt advantage, all light which is not abſolutely neceſſary to the experiment muſt be carefully ſhut out.

Having fitted up a little apparatus, according to the above directions, merely for the purpoſe of proſecuting theſe enquiries reſpecting the coloured ſhadows, I proceeded to make a great variety of experiments, ſome with pointed views, and others quite at random, and merely in hopes of making ſome accidental diſcovery that might lead to a knowledge of the cauſes of appearances which ſtill ſeemed to me to be enveloped in much obſcurity and uncertainty.

Having found that the ſhadows correſponding to two like wax candles were coloured, the one blue and the other yellow, by interpoſing a ſheet of yellow glaſs before one of them; I now tried what the effect would be when blue glaſs was made uſe of inſtead of yellow, and I found it to be the ſame; the ſhadows were ſtill coloured, the one blue and the other yellow, with the difference, however, that the colours of the ſhadows were reverſed, that which with the yellow glaſs was before yellow being now blue, and that which was blue being yellow.

I afterwards tried a glaſs of a bright amethyſt colour, and was ſurpriſed to find that the ſhadows ſtill continued to be coloured blue and yellow. The yellow, it is true, had a dirty purple caſt; but the blue, though a little inclining to green, was nevertheleſs a clean, bright, decided colour.

Having no other coloured glaſs at hand to puſh theſe particular enquiries farther, I now removed the candles, and, opening two holes in the upper parts of the ſhutters of two neighbouring windows, I let into the room, from above, two beams of light from different parts of the heavens; and placing the inſtrument in ſuch a manner that two diſtinct ſhadows were projected by the cylinder upon the paper, I was entertained by a ſucceſſion of very amuſing appearances. The ſhadows were tinged with an infinite variety of the moſt unexpected, and often moſt beautiful colours, which, continually varying, ſometimes ſlowly and ſometimes with inconceivable rapidity, abſolutely faſcinated the eyes, and, commanding the moſt eager attention, afforded an enjoyment as new as it was bewitching. It was a windy day, with flying clouds; and it ſeemed as if every cloud that paſſed brought with it another complete ſucceſſion of varying hues and moſt harmonious tints. If any colour could be ſaid to predominate, it was purple; but all the varieties of browns, and almoſt all the other colours I ever remembered to have ſeen, appeared in their turns; and there were even colours which ſeemed to me to be perfectly new.

Reflecting upon the great variety of colours obſerved in theſe laſt experiments, many of which did not appear to have the leaſt relation to the apparent colours of the light by which they were produced, I began to ſuſpect that the colours of the ſhadows might in many caſes, notwithſtanding their apparent brilliancy, be merely an optical deception, owing to contraſt, or to ſome effect of the other neighbouring colours upon the eye. To determine this fact by a direct experiment, I proceeded in the following manner: Having, by making uſe of a flat ruler, inſtead of the cylinder, contrived to render the ſhadows much broader, I ſhut out of the room every ray of day-light, and prepared to make the experiment with two Argand's lamps, well trimmed, and which were both made to burn with the greateſt poſſible brilliancy; and having aſſured myſelf that the light they emitted was preciſely of the ſame colour, by the ſhadows being perfectly colourleſs which were projected upon the white paper, I directed a tube about 12 inches long, and near an inch in diameter, lined with black paper, againſt the centre of one of the broad ſhadows; and looking through this tube with one eye, while the other was cloſed, I kept my attention fixed upon the ſhadow, while an aſſiſtant repeatedly interpoſed a ſheet of yellow glaſs before the lamp, whoſe light correſponded to the ſhadow I obſerved, and as often removed it. The reſult of the experiment was very ſtriking, and fully confirmed my ſuſpicions with reſpect to the fallacy of many of the appearances in the foregoing experiments. So far from being able to obſerve any change in the ſhadow upon which my eye was fixed, I was not able even to tell when the yellow glaſs was before the lamp, and when it was not; and though the aſſiſtant often exclaimed at the ſtriking brilliancy and beauty of the blue colour of the very ſhadow I was obſerving, I could not diſcover in it the leaſt appearance of any colour at all. But as ſoon as I removed my eye from the tube, and contemplated the ſhadow with all its neighbouring accompaniments, the other ſhadows rendered really yellow by the effect of the yellow glaſs, and the white paper, which had likewiſe from the ſame cauſe acquired a yellowiſh hue, the ſhadow in queſtion appeared to me, as it did to my aſſiſtant, of a beautiful blue colour. I afterwards repeated the ſame experiment with the apparently blue ſhadow produced in the experiment with day-light and candle-light, and with exactly the ſame reſult.

How far theſe experiments may enable us to account for the apparent blue colour of the ſky, and the great variety of colours which frequently adorn the clouds, as alſo what other uſeful obſervations may be drawn from them, I leave to philoſophers, opticians, and painters to determine. In the mean time, I believe it is a new diſcovery; at leaſt it is undoubtedly a very extraordinary fact, that the eyes are not always to be believed, even with reſpect to the preſence or abſence of colours.

I cannot finiſh this letter without mentioning one circumſtance which ſtruck me very forcibly in all theſe experiments upon coloured ſhadows, and that is, the moſt perfect harmony which always appeared to ſubſiſt between the colours, whatever they were, of the two ſhadows; and this harmony ſeemed to me to be full as perfect and pleaſing when the ſhadows were of different tints of brown, as when one of them was blue and the other yellow. In ſhort, the harmony of theſe colours was in all caſes not only very ſtriking, but the appearances were altogether quite enchanting; and I never found any body to whom I ſhewed theſe experiments, whoſe eyes were not faſcinated with their bewitching beauties. It is however more than probable, that a great part of the pleaſure which theſe experiments afforded to the ſpectators, aroſe from the continual changes of colour, tint, and ſhade with which the eye was amuſed, and the attention kept awake. We are uſed to ſeeing colours fixed and unalterable, hard as the ſolid bodies from which they come, and juſt as motionleſs; conſequently dead, unintereſting, and tireſome to the eye; but, in theſe experiments, all is motion, life and beauty.

It appears to me very probable, that a further proſecution of theſe experiments upon coloured ſhadows may not only lead to a knowledge of the real nature of the harmony of colours, or the peculiar circumſtances upon which that harmony depends; but that it may alſo enable us to conſtruct inſtruments for producing that harmony for the entertainment of the eyes, in a manner ſimilar to that in which the ears are entertained by muſical ſounds. I know that attempts have already been made for that purpoſe; but when I conſider the means employed, I am not ſurpriſed that they did not ſucceed. Where the flowing tide, the varying ſwell, the creſcendo is wanting, colours muſt ever remain hard, cold, and inanimate maſſes.

I am very ſorry that my more ſerious occupations do not at preſent permit me to purſue theſe moſt entertaining enquiries. Perhaps at ſome future period I may find leiſure to reſume them [*]. I am, &c.

Munich, March 1, 1793.

---

[*] Otto Guericke, Buffon, Mazeas, Beguelin, and many other philoſophers, have paid attention to the coloured ſhadows of bodies which form the ſubject of this intereſting letter. An abridgement of their obſervations and

III.

A Memoir upon the Diſcovery of America. By M. OTTO.

[Concluded from page 77.]

AFTER having performed ſeveral other intereſting voyages, the Chevalier Behem died at Liſbon in July 1506, regretted by every one, but leaving behind him no other work than the globe which we have juſt been ſpeaking of. It is made from the writings of Ptolemy, Pliny, Strabo, and eſpecially from the account of Mark Paul, the Venetian, a celebrated traveller of the thirteenth century; and of John Mandeville, an Engliſhman, who, about the middle of the fourteenth century, publiſhed an account of a journey of thirty-three years in Africa and Aſia. He has alſo added the important diſcoveries made by himſelf on the coaſts of Africa and America.

From theſe circumſtantial accounts, little known to modern writers, we muſt conclude that Martin Behenira, of whom Garcilaſſo makes mention, is the ſame Chevalier Behem, upon being the place of whoſe birth Nurenberg prides itſelf ſo much. It is probable that, as ſoon as he was knighted in Portugal, he thought it neceſſary to give a Portugueſe termination to his name, to make it more ſonorous and more conformable to the idiom of the country. Garcilaſſo, deceived by this reſemblance of ſound, has made him a Spaniard, in order to deprive Chriſtopher Columbus of the honour of having procured to his country ſo great an advantage. And what ought to confirm us in this opinion is, that we neither find in Mariana, nor any other Spaniſh hiſtorian, the name of this Martin Behenira, who was certainly a man of too much importance not to have had a diſtinguiſhed place in hiſtory. Beſides, the Spaniſh pride would have been flattered in giving to a native thoſe laurels with which it crowned Chriſtopher Columbus.

It is then very unlikely that this navigator was treated as an enthuſiaſt, when he offered to the Court of Portugal to make diſcoveries in the weſt. The ſearch after unknown countries was at that time the reigning paſſion of this Court; and even if the Chevalier Behem had not offered the intereſting ideas which he had procured, the novelty of the project would undoubtedly have engaged King John to ſupport the views of Columbus: but it appears that this prince declined it, becauſe all his thoughts were turned at that time to the coaſt of Africa, and the new paſſage to the Indies, from whence he procured great riches; whilſt the ſouthern coaſt of Brazil, and the territories of the Patagonians, ſeen by Behem, offered to him only barren lands inhabited by unconquerable ſavages. The refuſal of John II., very far from weakening the teſtimony of Behem's diſcoveries, is then

---

and deductions may be ſeen in Prieſtley's Optics, p. 436. Count Rumford is the firſt, as far as I know, who has ſhewn that the effect depends not immediately on the nature of the light, but principally upon the manner in which the organs of ſight, or perhaps the organs of thought (if in truth there be any difference here between them), are affected by the ſucceſſive actions they undergo. The colours called accidental, which are rendered permanent for a time after the impreſſion of bright objects upon the eye, and the effects of leſs forcible impreſſions, for which the laſt quoted work, p. 631, and the authors there referred to, may be conſulted;—the harmony and diſcord of colours, probably ariſing from the pleaſure or diſguſt afforded by the admixture of an accidental colour with a new ſenſation, or real colour;—the phenomena of dazzling, which is of the ſame nature as this laſt combination;—and the general arrangement and inferences to be found in the Zoonomia of Darwin, all bear evident relation to the facts exhibited by Count Rumford, and open a wide field for curious reſearch. N.

rather a proof of the knowledge which this politic prince had already procured of the exiſtence of a new continent: and it was only in 1501, that is to ſay, three years after the voyage of Vaſco de Gama to the Indies, that Emanuel thought proper to take advantage of the diſcoveries of Behem, by ſending Albarez Cabral to Brazil; a meaſure which was perhaps rather owing to the jealouſy which has always exiſted between Portugal and Spain, than to a deſire of making advantageous eſtabliſhments, for which the Indies were much more proper than this part of America.

If any doubts yet remain reſpecting the important diſcovery made by the Chevalier Behem, it is particularly the authority of Dr. Robertſon which attacks the teſtimony of the different authors we have tranſcribed. This learned writer treats the hiſtory of Behem as a fiction of ſome German authors, who had an inclination to attribute to one of their countrymen a diſcovery which has produced ſo great a revolution in the commerce of Europe. But he acknowledges, nevertheleſs, after Herrera, that Behem had ſettled at the iſland of Fayal; that he was the intimate friend of Chriſtopher Columbus; and that Magellan had a globe made by Behem, by the help of which he undertook his voyage to the South Sea; a circumſtance which proves much in favour of our hypotheſis. He relates alſo, that in 1492 this aſtronomer paid a viſit to his family at Nurenberg, and left there a map drawn by himſelf, of which Dr. Forſter procured him a copy, and which, in his opinion, partakes of the imperfection of the coſmographical knowledge of the 15th century: that he found in it, indeed, under the name of the iſland of St. Brandon, land which appears to be the preſent coaſt of Guiana, and lies in the latitude of Cape Verd; but that there is reaſon to believe that this fabulous iſland, which is found in many ancient maps, merits no more attention than the childiſh legend of St. Brandon himſelf. Although Dr. Robertſon does not appear diſpoſed to grant to Behem the honour of having diſcovered the new continent, we find the means of refuting him in his own Hiſtory. He allows that Behem was very intimate with Chriſtopher Columbus; that he was the greateſt geographer of his time, and ſcholar of the celebrated John Müller, or Regiomontanus; that he had diſcovered, in 1483, the kingdom of Congo upon the coaſt of Africa; that he made a globe which Magellan made uſe of; that he drew a map at Nurenberg, containing the particulars of his diſcoveries; and that he placed in this chart land which is found to be in the latitude of Guiana. Dr. Robertſon aſſerts, without any proof, that this land was but a fabulous iſland. We may ſuppoſe, upon the ſame foundation, that the Chevalier Behem, engaged in an expedition to the kingdom of Congo, was driven by the winds to Fernambouc, and from thence by the currents very common in thoſe latitudes towards the coaſt of Guiana; and that he took for an iſland the firſt land which he diſcovered. The courſe which Chriſtopher Columbus afterwards ſteered makes this ſuppoſition ſtill more probable; for if he knew only of the coaſt of Brazil, which they believe to have been diſcovered by Behem, he would have laid his courſe rather to the ſouth-weſt. The expedition to Congo took place in 1483: it is then poſſible that at his return Behem propoſed a voyage to the coaſts of Brazil and Patagonia; and that he requeſted the aſſiſtance of his ſovereign, which we have mentioned above. It is certain that we cannot have too much deference for the opinion of ſo eminent a writer as Robertſon; but this learned man not having it in his power to conſult the original German documents which we have quoted, we may be allowed to form a different opinion without being too preſumptuous.

But

But ſhould it be aſked, Why we take from Chriſtopher Columbus the reputation which all Europe has to this day allowed him?—why ſearch into the archives of an imperial city for the cauſes of an event which took place in the moſt weſtern extremity of Europe?—why the enemies of Chriſtopher Columbus, who were numerous, did not take advantage of the pretended Chevalier Behem to leſſen his conſequence at the Spaniſh Court?—why Portugal, jealous of the diſcovery of the New World, has not proteſted againſt the aſſertions of the Spaniards?—why Behem, who died only in 1506, had not left to poſterity any writing to confirm to himſelf ſo important a diſcovery?

To anſwer all theſe queſtions, I ſhall ſubmit to the impartial reader the following remarks:

1. Before Columbus, the great merit of a navigator conſiſted rather in conceiving the poſſibility of the exiſtence of a new continent, than in ſearching for lands in a region where he was ſure to find them. If it is then certain that Behem had conceived this bold idea before Columbus, the fame of the latter muſt be conſiderably diminiſhed.

2. The hiſtorical proofs which we have given above leaving us no doubt of the fact, we have only to explain the moral cauſes of the ſilence of the Spaniſh and Portugueſe authors, of the enemies of Columbus, and of Behem himſelf.

3. It is well known that, previous to the reign of Charles V. there was little communication between the learned men of different nations. Writers were ſcarce, excepting ſome monks, who have related, well or ill, the events which came to their knowledge, in chronicles which are no longer read; or they had but little notion of what paſſed in foreign countries. Gazettes and Journals were unknown, and the Learned were obliged to travel, to inform themſelves of the progreſs of their neighbours. Italy was the centre of the arts, and what are called ſciences, at that time. The frequent journeys of the German emperors to Rome gave them an opportunity of knowing perſons of merit, and of placing them in the different univerſities of the empire. It is to this circumſtance that we ought to attribute the great progreſs which the Germans made, particularly in mathematics, from the fourteenth to the ſixteenth century; during which time they had the beſt geographers, the beſt hiſtorians, and the moſt enlightened politicians. They were particularly attentive to what paſſed in Europe; and the multiplied connections of different princes with foreign powers aſſiſted them greatly in collecting, in their archives, the original pieces of the moſt important events of Europe. It is to this ſpirit of criticiſm and enquiry that we are indebted for the reformation of Luther; and we cannot deny that, particularly in the fifteenth century, there was more hiſtorical and political knowledge in Germany than in all the reſt of Europe, Italy excepted. It is not then aſtoniſhing that we ſhould find, in the archives of one of the moſt ancient imperial cities, the particulars of an expedition planned upon the banks of the Tagus, by a German, a man of great repute in his own country, and whoſe every action became very intereſting.

4. It was different in Portugal, where the whole nation, except the king, was plunged in the moſt profound ignorance. Every one was either merchant, ſailor or ſoldier; and if this nation has made the moſt important diſcoveries, we muſt aſcribe them rather to avarice than to a deſire of knowledge. They were ſatisfied with ſcraping together gold in every quarter of the known world, whilſt the German and the Italian took up the pen to tranſmit to poſterity the remembrance of their riches and cruelties. The Spaniards were

not much more informed before Charles V. introduced at Madrid the learned men of Flanders and Germany. It is then very poſſible that the Chevalier Behem made very intereſting diſcoveries in geography in 1485, without the public being acquainted with them. If he had brought back from his expedition gold or diamonds, the news would have been ſpread in a few weeks; but ſimple geographical knowledge was not of a nature to intereſt men of this turn of mind.

5. The long ſtay which Chriſtopher Columbus made at Madeira makes his interview with Behem more than probable. It is impoſſible that he ſhould have neglected ſeeing a man ſo intereſting, and who could give him every kind of information for the execution of the plan he had formed. The mariners who accompanied the Chevalier Behem might alſo have ſpread reports at Madeira and the Azores concerning the diſcovery which they had been witneſſes of. What ought to confirm us in this is, that Mariana ſays himſelf (book xxvi. chap. iii.) that a certain veſſel going to Africa was thrown by a gale of wind upon certain unknown lands; and that the ſailors, at their return to Madeira, had communicated to Chriſtopher Columbus the circumſtances of their voyage. All authors agree that this learned man had ſome information reſpecting the weſtern ſhores; but they ſpeak in a very vague manner. The expedition of the Chevalier Behem explains this myſtery.

6. This aſtronomer could not be jealous of the diſcoveries of Columbus, becauſe the laſt had been farther north, and that in a time when they did not know the whole extent of the New World; and when geographical knowledge was extremely bounded, it might be believed that the country diſcovered by Columbus had no connection with that diſcovered by Behem.

It appears however certain, that Behem diſcovered this continent before Columbus; and that this queſtion, which is only curious in Europe, becomes intereſting to the American patriot. The Grecians have carefully preſerved the fabulous hiſtory of their firſt founders, and have raiſed altars to them: Why are not Behem, Chriſtopher Columbus, and Veſpucius deſerving of ſtatues in the public ſquares of American cities? Theſe precious monuments would tranſmit to poſterity the gratitude which theſe benefactors of mankind ſhould inſpire. Without knowing it, they have laid the foundation of the happineſs of many millions of inhabitants; and Seſoſtris, Phul, Cyrus, Theſeus, and Romulus, the founders of the greateſt empires, will be forgotten before the ſervices rendered by theſe illuſtrious navigators can be effaced from the memory of man.

IV.

Deſcription of a Gravimeter, or Inſtrument for meaſuring the Specific Gravity of Solids and Fluids.

By Citizen GUYTON *.

EVER ſince the art of chemiſtry, by an approach to the accurate ſciences, has ſhewn that the phenomena of combinations produced or deſtroyed are not the reſult of occult qualities, but of a rupture of equilibrium determined by forces which afford the hope of admeaſurement by computation, philoſophers have been aware of the neceſſity of conducting their experiments with preciſion, that they might take account of all the circumſtances which might impede or favour this motion. The ſpecific gravity of bodies muſt neceſſarily conſtitute part of thoſe obſervations: for it ſerves not only to indicate the nature of bodies under examination, but likewiſe affords information reſpecting their purity, and the ſtate of their aggregation, condenſation, or rarefaction, which are alſo the immediate cauſes of divulſion or repoſe. It is therefore of importance that the inſtruments for meaſuring this property of bodies ſhould be brought to the utmoſt perfection they are capable of receiving, and that they ſhould be rendered as convenient as poſſible for habitual uſe.

---

* Read to the National Inſtitute on the 11th Germinal, in the 4th year of the Republic. It is inſerted in the twenty-firſt volume of the Annales de Chimie. This tranſlation is nearly verbal.

Of all the hydrometers hitherto known, that of Fahrenheit is acknowledged to be the moſt accurate. The well known principle of this inſtrument is, that it aſcertains the weight of a conſtant bulk of the ſeveral fluids under examination. Such hydrometers as are conſtructed to meaſure the denſity by the degree of immerſion, may ſerve, in ſome manufactories, to give an approximation ſufficiently near for the purpoſe required. But without conſidering the inequality of the ſtem, the tedious work of graduating by obſervation, and the uncertainty of eſtimate from one diviſion to another, it may be remarked, that they are not capable of correction for the different temperatures. In a word, they are not fit for the hand of the philoſopher.

The form which Nicholſon gave ſome years ago to the hydrometer of Fahrenheit *, rendered it proper to meaſure the denſity of ſolids. At preſent it is very much uſed. It gives, with conſiderable accuracy, the ratio of the ſpecific gravity to the fifth decimal, water being taken as unity. It is ſuſceptible of correction for the variations of temperature, and the impurity of the water which it is ſometimes more convenient to uſe, as may be ſeen in the article *Areometer*, in the chemical part of the Encyclopedie Methodique. It does not appear that any better inſtrument need be wiſhed for in this reſpect.

But this hydrometer has hitherto been conſtructed in metal only; ſo that it could not be applied either to ſalts or acids. It is known likewiſe that areometers conſtructed on the principles of Fahrenheit, for ſpirituous, ſaline, and acid liquors, require to be varied in magnitude, form, and quantity of ballaſt. In the one kind, the lower weight muſt be at a great diſtance from the buoyant part, to maintain the vertical ſituation; in the other, it is brought nearer, to operate on ſmaller maſſes of the fluid. Thoſe inſtruments which are intended for alcohol muſt be light, and thoſe applied to determine the denſity of concentrated acids muſt be heavy. The maſs and dimenſions of every inſtrument of this nature muſt be adjuſted in ſuch a manner that the additional weight may operate not only as a load, but as ballaſt. It cannot be applied in the upper baſon without deranging the vertical ſituation. From all which circumſtances it happens, that a collection of theſe inſtruments would be required, to anſwer every purpoſe of experiment. As a remedy for part of theſe inconveniences, it was ingeniouſly propoſed that the areometer ſhould terminate beneath in a hook, to which were ſuſpended at pleaſure certain glaſs balls filled with mercury, and conſtituting different ballaſts; but this did not anſwer in every reſpect. In all the inſtruments of this kind which I have had occaſion to examine, no attempt had been made, or at leaſt the object had not been accompliſhed, to render the point of interſection of the ſtem with the fluid, common to all the ballaſts. Whence it followed, that, upon changing the ballaſt, it was likewiſe neceſſary to change the ſlip of glaſs which carries the mark within the upper ſtem.

---

* From Lowthorpe's Abridgement of the Philoſophical Tranſactions, I. 604, or Boyle's Works, in quarto, London, 1772, IV. 204, it appears that the hydrometer was firſt invented by Boyle, and deſcribed under the name of a new Eſſay Inſtrument. It had a graduated ſtem, and, by means of a ſtirrup or clip underneath, it was applied, as perfectly as a graduated inſtrument could be, to aſcertain the ſpecific gravities of ſolids as well as fluids. Fahrenheit firſt applied a diſh for weights at the top, to aſcertain the ſpecific gravities of fluids only; as may be ſeen in Read and Gray's Abridgement of the Philoſophical Tranſactions, Vol. VI. Part I. p. 294. My inſtrument, referred to by Citizen Guyton, is a combination of both. It is deſcribed in the 2d volume of the Mancheſter Memoirs, and is repreſented in Plate VI. Fig. 1. N.

I have thought it poſſible, by following the principles of Fahrenheit and executing the inſtrument of Nicholſon in glaſs with a ſlight addition, to render it more generally uſeful and commodious, without diminiſhing its accuracy in any reſpect. I was well aware of the prejudice which is naturally entertained againſt polychreſt, or univerſal inſtruments, moſt of which are rendered of no uſe from the attempt to extend their application too far; but at the ſame time I was convinced that it would be a real advantage to every philoſophical obſerver, to require only one ſingle meaſure for determining the denſity of all bodies, whether ſolid or liquid. This is the object I propoſe to accompliſh. It will be ſeen how far I have ſucceeded. I muſt obſerve, on this occaſion, that the name hydrometer *(peſe-liqueur)* as well as that of areometer, is ſcarcely applicable to an inſtrument poſſeſſing theſe qualities: for theſe terms ſuppoſe that the liquid is always the thing weighed; whereas, with regard to ſolids, the liquid is the term of compariſon which is known, and to which the unknown weight is referred. I propoſe therefore to make uſe of the name gravimeter, which will eaſily be underſtood, and applied with propriety in every caſe.

This inſtrument, being executed, as already remarked, in glaſs, is of a cylindric form, being that which requires the ſmalleſt quantity of the fluid, and is on that account preferable, except ſo far as it is neceſſary to deviate for the ſecurity of a perpendicular poſition.

Like the inſtrument of Nicholſon, it carries two baſons; the one ſuperior, at the extremity of a thin ſtem; towards the middle of which the fixed point of immerſion is marked. The other lower baſon terminates in a point; it contains the ballaſt, and is attached to the cylinder by two branches. The moveable ſuſpenſion by means of a hook has the inconvenience of ſhortening the lever which is to ſecure the vertical poſition.

The cylinder is 22 millimeters (0.71 inches) in diameter; and 21 centimeters (6.85 inches) in length. It carries in the upper baſon an additional conſtant weight of five grammes. Theſe dimenſions might be increaſed, ſo as to render it capable of receiving a much more conſiderable weight; but it will hereafter be ſhewn, that this is unneceſſary.

I have added a piece which I call the diver *(plongeur)*, becauſe in fact it is placed in the lower baſon when uſed, and conſequently is entirely immerſed in the fluid *. It is a bulb of glaſs, loaded with a ſufficient quantity of mercury, in order that its total weight may be equal to the conſtant additional weight, added to the weight of the volume of water diſplaced by this piece.

It will readily be underſtood, that the weight being determined at the ſame temperature at which the inſtrument was originally adjuſted, it will ſink to the ſame mark on the ſtem, whether it be loaded with a conſtant additional weight in the upper baſon, or whether the effect of this weight be produced by the additional piece in the lower diſh.

From this explanation there will be no difficulty in deducing how this inſtrument may be adapted to every caſe of practice.

---

* As we have no word in the Engliſh language which can with propriety be uſed as the tranſlation of the word *plongeur*, I ſhall take the liberty to call this weight the additional piece. N.

It may be uſed, 1. for ſolids. It is the hydrometer of Nicholſon, from which it differs in no reſpect. The only condition will be, as in his inſtrument, that the abſolute weight of the body to be examined ſhall be rather leſs than the conſtant additional weight, which in this inſtrument is five grammes (115 grains).

2. For liquids of leſs ſpecific gravity than water, the inſtrument, without the additional weight above, weighs about two decagrammes (459 grains) in the dimenſions before laid down. It would be eaſy to limit its weight to the utmoſt accuracy. We have therefore the range of one-fifth of buoyancy, and conſequently the means of aſcertaining all the intermediate denſities from water to the moſt highly rectified ſpirit of wine, which is known to bear in this reſpect the ratio of eight to ten with regard to water.

3. When liquids of greater ſpecific gravity than water are to be tried, the conſtant weight being applied below, by means of the additional piece, which weighs about ſix grammes (138 grains), the inſtruments can receive in the upper baſon more than four times the uſual additional weight, without loſing the equilibrium of its vertical poſition. In this ſtate it is capable of ſhewing the ſpecific gravity of the moſt concentrated acids.

4. It poſſeſſes another property common to the inſtrument of Nicholſon, namely, that it may be uſed as a balance to determine the abſolute weight of ſuch bodies as do not exceed its additional load.

5. Laſtly, the purity of the water being known, it will indicate the degrees of rarefaction and condenſation in proportion to its own bulk.

I have little to ſay reſpecting the conſtruction of this inſtrument. Every workman in glaſs who ſhall once ſee it, will be able to make it without difficulty. The additional piece for the lower baſon will require ſome attention to make it perfectly agree with the conſtant upper weight, as to the immerſion of the inſtrument. But this object may, by careful adjuſtment, be aſcertained with the utmoſt certainty and accuracy.

The bulb of glaſs is for this purpoſe drawn out to a fine point; a ſufficient quantity of mercury is then introduced to ſink it, and the aperture cloſed with a morſel of wax. The bulb being then placed in the lower baſon of the inſtrument, the upper baſon is to be loaded until the mark on the ſtem becomes accurately coincident with the ſurface of the water. The ſum of the weights added above is preciſely equal to that of the quantity of mercury neceſſary to be added to that in the glaſs bulb; which done, nothing more is neceſſary than to ſeal the point by fuſion, taking care not to change its bulk.

Though this inſtrument is rather delicate in form, it has no other imperfection than the natural brittleneſs of the material, which muſt neceſſarily be uſed for experiments with ſaline and acid liquors. For ſix months paſt I have made very frequent uſe of one of theſe inſtruments in the Polytechnic School, without any other inconvenience than that one of the branches of the lower baſon was accidentally broken.

Nothing more remains but to render it portable. I apprehend that this object is ſufficiently ſecured by means of a caſe in which all the delicate parts are ſecured from preſſure, and the heavier parts ſupported in ſuch a manner as to reſiſt the exceſs of motion, they are capable of acquiring by virtue of their maſs. This laſt circumſtance is frequently overlooked by ſuch workmen as are employed in the package of inſtruments; whence it neceſſarily follows, that ſome ſtrain or fracture muſt be produced when matters of very unequal denſity are expoſed to receive a common impulſe.

The method of ſecuring this inſtrument in its caſe will be better underſtood from the figure, than from the moſt extended verbal deſcription.

ADDITION to the foregoing MEMOIR.

THE conſtant uſe I have made of the gravimeter ſince I preſented the deſcription to the National Inſtitute, has led me to make ſome ſlight changes, by which its conſtruction is rendered more eaſy, and the effects of brittleneſs in one of its principal parts are removed.

It has alſo appeared to be of ſome utility to annex to this memoir the formula, by means of which, the gravimeter being once well adjuſted, we are enabled, by a very ſimple calculation, *to find the ſpecific gravity of any ſubſtance whatever in proportion to that of diſtilled water at the temperature of* 12.5 *degrees of the decimal thermometer, and* 757.7 *millimeters of preſſure, without the uſe either of diſtilled water, or the thermometer or barometer.* The aſtoniſhment which I have ſometimes remarked when the problem has been thus announced, induces me to think that the ſolution here added will be acceptable, inſtead of the reference to the article *Areometer* of the Dictionary of Chemiſtry, in the Encyclopédie Methodique.

I ſhall likewiſe add an explanation of the figures which repreſent the inſtrument and its caſe, together with examples of the application of the gravimeter to tables of ſpecific gravity.

Deſcription of a Solid Additional Piece for the Lower Baſon.

THE condition of hermetically ſealing the additional piece without changing its volume neceſſarily requires that the part near the aperture ſhould be very thin. Hence it has ſometimes happened that the point was broken without any external blow, but merely in conſequence of the motion of the mercury contained within. A ſimilar glaſs bulb might indeed be added; but this conſtitutes but a ſmall part of the remedy. For it is neceſſary to adjuſt the inſtrument again to preſerve the property of meaſuring the ſpecific gravity of the denſer fluids, and it has been found that this operation was not exempt from difficulties when the deſired degree of preciſion was to be ſought.

I have remedied this inconvenience by ſubſtituting in the place of the glaſs bulb loaded with mercury a ſmall maſs of ſolid glaſs, as the ſtopper of a bottle, which is firſt brought to the proper form by grinding, and afterwards carefully diminiſhed, until, when placed in the lower baſon of the inſtrument, its immerſion in diſtilled water at the required degrees of temperature and preſſure ſhall be exactly the ſame as when the inſtrument is floated in the ſame liquid with its conſtant additional weight in the upper baſon only.

By this means there is a certainty of acquiring the utmoſt degree of preciſion at firſt trial; becauſe the whole proceſs is reduced to the mere adjuſtment of a weight.

*Concerning the Application of the Gravimeter, to find the Specific Gravity of any Subſtance whatever, without requiring Diſtilled Water, or the Thermometer or Barometer, or any ſubſequent Correction**.

THE gravimeter being ſuppoſed to be well regulated, let x repreſent the ſpecific gravity ſought; b the additional weight neceſſary to ſink the inſtrument to the mark in the unknown fluid; c the weight which placed with the ſolid in the upper baſon immerſes the inſtrument to the mark; d the additional weight to produce the ſame effect when the body is in the lower baſon; Π the ſpecific gravity of diſtilled water, at the temperature of 12.5 degrees of the decimal thermometer, and the preſſure of 757.7 millimeters $= 1$; Π' the ſpecific gravity of the water made uſe of.

---

* There is a correction overlooked by the celebrated author of this paper, and by almoſt every writer on this ſubject. It ariſes from the expanſion or contraction of the inſtrument itſelf, and of the ſolid under examination.

The following formula gives the ſolution *: $x = \frac{(b-c)\,\Pi'}{d-c}$

The value of Π' is firſt to be found, which is greater than unity when the water made uſe of is heavier than diſtilled water, and in the contrary caſe is a fraction.

Let P repreſent the weight of the gravimeter without any additional weight †; V the conſtant volume of the immerſed part; a the additional conſtant weight in the upper baſon, or that which immerſes it to the mark in diſtilled water Π; and we ſhall have $P + a = V\,\Pi$; whence $V = \frac{P + a}{\Pi}$

Again b repreſents the weight more or leſs than a, which muſt be ſubſtituted to produce the ſame immerſion in another liquor different from diſtilled water.

‡ We ſhall therefore have $\Pi' = \frac{P+b}{V} = \frac{P + b}{P + a}$.

The value of Π' being found, every thing elſe is known; nothing more being neceſſary than to ſubſtitute this value in the formula.

I am perſuaded that philoſophers will immediately perceive the advantages of this method. Diſtilled water was wanting; but this praxis renders it unneceſſary. Even if diſtilled water were at hand, it would ſeldom happen that the times of the ſtandard temperature and preſſure would agree with thoſe of the experiment; and when an artificial temperature is produced, it is ſubject to vary during the courſe of the experiment. All theſe difficulties are removed; and even when diſtilled water is at hand, I prefer, more eſpecially in ſummer, water containing a ſmall portion of neutral ſalt. Two motives juſtify this preference:

---

mination. I am not prepared to ſtate the method of applying this correction; and ſhall only remark, in this place, that the mere change of dimenſions from temperature in a piece of ſteel (independent of the greater change in the whole inſtrument) will alter the fourth figure at every third degree of Fahrenheit. For pyrometrical data ſee Philoſ. Journal, I. 58. N.

* For the ſake of thoſe who are unacquainted with ſymbols, I ſhall here give the rule in words at length. From the weight in the upper diſh, when the inſtrument is properly immerſed in the unknown fluid, take the weight which is placed with the body in the ſame ſcale at the like adjuſtment. The remainder is the abſolute weight of the ſolid. Multiply this by the ſpecific gravity of the fluid, and reſerve the product. From the additional weight when the body is placed in the lower baſon, take the additional weight when it was placed in the upper. The remainder will be the loſs of weight by immerſion. Divide the reſerved product by the loſs by immerſion, and the quotient will be the ſpecific gravity of the ſolid with regard to diſtilled water at the ſtandard temperature and preſſure. N.

† In the upper diſh. For as the lower additional piece acts only by its reſidual weight, and this muſt vary with the fluid, it is clear that it muſt be conſidered as part of the inſtrument, and enter into the value of P whenever it is uſed. In fact, the gravimeter with this piece is an inſtrument perfectly diſtinct from that in which it is not uſed, and the adjuſtment may then be made as well (and perhaps more eaſily) by a conſtant ſmall weight in the upper diſh, as by the very delicate proceſs of the author. N.

‡ Or in words: To the weight of the gravimeter add the weight required in the upper baſon to ſink it in the unknown fluid. Again, to the weight of the gravimeter add the weight required in the ſame manner to ſink it in diſtilled water. Divide the firſt ſum by the latter, and the quotient will be the ſpecific gravity of the fluid in queſtion. N

1. It is more convenient to add a few milligrams to the conſtant additional weight, than to compoſe a leſs by a ſeries of ſub-multiples; 2. By making uſe of a liquid, at the temperature of the ſurrounding air, it is evidently leſs expoſed to ſudden variations. Theſe circumſtances are more favourable to accuracy in the reſult.

Explanation of the Figures.

Fig. 2, Plate VI.—The gravimeter.
a. The lower baſon.
b. The upper baſon.
c. The point of immerſion marked on a thin piece of glaſs in the inſide of the ſtem.

Fig. 3. The piece called *plongeur*, which is placed in the lower baſon a when experiments are made on fluids of greater denſity than water.

Fig. 4. The gravimeter in the cylindric veſſel filled with water, in which it floats, immerſed to the mark c, by means of the additional conſtant weight d.

It is convenient to chooſe a veſſel of ſuch a depth that the inſtrument may be at liberty to float at the level of the mark, or even beneath it, without its being poſſible that the bottom of the upper baſon ſhould ever deſcend to the ſurface of the water.

Fig. 5. The gravimeter in its caſe.
A. The cylindrical part of the inſtrument lodged in a groove in the caſe, and ſecured above by the two projections e e which leave the ſtem at liberty. It is ſecured at the middle by the braſs button f, and is preſſed below by a piece of cork g, which reſts on the fixed block h.
i. Sliding piece and ſcrew to ſupport the ballaſt-piece, and prevent the branches from being endangered by any internal movement of the mercury.
k. The additional ballaſt-piece, or *plongeur* in its ſeparate cell.
l. The conſtant additional weight placed in a cell cut in the ſolid wood, and cleared out at the ſides, ſo that it may be conveniently taken up when wanted.
m. The inner ſurface of the cover hollowed out at n to receive without friction the projecting part of the upper baſon. A paper is paſted on the inner ſurface of the cover, to ſhew the weight of the gravimeter with or without the additional ballaſt-piece, and the volume of water it diſplaces in either caſe, which are often required to be accurately known.

On the uſeful Application of the Gravimeter to the Reſults of Tables of Specific Gravity.

IT is frequently neceſſary, in philoſophical as well as commercial tranſactions, to determine the proportions of a mixture of two liquids, or an alloy of two metals; and it has been for ſome time known, that in order to render this operation eaſy and certain, recourſe muſt be had to tables drawn up from obſervation with the aſſiſtance of an inſtrument capable of giving the ſpecific gravity to at leaſt three decimals. The gravimeter will perfectly anſwer this purpoſe. To ſhew this more effectually, I ſhall apply it to the mixture of alcohol and water, and the alloy of tin and lead. Theſe two compoſitions are preciſely what in a commercial point of view moſt frequently require to be examined in this reſpect. And I am convinced that an exhibition of the moſt accurate reſults of experiments of ſuch extent and minuteneſs, cannot fail to prove acceptable in this place.

A TABLE of the Specific Gravities of the Mixtures of Alcohol and Water, with the Proportion of theſe Fluids at the Temperature of 60 Degrees of the Scale of Fahrenheit, 12,44 of Reaumur, 15,55 of the Decimal Thermometer.

Note. I have placed in this table the ratios publiſhed by Citizen Chauſſier, from the Memoirs of Citizen Gouvenain, at the article ALCOHOL of the Encyclopédie Methodique, and thoſe given in the tables of Mr. Gilpin, which are recommended by the philoſophers of Germany. Journ. Phyſ. de M. Gren, 1796, Tom. III. p. 128.

Centeſimal parts of the mixture *.	Specific Gravities.	
	According to Chauſſier.	According to Gilpin (laſt table. N.)
Alcohol - 100	0.7980	0.825
95	0.8165	0.83887
90	0.8340	0.85244
85	0.8485	0.86414
80	0.8620	0.87606
75	0.87525	0.88762
70	0.8880	0.89883
65	0.9005	0.90941
60	0.9120	0.91981
55	0.9230	0.92961
50	0.9334	0.93882
45	0.94265	0.94726
40	0.9514	0.95493
35	0.95865	0.96158
30	0.96535	0.96736
25	0.97035	0.97239
20	0.97605	0.97723
15	0.9815	0.98213
10	0.9866	0.98737
5	0.99335	0.99327
0	0.99835	1.00000

We ſhall not be ſurpriſed at the difference between theſe two tables, if we attend to the circumſtance that the alcohol uſed by Chauſſier was more highly rectified, its ſpecific gravity having been only 0.798 at the ſame temperature at which the ſpirit uſed by Gilpin gave .0825. But it is not eaſy to conceive that this difference could produce ſo conſiderable a change in the penetration or diminution of volume of the mixtures; that indicated by Gilpin at equal parts of ſpirit and water being 0.025, whereas, according to Chauſſier, it is 0.0454 †.

TABLE

---

* By meaſure, the water being added as the ſpirit is diminiſhed, ſo as to keep the volume conſtantly = 100.

† For want of that part of the New Encyclopedie which contains the tables of Chauſſier, I am not qualified to give an account of the degree of accuracy they may poſſeſs. The tables of Gilpin are to be found in the ſecond part of the Philoſophical Tranſactions for 1794. They are comprehended in one hundred quarto pages, and expreſs—for every ſingle degree of Fahrenheit, from 30° to 80° incluſive;—and for all mixtures of ardent ſpirit and water, formed by the addition of 1, 2, 3, &c. parts by weight of water, as far as 100, to the conſtant quantity 100 of ſpirit, at 0.825 ſpecific gravity when at 60;—and for all mixtures formed in like manner by adding

TABLE of the Correſpondencies of the Specific Gravities of Alloys of Tin and Lead, with the reſpective Proportions of theſe Metals.

In conſtructing this table, I have made uſe of the obſervations publiſhed by M. Bergenſtierna in the Memoirs of the Academy of Stockholm for 1780, and reprinted in the Manuel Syſtematique of M. Gren (§ 3195). But as he grounded all his ratios on the variations of abſolute weight under equal volumes, and made uſe of foreign weights, I have been obliged to change the expreſſion for the more ſimple and uſual proportion to diſtilled water, taken as unity.

At every fifth line I have added the mathematical ſpecific gravity, or that which is determined by calculation, in order to ſhew the changes of bulk which reſult from the combination, and in this particular caſe diminiſh the denſity inſtead of increaſing it.

The ſpecific gravity of pure lead to water is as 11.1603 to 1.

The ſpecific gravity of pure tin is 7.2914 to 1.

Compound Metal.		Mathematical Specific Gravity.	Real Specific Gravity.	Compound Metal.		Mathematical Specific Gravity.	Real Specific Gravity.
Tin.	Lead.			Tin.	Lead.		
99	1	7.3300	7.3252	89	11		7.6468
98	2		7.3552	88	12		7.6787
97	3		7.3871	87	13		7.7106
96	4		7.4189	86	14		7.7425
95	5	7.4848	7.4508	85	15	7.8717	7.7744
94	6		7.4828	84	16		7.8063
93	7		7.5146	83	17		7.8382
92	8		7.5511	82	18		7.8701
91	9		7.5835	81	19		7.9020
90	10	7.6782	7.6149	80	20	8.0652	7.9339

adding 1, 2, 3, &c. parts by weight of the ſpirit, to the conſtant quantity 100 of water. (1) The ſpecific gravity to five places of figures; (2) the proportion of water by meaſure to the ſpirit taken as 100; (3) the actual bulk of this laſt mixture; (4) the diminution of bulk by mixture; (5) the quantity of ſpirit per cent.; and (6) a decimal multiplier, which being applied to the bulk of the mixture gives the meaſure of the pure ſpirit in a mixture of that denſity. Hence it may be inferred how extenſively uſeful this table muſt prove to all practical philoſophers and manufacturers. With regard to its accuracy, I muſt refer to the ample reports of Dr. (now Sir Charles) Blagden, in the Tranſactions for 1790, and ſhall only obſerve that the experiments were made with the balance of the Preſident of the Royal Society; of the admirable mechaniſm and ſenſibility of which, ſome account, though imperfect, is given in the Journal de Rozier, Vol. XXXIII. and that the term or ſtandard ſpirit .825 was arbitrarily choſen from motives of convenience and accuracy.

The ſpirit was obtained from malt. The greateſt ſtrength obtained by adding hot cauſtic alkali to ſpirit and then carefully diſtilling, gave a ſpecific gravity of 0.813 at 60 degrees. Perhaps the circumſtances of M. Gouvenain's experiments may ſhew by what means he ſucceeded in arriving at 0.7980 at the ſame temperature. No uſeful purpoſe would be anſwered by conjecture in this place. From the laſt line in the table here given by Citizen Guyton, the ſpecific gravity of mere water is ſuch as to indicate a temperature of 75 degrees inſtead of 60 in Chauſſier's column. In which caſe Gilpin's ſtandard ſpirit would have a ſpecific gravity of 0.8178. With regard to the conſiderable difference in the penetration or diminution of bulk, it remains to be ſhewn whether it depends upon any part of the proceſſes by which the alcohol was obtained and concentrated, or, as is more probable, upon ſome miſtake in the calculations or methods of expreſſing the quantities. N.

Compound Metal.		Mathematical Specific Gravity.	Real Specific Gravity.
Tin.	Lead.		
79	21		7.9658
78	22		7.9977
77	23		8.0296
76	24		8.0615
75	25	8.2586	8.0934
74	26		8.1253
73	27		8.1572
72	28		8.1891
71	29		8.2210
70	30	8.4520	8.2529
69	31		8.2848
68	32		8.3167
67	33		8.3486
66	34		8.3828
65	35	8.6455	8.4170
64	36		8.4511
63	37		8.4853
62	38		8.5195
61	39		8.5537
60	40	8.8397	8.5879
59	41		8.6228
58	42		8.6562
57	43		8.6904
56	44		8.7246
55	45	9.0333	8.7588
54	46		8.7929
53	47		8.8271
52	48		8 8613
51	49		8.8955
50	50	9.2258	8.9319
49	51		8.9729
48	52		9.0139
47	53		9.0550
46	54		9.0960
45	55	9.4203	9.1373
44	56		9.1552
43	57		9.2190
42	58		9.2600
41	59		9.3033
40	60	9.6139	9.3466
39	61		9.4727
38	62		9.4355
37	63		9.4788
36	64		9.5221
35	65	9.8074	9.5676
34	66		9.6132
33	67		9.6565
32	68		9.7021
31	69		9.7454
30	70	10.0010	9.7887
29	71		9.8297
28	72		9.8730
27	73		9 9163
26	74		9.9573
25	75	10.1945	9.9983
24	76		10.0416
23	77		10.0871
22	78		10.1350
21	79		10.1806
20	80	10.3881	10.2261
19	81		10.2717
18	82		10.3173
17	83		10.3629
16	84		10.4084
15	85	10.5799	10.4586
14	86		10.5062
13	87		10.5543
12	88		10.6021
11	89		10.6500
10	90	10.7734	10.7001
9	91		10.7479
8	92		10.7958
7	93		10.8414
6	94		10.8869
5	95	10.9668	10.9354
4	96		10.9781
3	97		11.0236
2	98		11.0692
1	99		11.1148
0	100	11.1216	11.1603

V.

Deſcriptions of the Improved Air Pumps of PRINCE *and* CUTHBERTSON; *with Obſervations.*

THE important uſes of the Air Pump are too many and too well known to require enumeration. It is an engine, not only calculated to demonſtrate many of the leading principles in natural philoſophy, but indiſpenſable for the performance of ſome of the moſt accurate experiments in the Pneumatic Chemiſtry. The air is exhauſted out of a receiver by this inſtrument, by an action nearly ſimilar to that of the common ſucking pump, as it is called. In the common pump there is a bucket, which by leathering is made to ſlide, water-tight, up and down the pipe; but inſtead of a bottom, it is provided with a valve or flap opening upwards. By this contrivance the bucket may be moved downwards through a column of water without producing any progreſſive motion in the fluid, becauſe the valve then opens and affords a free paſſage; and, on the contrary, when it is moved upwards, it muſt alſo move the whole of the column above the flap or valve, which is then ſhut. Another valve, alſo opening upwards, is fixed in the pipe below the ordinary range of the bucket, and ſerves to prevent the return of any fluid which may lie between the two valves during the deſcent of the bucket itſelf.

In working this common and very uſeful engine, it is firſt neceſſary to pour a ſmall quantity of water into the pipe above the bucket; after which, the handle being moved produces the following effect: As the bucket deſcends, the ſpace between the two valves becomes diminiſhed, and the air contained in that ſpace lifts the upper valve, and in part riſes with noiſe through the water; after which, as the bucket riſes, and again enlarges the ſpace, the remaining air expands by its ſpring, which is thus weakened. The air in the pipe beneath the lower valve will therefore act more ſtrongly upwards by its entire ſpring than the air above does downwards. It will open the valve, and riſe up into the upper ſpace; at the ſame time that the preſſure of the external air, upon the water in the well, will drive part of the water after it into the lower part of the pipe. By continuing the work for a few ſtrokes the included air is thus entirely drawn out and ſucceeded by water, provided the height from the ſurface of the fluid in the well to the upper valve be not ſo great as to include a column of upwards of thirty-three feet. For ſuch a column would be ſufficiently heavy to counterpoiſe the whole preſſure of the atmoſphère, and would accordingly prevent any farther aſcent.

If this laſt-mentioned caſe were to exiſt, or if the lower aperture of the pipe were cloſed, it may without difficulty be underſtood that in theory, that is to ſay, with a perfect inſtrument, the air beneath the lower valve would become more and more rarefied to a certain point as the proceſs was continued. And the progreſs and extent of this rarefaction would be computable from the known ratios of the greateſt and leaſt ſpaces between the valves when in their extreme poſitions, together with the aſſumed conditions that the air, however greatly rarefied, would diffuſe itſelf equally through the containing ſpace, and that the lower valve ſhall afford no reſiſtance to the power applied to open it when the bucket is elevated. It will alſo follow, that the ſame rarefaction might be produced in any veſſel containing air, and communicating only with the pipe of the pump beneath the lower valve.

Thus it is ſeen that the common ſucking pump is, in fact, an air pump before it operates as a pump for water; and that nothing more is required to conſtitute the philoſophical air pump than convenience of ſize, accuracy of workmanſhip, and, that the reſiſtance at the valves be either diminiſhed or removed.

It would be to little purpoſe to enter into any diſcuſſion concerning the original conſtruction of the air pump, as invented by Otto Guericke, and afterwards improved and moſt uſefully employed by our eminent countryman Robert Boyle. It is generally allowed at preſent, that ſuch air pumps as operated by means of ſtop-cocks inſtead of the lower valve, are not durable, but ſoon become leaky. If ever theſe pumps, of which engravings are to be ſeen in the works of Muſchenbroek and S'Graveſande, were uſed in England, which I think was not the caſe, they muſt have been long ſuperſeded by the cheaper and more ſimple contrivance of valves formed by tying a ſtrip of bladder over a ſmall hole through which the air is allowed to paſs in one direction only. Mr. Smeaton was of opinion, that the operation of the air-pump is rendered defective from the ſmall ſurface of preſſure at which the air acts againſt the bladder of the lower valve, as well as from the preſſure of the external air, which is ſuppoſed to prevent the upper valve from opening in the laſt ſtages of the proceſs. To remedy theſe imperfections, he made a number of hexagonal holes inſtead of the one ſmall hole in the valve; and by paſſing the rod of the piſton through a collar of leathers at the top of the barrel, which was cloſed, with the exception of a valve opening outwards, he prevented the external air from acting upon the valve of the piſton in its deſcent *. Mr. Abraham Brooke of Norwich, by experiments with the common air-pump, has rendered it at leaſt doubtful, whether, in pumps equally well made, theſe be improvements at all †. But, previous to the time this laſt author wrote, there exiſted two other air-pumps, containing improvements both in principle and workmanſhip ſufficient to annihilate any controverſy reſpecting the merits of the common pump, and that of Smeaton. Theſe were made by the Rev. Mr. Prince, of Salem in North America, and Mr. John Cuthbertſon of Amſterdam. The firſt is deſcribed in the only volume of Memoirs publiſhed by the American Academy, and is ſcarcely known in the philoſophical world. The other is deſcribed in a publication ‡ by the inventor; of which the circulation muſt, in the ordinary courſe of things, be conſiderably limited. I have thought it my duty to preſent both to the world in this place.

Mr. Prince's paper contains a conſiderable portion of matter relative to the air-pump of Smeaton, partly deſcriptive of the improvements made by that ingenious mechanic, and partly by way of compariſon with his own. For the ſake of brevity, I have omitted theſe, and other matters of leſs conſequence; but in the eſſential parts of the account I have kept as nearly as poſſible to the words of the author.

From the account of Mr. Smeaton's ſucceſs in facilitating the opening of the valves at the bottom of the barrel and in the piſton, Mr. Prince ſuppoſed, if thoſe valves were entirely removed, and the remaining air in the barrel could be more perfectly expelled, the rarefaction might be carried ſtill further. Upon this plan he conſtructed his pump. He removed the lower valve, and opened the bottom of the barrel into a ciſtern on which it was placed, and which had a free communication with the receiver. For the valve on the plate at the top of the barrel (which is conſtructed like Smeaton's) makes it unneceſſary, as he remarks, that there ſhould be any at the bottom in order to rarefy the air in the receiver.

The ciſtern is deep enough to allow the piſton to deſcend into it below the bottom of the barrel. Suppoſe then the piſton to be ſolid, that is, without a valve in it; when it enters the barrel and riſes to the top plate, which is made air-tight with a collar of leathers, &c.

---

* Philoſophical Tranſactions, XLVII.

† Miſcellaneous Experiments and Remarks on Electricity, &c. by A. Brooke. Norwich, 1789. P. 123, and elſewhere.

‡ Deſcription of an improved Air Pump. By John Cuthbertſon. Octavo, 41 pages. London; no date. This artiſt is at preſent ſettled in London.

like Smeaton's, it forces out all the air above it; and, as the air cannot return into the barrel on account of the valve on the top plate, when the piſton deſcends, there will be a vacuum formed between that and the plate, every thing being ſuppoſed perfect. But in working the pump, the piſton is not allowed to deſcend entirely into the ciſtern ſo far as to leave the bottom of the barrel open; becauſe, as the ciſtern, for another purpoſe, is made larger than the bore of the barrel, this might make the piſton-rod work unſteadily in the collar of leathers, and cauſe it to leak; but it deſcends below a hole in the ſide of the barrel near the bottom, which opens a free communication between the barrel, ciſtern, and receiver. Through this hole the air ruſhes from the ciſtern into the exhauſted barrel, when the piſton has dropped below it; and by its next aſcent this air is forced out as the other was before. If now, the capacity of the receiver, ciſtern, pipes, &c. below the bottom of the barrel, taken together, be equal to the capacity of the barrel, half the remaining air will be expelled by every ſtroke.

But as the working a pump of this kind with a ſolid piſton would be laborious, on account of the reſiſtance it would meet with in its deſcent from the air beneath (though this would be leſſened by every ſtroke as the air became more rarefied), the inventor, to remedy this inconvenience, pierced three holes in the piſton at equal diſtances from each other; and a circular piece of bladder, which is tied over the top of the piſton, to make the joint more perfect with the top plate, and to defend them from injury when the piſton is brought up againſt it, forms a kind of valves over the holes, which open eaſily enough to prevent any labour in working the pump, as it allows the air to paſs through the piſton when it deſcends. But the air does not neceſſarily depend upon a paſſage through the piſton in order to get into the barrel: for when the air becomes ſo weak from its rarefaction that it cannot open this valve, it will ſtill get into the barrel when the communication is opened by the hole at the bottom. This piſton, therefore, will deſcend as eaſily as any other; and theſe valves do not impede the rarefaction, ſince it is of no conſequence, as to this, whether they open or not. By this conſtruction the valves, which Mr. Smeaton only made to open with more eaſe, are rendered unneceſſary in rarefying the air, and that at the bottom of the barrel, which is the moſt difficult to be made and kept in order, is entirely removed; the valve on the top plate being the only one neceſſary in rarefying the air.

But as in a ſingle-barrelled pump of this conſtruction, where there is no valve at the bottom to prevent the air, which follows the piſton in its aſcent, from returning into the receiver in its deſcent, a fluctuation would be produced which might prove detrimental in ſome experiments, this pump is made with two barrels, which rarefy the air at every ſtroke of the winch. In this conſtruction, the capacity of the two barrels taken together below the piſtons is always the ſame; for while one is deſcending the other aſcends, and what is taken from the one is added to the other.

Having thus ſet aſide the valves which in ſome meaſure prevented the air from entering the barrel above the piſton, he next attempted to expel the air more perfectly out of the barrel than Mr. Smeaton had done, by making a better vacuum between the piſton and the top plate, which would allow more of the air to expand itſelf into the barrel from the receiver.

Upon Mr. Smeaton's plan, our author alſo contrived to connect the valves on the top plates with the receiver occaſionally, by means of a pipe and cock, by the turning of which the machine may be made to exhauſt or condenſe at pleaſure. This is done in the following manner: There is a croſs piece laid over the valves extending from one barrel to the other, which has a duct through it connected with a ſmall pipe ſtanding between the barrels; through this pipe the air paſſes into a duct at the bottom piece leading to the cock. In this piece is likewiſe the duct leading from the ciſtern to the cock; and with this cock alſo is connected the pipe leading to the receiver. The key is pierced with two holes, in ſuch a manner, that one of them will connect the pipe coming from the receiver with the duct in the bottom piece leading to the ciſtern, or with the other leading to the valves, as may be required for exhauſting or condenſing. The other hole through the key will open, occaſionally, to the atmoſphere either of theſe ducts round the cock: ſo that, having the direction of the air which paſſes through the valves under the command of this cock, the pump may exhauſt or condenſe at pleaſure; for when the key connects the pipe from the receiver and the duct leading to the ciſterns together, the pump will exhauſt; and when it connects the pipe with the duct leading to the valves, it will condenſe, as the other hole in the key at the ſame time opens to the atmoſphere the duct leading to the ciſterns, by which paſſage the air enters the barrel from the atmoſphere, is forced out at the valves, and through the pipe and cock into the receiver. In this part of the machine which is contrived for condenſation, the inventor has, by an additional part, endeavoured to get the air more perfectly out of the barrel.

We have ſeen, ſays he, that Mr. Smeaton, by making the piſton of his pump fit more exactly to the bottom of the barrel, and by ſhutting up the top to prevent the preſſure of the atmoſphere on the piſton valve, was able to get more of the air above it than could be effected in the common pump: but ſtill the difficulty, though ſo far removed, remains in the top of the barrel; for, as the piſton cannot be made to fit ſo exactly to the top plate, but that there will be ſome lodgement for air, it is impoſſible to expel it entirely. More perhaps might be expelled if the valve on the top could be made to open more eaſily, by removing the weight of air from it; for the atmoſphere preſſing on this valve will prevent its opening freely, in the ſame manner as, when preſſing on the piſton valve, it obſtructs the opening of that in the common pump.

The difficulty which Mr. Smeaton removed from the piſton valves, Mr. Prince endeavoured to remove from the valve on the top plate; that this valve, having the preſſure of the atmoſphere taken off, might open with the ſame eaſe as the piſton valve does in his pump. To effect this, there is connected with the duct on the bottom piece which conveys the air from the valves to the cock, a ſmall pump of the ſame conſtruction as the large one; having the barrel opening into a ciſtern, the piſton rod moving through a collar of leathers, and a valve near the top, through which the air is forced into the atmoſphere. This piſton is ſolid: becauſe the diameter being only half an inch, does not make it work hard. This pump, which is of one barrel only, he calls the valve pump; its chief uſe being to rarefy the air above the valves, or remove the weight of the atmoſphere from off them. To uſe this pump, it is neceſſary the key of the cock ſhould be pierced differently from that of Mr. Smeaton's; for, as the pipes round his are placed at equal diſtances, when the one from the bottom of the barrel is connected with that from the receiver to exhauſt it, the other from the valve on the top plate is opened to the atmoſphere by the other paſſage through the cock. But in order to rarefy the air above the valve in my pump, it is neceſſary this laſt paſſage ſhould be ſhut up when the valve pump is uſed. Inſtead, therefore, of placing the three ducts at equal diſtances round the cock, I have divided the whole into five equal parts; leaving the diſtance of one fifth part between the ducts leading from the ciſtern and the valves to the cock, and two fifths between each of theſe and the one leading from the cock to the receiver. By this adjuſtment, when the communication is open between the receiver and the valves for condenſation, the other hole through the cock opens the ciſterns to the atmoſphere; but when the communication is made between the ciſterns and the receiver for exhauſtion, a ſolid part of the key comes againſt the duct leading to the valves, and ſhuts it up, and the air which is forced out of the barrel paſſes through the atmoſphere into the valve pump; for the valve of the ſmall pump may be kept open while the great one is worked.

Upon this conſtruction alſo we are able to make the pump with two barrels like the common pump, which cannot be done conveniently where the lower valve is retained; becauſe it would be difficult to make the piſton in one barrel come exactly to the bottom, at the ſame time that the piſton in the other touched as exactly at the top; it would at leaſt require a nicety in the workmanſhip which would be troubleſome to execute.

In this pump the piſtons do not move the whole length of the barrels: there is a horizontal ſection made in them a little more than half way from the bottom where the top plates are inſerted. See Fig. 6. Pl. VI. By this means the pump is made more convenient and ſimple, as the head of it is brought down upon the top of the barrels in the ſame manner as in the common air pump. The barrels alſo ſtand upon the ſame plane with the receiver plate, and this plane is raiſed high enough to admit the common gauge of thirty-two or three inches, to ſtand under it without any inconvenience in working the pump, as the winch moves through a leſs portion of an arch at each ſtroke, than it would if the piſtons moved the whole length of the barrels.

There is alſo placed, between the barrels in this pump, on the croſs piece over the valves, a gauge to meaſure the degree of condenſation, having a free communication with the valves, cock, &c. This gauge is ſo conſtructed that it will alſo ſerve to meaſure the rarefaction above the valves when the air is worked off by the valve pump. It conſiſts of a pedeſtal, which forms a ciſtern for the mercury, a hollow braſs pillar, and glaſs tube hermetically ſealed at one end, which moves up and down in the pillar through a collar of leathers. The die of the pedeſtal is made of glaſs, as well to hold the quickſilver as to expoſe its ſurface to view, that it may be ſeen when the open end of the tube is put down into it or raiſed out of it. The body of the pillar is partly cut away to expoſe the tube to view in the ſame manner.

If the pump be uſed as a condenſer, the degree of condenſation is ſhewn by a ſcale marked on one edge of the pillar: if it be uſed as an exhauſter, the degree of the rarefaction of the air above the valves is ſhewn by a ſcale marked on the other edge of the pillar.

This gauge will alſo ſerve to ſhew when the valves have done playing, either with the weight of the atmoſphere on them or taken off. If we want to know when they ceaſe opening with the weight of the atmoſphere on them, draw the piſton of the valve pump into its barrel, to prevent any air eſcaping through that valve: in this ſituation work the great pump again; and if any air paſſes through the valves into the pipe, the gauge will riſe by condenſation. This condenſed air muſt then be let out by opening the communication at the cock with the outward air. By repeating this till the gauge riſes no longer, we may know the valves will open no more while the weight of the atmoſphere lies on them; and the rarefaction in the receiver can be carried no further. When the weight of the atmoſphere is to be removed, after conducting as in the former experiment, raiſe the open end of the tube above the ſurface of the mercury, and then work the valve pump; and the air will be rarefied over the valves and in the tube to the ſame degree (we may ſee when the valve of this pump has done playing, by unſcrewing the cap that covers it). The open end of the tube is then to be immerſed into the mercury, and the great pump worked. The air which paſſes through the valves will then raiſe the gauge by condenſation; and thus by alternately raiſing and depreſſing the tube, and working the two pumps in their turns, we may carry the rarefaction of the air in the receiver as far as the power of the pump will go. If one of Mr. Smeaton's pear gauges be uſed in the receiver, as he directs, the difference of the rarefaction in the two experiments may be known. And as the air above the valves may be rarefied to different degrees, it may be known, by the two gauges, what proportion the rarefaction above the valves bears to the degree of exceſs in the receiver. This condenſing gauge can be taken off, and a button ſcrewed into the hole in its ſtead, in any caſe wherein a greater degree of condenſation is required than the glaſs will bear. When a glaſs receiver is uſed, this gauge may be placed within it, where it will meaſure any degree of condenſation the receiver will bear without danger to the gauge; or the capacity of any receiver may be meaſured by this gauge before it is removed from its place, by ſhewing how many ſtrokes of the winch will throw one atmoſphere into the receiver; then turning the cock to prevent any air eſcaping, change the gauge for the button. When this is done, the degree of condenſation may be further meaſured by the number of ſtrokes.

As in caſes where great condenſation is required, there muſt be a great deal of labour, and a great ſtrain on the teeth of the wheel and piſton rods, on account of the great diameter of the piſtons *; to remedy this, Mr. Prince has fitted a condenſer of a ſmaller bore than the barrel of the great pump to the ciſtern of the valve pump, to be ſcrewed on occaſionally, by which the condenſation may be finiſhed, inſtead of the great pump. Or, to ſave the work and expence of this condenſer, the valve pump, if made a little larger, may be eaſily fitted for the ſame purpoſe, by having a plate made to ſcrew into the bottom of the cylinder occaſionally, with a valve on it opening into the ciſtern: a hole muſt alſo be made to be opened on the ſame occaſion near the top of the cylinder, to let air in below the piſton when this is drawn up above it.

The common gauge, which is generally placed under the receiver plate in this pump, is placed in the front, that it may be ſeen by the perſon who is working the pump, and that the place may be left free for other uſes.

The plate is ſo fixed to the pipe leading to the cock, that it may be taken off at pleaſure, and uſed as a transferer; or any tube or apparatus may be fixed to it to perform ſome experiments without removing it, which will ſave trouble and make leſs apparatus neceſſary.

---

* In this pump the piſtons are two inches diameter; ſo that there will be about forty-eight pounds added to the reſiſtance in opening the valves for every atmoſphere thrown into the receiver. P.

It is certainly an injury to an exhauſting pump to uſe it for condenſing. This operation ought always to be performed with a long ſyringe of ſmall bore. Such an inſtrument would coſt much leſs than the condenſing part of an air pump, and will at the ſame time have much more power. N.

The head of this pump is not divided as the common one is, to diſlodge the teeth of the wheel from the piſton rods when the pump is to be taken apart, but is made whole, except a ſmall piece in the back, where the wheel is let in; which makes it much more convenient to remove the head, or place it on the barrels. The wheel is freed from the piſton rods when required, by puſhing it into the back part of the head; and when it is drawn into its place and connected with them again, a button is ſcrewed into the ſocket of the axis behind, to keep it in its place. This makes the head leſs troubleſome to remove; but its chief uſe is to diſlodge the piſton rods from the wheel, that they may be put down into the ciſterns when the pump is not in uſe, where they will ſtand uncompreſſed, and retain their elaſticity better than if kept in the barrels. In theſe ciſterns they may alſo ſtand covered with oil, if neceſſary, as they are large enough to admit of it.

The principal joints of the pump are ſunk in ſockets, that the leathers which cloſe them may be covered with oil to prevent leaking[*].

For convenience, the lower part of the pump is fitted with drawers to contain the apparatus. A door opens behind one range to a place reſerved the whole height, to get at the under part of the receiver plate, and fix apparatus to it for ſome experiments. In this place ſtand the long tubes, and ſuch tall glaſſes belonging to the apparatus as will not go into the drawers. The barrels, &c. of the pump are covered with a caſe or head, which keeps them from duſt and accidents when the pump is not in uſe. The apparatus is ſecured between ſliders, &c. in the drawers, ſo that the whole machine may be eaſily removed in one body, without danger.

As there was no glaſs manufactory in Mr. Prince's vicinity, he ſent to Europe for his apparatus; but unluckily the gauges, with ſome other parts, had not been forwarded to him when he wrote his account. He had only a ſmall tube of two-tenths inch bore, which he uſed as a common gauge; but this was not ſufficient to determine the power of the pump.

All that he could ſay of the inſtrument was, that he found it much more convenient to uſe than one of the common ſort; that it would exhauſt a receiver much ſooner, and keep in order much longer, for being made without valves, which muſt depend on the ſpring of the air to open them. When a common pump in his poſſeſſion had been fitted up with valves, leathers, &c. at the ſame time with this, the valves of the common pump have become too dry and ſtiff to uſe, while this pump has continued in good order. He attributes this, in part, to the moiſture which the valves on the top plates receive from the piſtons every time the pump is uſed; the piſtons being always kept moiſtened with oil in the ciſterns, where they ſtand when the pump is not in uſe; and in part to the power which the piſtons have over theſe valves, by condenſing the air againſt them.

Fig. 6, Plate VI, repreſents a perpendicular ſection of one of the barrels, the two ciſterns, condenſing gauge, &c., where AB repreſents the barrel; CD is the ciſtern on which it ſtands; a a a a the leathered joint, ſunk into a ſocket and buried in oil: EF is the piſton; the cylindrical rod paſſing through a collar of leathers, GG, in the box HI. K ſhews the place of the valve on the top plate K, covered by the croſs piece MM, into which the pipe OO

[†] This, I find, is very effectual; having never known one of the joints ſecured in this way to leak, though the pump has ſtood for a long time; whereas a portable pump which I have, made by Mr. Nairne, London, has leaked, and repeatedly been refitted with new oiled leathers in the ſame time. P.

is ſoldered, that conveys the air from the valves to the duct going under the valve pump, as may be ſeen in Figure 8; o is part of the ſaid duct; p is the joint ſunk into a ſocket in the croſs piece PP, which connects the ciſterns, and has a duct through it leading to them. Into this duct open the ducts q and r, the firſt leading to the gauge in front of the pump, the other to the cock and receiver.

The other barrel is left out of the figure to ſhew ſome of the parts more diſtinctly, except Q, which is the top of the barrel retained and brought down out of its place to ſhew the top plate that ſhuts up the barrel, ſeparated from the box which contains the collar of leathers. S ſhews one of the holes in the plate over which the valve lies, and which is covered by R in the croſs piece. VV is the piſton ſhewing the valve open on the top, which is to prevent labour when the pump condenſes. WX is the ciſtern, in which is more diſtinctly ſeen the ſhoulder for the leather which cloſes the point between this and the barrel, and alſo the ſocket in which the oil lies over the leather. YZ is the condenſing gauge, with the orifice of the tube raiſed above the ſurface of the quickſilver. e e is the collar of leathers, through which the glaſs tube moves. i is a ſmall pipe coming up through the quickſilver to make a communication between the valves and the gauge.

Fig. 7. is a view of the upper ſurface of the top plate which cloſes the barrel, being ſoldered into it, ſhewing the place of the valve over the three ſmall holes, one of which only can be ſeen at S in Fig. 3.

Fig. 8. is a perpendicular ſection of the bottom piece, pipes, valve pump, cock, &c. at right angles with the other ſection, Fig. 6. AB is the pipe between the barrels, as repreſented in Fig. 6. The button o is here ſcrewed into the top inſtead of the gauge. CD is the valve pump and its ciſtern, e the place of the valve under the cap. EF the cock, ſhewing the duct through it leading to the atmoſphere. GH the pipe leading from it to the ſtem of the receiver plate, in which is the cock I, to ſhut up the duct when the plate is uſed as a transferer. KK is the plate. L, a piece to ſhut up the hole, into which tubes &c. are occaſionally ſcrewed to perform experiments without removing the plate. The dotted line at O ſhews the place of the ſcrew which preſſes the plate againſt the pipe: PQ the pipe and common gauge ſtanding in front of the pump.

Fig. 9. is a horizontal ſection of the cock, and pieces containing the ducts leading from it to the receiver, the ciſterns, and the valves on the top of the barrels. AB the duct connecting the ciſterns together. CD the duct leading from the ciſterns to the cock. GH the duct leading from the cock through the pipe AB (Fig. 8.) to the valves. DE the duct through the cock, which occaſionally connects the two laſt mentioned ducts with the duct EF, leading from the cock to the receiver. I, the duct in the cock leading to the atmoſphere, which, when connected with the duct at D, lets the air into the ciſterns and barrels for condenſation; the other duct through the cock at the ſame time connecting H and E. This duct alſo, when connected with E, reſtores the equilibrium in the receiver. KL is part of the duct leading from the ciſterns to the gauge. The dotted circles ſhew the places of the pipe and valve pump on the piece, and r the place where the air enters the valve pump from the duct GH, and is thrown into the atmoſphere when the pump exhauſts.

Fig. 10. ſhews the under ſurface of the boxes which contain the collars of leathers with the croſs piece which connects them together, having a duct through it, as repreſented by the

dotted line, through which the air paſſes from the valves to the pipe. This Fig. is deſigned chiefly to ſhew the places in which the valves play, as at I.

Fig. 11. is a ſide view of the pump, ſhewing the ſituation of the valve pump and handle of the cock; where A is the pump, and B the handle.

Fig. 12, Plate VII. is a perſpective view of Cuthbertſon's pump, with its two principal gauges ſcrewed into their places: theſe need not be uſed together, except in caſes where the utmoſt exactneſs is required; for in common experiments either of the two may be taken away, and a ſtop ſcrew put into its place. When the pear gauge is uſed, a ſmall round plate, large enough for the receiver to ſtand upon, muſt firſt be ſcrewed into a hole at A; but when this gauge is not uſed, this hole muſt be cloſed with a ſtop ſcrew. When all three gauges are made uſe of, and the receiver is exhauſted, the ſtop ſcrew B, at the bottom of the pump, muſt be unſcrewed, to admit the air into the receiver; but when the gauges are not all uſed, the ſtop ſcrew at A, or either of the other two which are in the place of the gauges, may be unſcrewed for this purpoſe.

In Figure 1, CD repreſents one of the barrels of the pump, F the collar of leathers, G a hollow cylindrical veſſel to contain oil; R is alſo an oil veſſel, which receives the oil that is driven with the air through the hole a a, when the piſton is drawn upwards; and when this is full, the oil is carried over with the air along the tube T into the oil veſſel G. c c is a wire which is driven upwards, from the hole a a, by the paſſage of the air; and as ſoon as this has eſcaped, falls down again by its own weight, ſhuts up the hole, and prevents any air from returning by that way into the barrel: at d d are fixed two pieces of braſs, to keep the wire c c in ſuch a direction as may preſerve the hole air-tight. H is a cylindrical wire which carries the piſton I, and is made hollow to receive a long wire q q that opens and cloſes the hole L, which forms the communication with the receiver ſtanding on the plate. m is part of a pipe, one end of which is ſcrewed into L, and the other into the centre of the receiver plate. M is a ſtop ſcrew, ſerving only to cloſe that hole. OP is a ſmall ſteel ſcrew, one end of which is ſcrewed into the wire q q, that opens and ſhuts the hole L; and upon the other end O is ſcrewed a nut, which, ſtopping in the ſmalleſt part of the hole, prevents the wire from being lifted or carried too high. This wire and ſcrew are more clearly ſhewn in Figure 2 and in Figure 6; they ſlide through a collar of leathers r r, Figure 2 and Figure 5, in the middle piece of the piſton. Figures 4 and 5 are the two main parts which compoſe the piſton; and when the pieces in Figures 3 and 6 are added to it, the whole is repreſented by Figure 2. Figure 5 is a piece of braſs, turned in a conical form, with a ſhoulder or ledge at the bottom; a long female ſcrew is cut in it, about two thirds of its length; and the remaining part of the hole, in which there is no ſcrew, is about the ſame diameter as the ſcrew part, except a thin plate at the end, which is of a width exactly equal to the thickneſs of q q. That part of the inſide of the conical piece of braſs, in which no thread is cut, is filled with oiled leathers with holes in them, through which q q can ſlide air-tight; there is alſo a male ſcrew with a hole in it, which is fitted to q q, and ſerves to preſs down the leathers r r. In Figure 4, a a a a is the outſide of the piſton, the inſide of which is turned exactly to fit the outſide of Figure 5. b b are round leathers, about 60 in number; c c is a circular plate of braſs, of the ſize of the leathers; and d d is a ſcrew, which ſerves to preſs them down as tight as is neceſſary. The male ſcrew at the end of Figure 3, is made to fit the female ſcrew in Figure 5: now if Figure 6 be puſhed into Figure 5, this into Figure 4, and Figure 3 ſcrewed into the end of Figure 5, theſe will compoſe the whole piſton, as repreſented by Figure 2. H in Figure 1 repreſents the ſame part as H in Figure 2, and is that to which the rack is fixed. If this therefore be drawn upwards, it will make Figure 5 ſhut cloſe into Figure 4, and drive out the air above it; and when it is puſhed downwards, it will open as far as the ſhoulders a a Figure 4 will permit, and ſuffer the air to paſs through. A A Figure 7, is the receiver plate; B B is a long ſquare piece of glaſs ſcrewed to the undermoſt ſide of the plate, through which a hole is drilled, correſponding with that in the centre of the receiver plate, and with the three female ſcrews b b c.

Suppoſe the piſton to be at the bottom of the barrel, and a receiver to ſtand upon the plate, the inſide of the barrel, from the top of the piſton to a, is full of air, and the piſton ſhut; when drawn upward, by the hollow cylindrical wire H, it will in its courſe drive the air before it, through the hole a a, into the oil-veſſel R, and out into the atmoſphere by the tube T. The piſton will then be at the top of the barrel at *a*, and the wire *q q* will ſtand nearly as it is repreſented in the figure, juſt raiſed from the hole L, and prevented riſing higher by means of the nut *o*. While the piſton is moved upwards, the air will expand in the receiver, and be driven along the bent tube *m* into the inſide of the barrel. Thus the barrel will be filled with air, which, as the piſton riſes, will be rarefied, in proportion as the capacity of the receiver pipes and barrel is to the capacity of the barrel alone. When the piſton is moved downwards again by H, it will force the conical part, Figure 5, out of the hollow part, Figure 4, as far as the ſhoulders *a a*: Figure 2 will reſt upon *a a* Figure 4, which will then be ſo far open as to permit the air to paſs freely through it, while at the ſame time the end of *q q* is forced againſt the top of the hole, and cloſes it in order to prevent any air from returning into the receiver. Thus the piſton, while moved downwards, ſuffers the air to paſs out between the ſides of Figure 4 and 5, and when it is at the bottom of the barrel will have the column of air above it; conſequently, when drawn upward, it will ſhut and drive out this air, and by opening the hole L give a free paſſage to more air from the receiver. This proceſs being continued, the air will be exhauſted out of the receiver as far as its expanſive power will permit; for in this machine there are no valves, as in the common air pumps, to be forced open by the air in the receiver, which, when its elaſticity is diminiſhed, it becomes unable to effect; but every thing is contrived to open by the motion of the piſton, and there is nothing to prevent the air from expanding to its utmoſt degree.

In exhauſting with this machine, no other directions need be obſerved, but ſuch as are common to all air pumps; nor is any peculiar care required to preſerve it in order, except that the oil veſſel G be always kept about half full of oil. When it has ſtood a conſiderable time without being uſed, it will be proper to draw a table-ſpoonful or two through it, by pouring it into the hole in the middle of the receiver plate when the piſton is at the bottom of the barrel; then, by moving the winch backward and forward, to raiſe and depreſs the piſton, the oil will be drawn through all the parts of the machine; and what is more than is neceſſary in the inſide will be forced out through the tube T into the oil veſſel G. Near the top of the cylindrical wire H is a ſquare hole, which is intended to let in ſome of the oil from the veſſel G, that the oiled leathers through which the wire *q q* ſlides, may always be ſufficiently ſupplied with it.

When the pump is required to condenſe, either at the ſame time that it exhauſts, or ſeparately, the piece which contains the bent tube T muſt be taken away, and Figure 8 put into its place, and faſtened by the ſcrews which had before confined that piece. Figure 8 is drawn in the plate as it is made for a double-barrelled pump; but for a ſingle barrel only one piece is uſed, repreſented by b a a, the double piece being cut off at the dotted line a a. In this piece is a female ſcrew, to receive the end of a long braſs tube; to which a bladder, if ſufficient for the experiment, muſt be tied; or elſe a glaſs, properly confined for this purpoſe, muſt be ſcrewed to it. Then the air, which is exhauſted out of a receiver ſtanding on the plate, will be forced into the bladder, or glaſs connected with the braſs tube. But if the pump be double-barrelled, the apparatus, as repreſented by Figure 8, muſt be uſed, and the long braſs tube ſcrewed into the female ſcrew at C.

Figures 9 and 10 repreſent the two gauges; namely, the ſyphon gauge, and the barometer or long gauge. When theſe are uſed, Figure 9 muſt be ſcrewed into the female ſcrew c b, or into that at the other end c Figure 7, and Figure 10 into the female ſcrew a b Figure 7.

If it be uſed as a ſingle air pump, either to exhauſt or condenſe, the ſcrew K, which faſtens the rack to the cylindrical wire H, muſt be taken out; then turning the winch till this wire is depreſſed as low as poſſible, the machine will be rendered fit to exhauſt as a ſingle air pump: and if it be required to condenſe, the directions given in the laſt paragraph but one, with regard to the bent tube T, and Fig. 8, muſt be obſerved.

Mr. Cuthbertſon's treatiſe contains a relation of various experiments made with this air pump, which ſhew its great power of exhauſting, and are in other reſpects entitled to the conſideration of philoſophers. With the double ſyphon gauge, and alſo with the long gauge, compared with an attached barometer, in which the mercury had been repeatedly boiled, the difference between the heights of the mercurial columns proved to be no more than one fortieth of an inch, the barometer then ſtanding at 30 inches. This gives an exhauſtion of 1200 times. On ſome occaſions, when the air was in a very dry ſtate, he obſerved the difference to be as low as one hundredth of an inch, which indicates more than double the rarefaction.

On a review of theſe improvements of the air pump, I perceive in each inſtrument ſo happy a combination of philoſophical acuteneſs and mechanical ſkill, that it is with a degree of diffidence that I venture to ſpeculate on their reſpective merits and blemiſhes. There is no proviſion to open the upper fixed valve of Prince's greater barrel, except the difference between the preſſures of the elaſtic fluid on each ſide of the ſtrip of bladder; and this may reaſonably be inferred to limit the power of his ſmall pump. In Cuthbertſon's pump, the ſame valve is expoſed to the action of the atmoſphere, together with that of a column of oil in the oil veſſel. The miſchief in either inſtrument is probably trifling; but in both the valve might have been opened mechanically. If this were done, the ſmall pump of Prince might perhaps be unneceſſary in moſt ſtates of the atmoſphere. With regard to the lower valves, Cuthbertſon, by an admirable diſplay of talents as a workman, has inſured their action. Prince, on the other hand, has, by the proceſs of reaſoning, ſo far improved the inſtrument that no valves are wanted. In this reſpect he has the advantage of ſimplicity and cheapneſs, with equal effect. The mechanical combination of Cuthbertſon's pump reduces the operation to one ſimple act of the handle: but Prince's engine requires ſome manipulation with regard to the play of the ſmall pump; though this might have been remedied by a more ſkilful diſpoſition of the firſt mover.

The moſt perfect ſcheme for an air pump, taking advantage of the labours of theſe judicious operators, ſeems to be that in which two piſtons of the conſtruction of Prince ſhould work in one barrel; one piſton being fixed at the lower end of the rod, and the other at the middle. The lower piſton muſt come clear out of the barrel when down, and work air-tight through a diaphragm at an equal diſtance from the effective ends of the barrel. In the diaphragm muſt be a metallic valve of the form of Cuthbertſon's lower valve, but with a ſhort tail beneath, that it may be mechanically opened when the piſton comes up. Above the diaphragm, muſt work the other piſton ſimilar to the firſt; but as it cannot quit the barrel when down, a ſmall portion of the barrel muſt be enlarged juſt above the diaphragm, ſo that the leathers may be clear in that poſition. Laſtly, the top of the barrel muſt be cloſed and fitted with a valve and oil veſſel, according to the excellent contrivance of Cuthbertſon.

If we ſuppoſe the workmanſhip of ſuch a pump to leave the ſpace between the diaphragm and lower piſton, when up, equal to one-thouſandth part of the ſpace paſſed through by the ſtroke of that piſton, the rarefaction produced by this part of the engine will in theory bear the ſame proportion to that of the external air. And the ſame ſuppoſition applied to the upper piſton would increaſe the effect one thouſand times more. Whence the rarefaction would be one million times. How far the practical effect might fall ſhort of this from the imperfections of workmanſhip, or the nature of the air, which in high rarefactions, may not diffuſe itſelf equally through the containing ſpaces, or from other yet unobſerved circumſtances, cannot be deduced from mere reaſoning without experiment.

VI.

Uſeful Notices reſpecting various Objects.—A Method of preventing Heat in Grinding—Concerning Gold, Silver, and other Metals reduced into very thin Leaves by the Hammer—Globules for Microſcopes—On the Plumb-Line, and Spirit-Level.

1. *On the Art of Grinding.*

A CURIOUS fact was mentioned to me, ſome years ago, reſpecting grinding, which promiſes to be of ſome uſe in the arts. Daily experience, as well as philoſophical experiment, ſhews us that heat is produced or developed by friction. The fact of ſparks flying from a dry grindſtone when a piece of iron or ſteel is applied to its ſurface during the rotation, has been ſeen by every one. The heat produced during this proceſs is ſuch that the ſteel very ſoon becomes ignited, and hard tools are very frequently ſoftened and ſpoiled, for want of care during the grinding. When a cylindrical ſtone is partly immerſed in a trough of water, the rotation muſt be moderate and the work ſlow, otherwiſe the water would ſoon be thrown off by the centrifugal force; and when this fluid is applied by a cock from above, the quantity is too ſmall to preſerve the requiſite low temperature. It is even found, that the point of a hard tool, ground under a conſiderable maſs of water, will be ſoftened if it be not held ſo as to meet the ſtream; ſparks being frequently afforded even under the water. My informant aſſured me, that fine cutlery is ground in Germany on a cylinder of a peculiar kind of pottery inſtead of ſtone, upon the face of which pulverifſed hone is occaſionally applied by means of tallow. The peculiar advantage of this kind of pottery was ſtated to be, that it never heats, however rapid the motion.

This object ſeemed to deſerve an experimental inveſtigation. The three bodies ſubjected to experiment were pottery, pulveriſed ſilex, and tallow. The effect, namely that it could ſupport violent and rapid friction without increaſe of temperature, appeared at firſt ſcarcely credible. The mind was rather diſpoſed to reject the evidence, than inveſtigate the cauſe. No indication was given reſpecting the nature of the pottery. It did not ſeem probable that any peculiarity in the ſiliceous ſand ſhould produce this deſirable effect; but an eaſy proceſs of reaſoning might have pointed out the effect of the tallow, which is indeed curious. I cannot however aſſume the merit of inveſtigating the ſubject *à priori*; for my apparatus was ready for experiment, and the fact ſpoke for itſelf before I had ſyſtematiſed the notions which occurred to me.

The pottery grindſtone was not eaſily attainable. I therefore procured a Newcaſtle grindſtone of a fine grit and ten inches in diameter; and alſo a block of mahogany to be uſed with emery on its face. Both the ſtone and the wooden block were mounted on an axis to be occaſionally applied between the centres of a ſtrong lathe. In this ſituation both were turned truly cylindrical, and of the ſame diameter. The face of the wood was grooved obliquely in oppoſite directions, to afford a lodgement for the emery. The face of the ſtone was left ſmooth, and there was a trough of a proper ſize applied beneath the ſtone to hold water. The grindſtone was then uſed with water, and the wooden cylinder was faced with emery and oil. The inſtrument ground was a file, out of which it was propoſed to grind all the teeth. The rotation was produced by the mechaniſm of the lathe; the velocity being ſuch as to turn the grinding apparatus about five revolutions in a ſecond. The ſtone operated but ſlowly, and the water from the trough was ſoon exhauſted, with inconvenience to the workman, who could ſcarcely be defended from it but by ſlackening the velocity. The emery cylinder cut rather faſter. But notwithſtanding the friction was made to operate ſucceſſively and by quick changes on the whole ſurface of the file, it ſoon became too much heated to be held with any convenience; and when a cloth was uſed to defend the hand, the work not only became awkward, but the heat increaſed to ſuch a degree that the oil began to be decompoſed, and emitted an empyreumatic ſmell. The ſtone was then ſuffered to dry, and the file tried upon its face. It almoſt immediately became blue, and ſoon afterwards red-hot. Both the cylinders were then covered with tallow, by applying the end of a candle to each while revolving, and emery was ſprinkled upon the cylinder of wood. The ſame tool was then applied to the grindſtone in rapid motion. At the firſt inſtant the friction was ſcarcely perceptible; but very ſpeedily afterwards the zone of tallow preſſed by the tool became fuſed, and the ſtone cut very faſt. The tool was ſcarcely at all heated for a long time; and when it began to feel warm, its temperature was immediatly lowered by removing it to a new zone of the cylinder. The ſame effect took place when the experiment was repeated with the wooden cylinder.

It is not difficult to explain this by the modern doctrine of heat. When oil was uſed upon the wooden cylinder, the heat developed by the friction was employed in raiſing the temperature of the tool and of the fluid oil. But when tallow was ſubſtituted inſtead of the oil, the greateſt part of the heat was employed in fuſing this conſiſtent body. From the increaſed capacity of the tallow, when melted, this heat was abſorbed, and became latent, inſtead of being employed to raiſe the temperature: and whenever, by continuing the proceſs, the tallow already melted began to grow hot, together with the tool, it was eaſy to reduce the temperature again by employing the heat on another zone of conſiſtent tallow. I uſed theſe two cylinders with much ſatisfaction, in a conſiderable quantity of work.

In this ſtage of experiment, I concluded that the cylinder of pottery mentioned to be uſed in Germany was either of no particular utility beyond that of a common grindſtone, or that the report might be inaccurate in this reſpect. But it happened that the ſmall ſtone here mentioned was laid aſide for about three years. At the end of this term it was again brought into uſe, and it was found that the tallow had undergone ſome change, either from the ſtone itſelf, or the action of the external air, which enabled it to defend the grit much more effectually than at firſt. It ſeemed to have become leſs fuſible. I think it probable that this might not have happened with a pottery cylinder, or at leaſt that it would have been more eaſy to clean and reſtore the ſurface to its firſt ſtate.

2. *Concerning Gold, Silver, and other Metals reduced into very thin Leaves by the Hammer.*

IT is generally thought by chemiſts, and others, that leaf gold conſiſts of the metal in a high ſtate of purity. It is never pure, becauſe pure gold is too ductile to be worked between the gold-beaters' ſkin. The neweſt ſkins will work the fineſt gold, and make the thinneſt leaf, becauſe they are the ſmootheſt. Old ſkins, being rough or foul, require coarſer gold. The finer the gold, the more ductile; inſomuch that pure gold, when driven out by the hammer, is too ſoft to force itſelf over the irregularities, but would paſs round them, and by that means become divided into narrow ſlips. The fineſt gold for this purpoſe has three grains of alloy in the ounce, and the coarſeſt twelve grains. In general the alloy is ſix grains, or one-eightieth part. That which is called pale gold contains three pennyweights of ſilver in the ounce. The alloy of leaf gold is ſilver, or copper, or both, and the colour is produced of various tints accordingly. Two ounces and two pennyweights of gold is delivered by the maſter to the workman, who, if extraordinarily ſkilful, returns two thouſand leaves, or eighty books, of gold, together with one ounce and ſix pennyweights of waſte cuttings. Hence one book weighs 4.8 grains; and as the leaves meaſure 3.3 inches in the ſide, the thickneſs of the leaf is one two hundred and eighty-two thouſandth part of an inch.

Silver leaf is ſaid to be pure ſilver. It is extenſible in this way when compared with gold, rather more than in the proportion of the ſpecific gravities. Some leaf ſilver which I tried was thicker than the gold in the proportion of ſeven to four. So that the weight of metal covering equal ſurfaces approached to equality.

The yellow metal called Dutch gold is fine braſs. It is ſaid to be made from copper-plates, by cementation with calamine, without ſubſequent fuſion. Its thickneſs, compared with that of leaf gold, proved as 19 to 4, and under equal ſurfaces it is conſiderably more than twice as heavy as the gold.

The Dutch ſilver appears to be tin, not only from its habitudes with re-agents, but from the conſideration that there is no other cheap white metal of ſufficient ductility. It is ſomewhat more than ten times as thick as gold leaf, and about two and a half times as heavy under equal ſurfaces.

The thinneſt tin-foil in ſmall ſheets for ſilvering looking-glaſſes, is about one-thouſandth of an inch thick, or near three hundred times the thickneſs of gold leaf. It uſually contains lead.

3. *Globules for Microſcopes.*

AT the beginning of the preſent century, the ſimple microſcope was very much uſed. Among other advantages, it poſſeſſes the very deſirable requiſites of ſimplicity and cheapneſs. In particular it is an inſtrument not difficult to be conſtructed by ſuch ingenious philoſophers as by narrow circumſtances and remote ſituations are obliged to have recourſe to their own ſkill and ingenuity for experimental implements. The hiſtory of natural philoſophy abounds with inſtances of eminent men who come under this deſcription. To theſe at leaſt it will be of importance to know a ready method of forming very bright ſpherules of glaſs for microſcopic uſes. The uſual method has been to draw out a fine thread of the ſoft white glaſs called cryſtal, and to convert the extremity of this into a ſpherule by melting it at the flame of a candle. But this glaſs contains lead, which is diſpoſed to become opake by partial reduction, unleſs the management be very carefully attended to. I find that the hard glaſs uſed for windows ſeldom fails to afford excellent ſpherules. This glaſs is of a clear bright green colour when ſeen edgeways. A thin piece was cut from the edge of a pane of glaſs leſs than one-tenth of an inch broad. This was held perpendicularly by the upper end, and the flame of a candle was directed upon it by the blow-pipe at the diſtance of about an inch from the lower end. The glaſs became ſoft; and the lower piece deſcended by its own weight to the diſtance of about two feet, where it remained ſuſpended by a thin thread of glaſs about one five-hundredth of an inch in diameter. A part of this thread was applied endways to the lower blue part of the flame of the candle without the uſe of the blow-pipe. The extremity immediately became white-hot, and formed a globule. The glaſs was then gradually and regularly thruſt towards the flame, but never into it, until the globule was ſufficiently large. A number of theſe were made, and, being afterwards examined by viewing their focal images with a deep magnifier, proved very bright, perfect, and round.

4. *On the Plumb-Line and Spirit-Level.*

THE ſpirit-level is an inſtrument ſubſtituted inſtead of the plumb-line for determining the level. It ſeldom happens that wire can be procured ſo fine as the one-thouſandth of an inch in diameter. Upon examining this with the microſcope, it ſcarcely ever proves truly cylindrical. It may with reaſon be ſuppoſed that wire, from its curl upon the bobbin, and its elaſticity, may not, even when loaded with a conſiderable weight, form an accurate right line. As the radius of a circle is equal to 206265 ſeconds of meaſure nearly, the diameter of ſuch a wire would cover a ſpace on the line equal to 206 ſeconds on an inſtrument of one inch radius, or 17 ſeconds if the radius were 12 inches. How far this quantity, in the biſection of two dots, of near half a minute broad, may be affected by the cauſes of error here mentioned, muſt be aſcertained, in each particular inſtance, by careful experiments. But theſe conſiderations, joined to the circumſtance that the ſpirit-level is ſomewhat more portable and eaſy of application, have tended to bring this laſt very delicate inſtrument into eſtimation with aſtronomers.

The ſpirit-level conſiſts of a tube of glaſs nearly filled with the pureſt ardent ſpirit, or with highly rectified ether. In order that this inſtrument may anſwer the intended purpoſe, it is neceſſary that the inner ſurface ſhould not be cylindrical or ſtraight on that ſide which is placed uppermoſt when in uſe. Suppoſe this tube to be laid nearly in the horizontal poſition; the ſpirit will occupy the lower ſpace, and the empty part above will exhibit the appearance of a bubble, which will run to the higher end. It follows therefore, ſuppoſing the tube to be ſlightly curved, and the convexity uppermoſt, that the bubble will be near the middle when the tube is level. In the portable ſpirit-level, the tube is properly ſet in braſs, and fixed by means of ſcrews in a ſmall braſs trough, the bottom of which is ground very ſtraight. The ſcrews are uſeful to place the bubble in ſuch a poſition that the lower ſurface of the trough may be parallel to a tangent ſuppoſed to be applied to the middle point of the curve of the level. The adjuſtment is effected without much difficulty, by placing the level on an adjuſtable plane, and then reverſing it. If the bubble ſtand accurately in the ſame poſition between two marks made on the tube in both ſituations of the level, it follows that neither end of the plane nor of the lower ſurface of the frame of the level is elevated; or in other words, that every ſurface to which the level may be applied, and on which the bubble ſtands in the poſition here mentioned, is horizontal.

This eaſy praxis may be effected in various ways, according to the nature and figure of the inſtrument of which the poſition is to be determined: but the accuracy of the reſult will depend upon the ſenſibility of the level; that is to ſay, the ſpace paſſed over by the bubble for every minute or ſecond of the quadrant, and the certainty with which, under circumſtances preciſely ſimilar, it ſhall arrive at the ſame poſition. In the beſt levels the curve muſt be circular; for in ſuch the bubble will move with more activity, ſettle itſelf with more certainty, and deſcribe equal ſpaces by equal changes of inclination. An ordinary good ſpirit-level will exhibit a movement of upwards of half an inch for each minute of inclination, and alter the poſition of its bubble by a change of five ſeconds, or leſs. In ſuch a tube the radius of the curve will be about 150 feet. But extraordinary levels are much more delicate. De la Lande* ſpeaks of a level filled with ether, the bubble of which paſſed over fourteen inches by equal ſpaces of one-tenth of an inch for every ſecond. The radius of this curve was conſequently 1719 feet, or near one-third of a mile.

The tubes of ſpirit-levels are ſelected by trial. If a long piece of tube be nearly filled with ardent ſpirit, and corked at the ends, the run of the bubble may be tried with a ſuitable inſtrument called the level-trier, throughout the whole length on all ſides. By this means it may be known whether, and in what parts, it may be deſirable to divide the tube for the purpoſes of filling and cloſing. It is remarkable that theſe tubes in general prove either good throughout, or good for nothing; for it ſeldom happens, where one good level can be taken, that the remainder is unſerviceable. A reſpectable mathematical inſtrument-maker aſſures me, that he finds it a good practice to go to the glaſs-houſe and cauſe the tubes to be drawn without ſuffering them to be turned round. The uſual practice in drawing tubes is, that two workmen extend a piece of blown glaſs, while hot, by drawing the ends aſunder; while a third cools the thinner parts, by blowing the air againſt it with a hat. To prevent the glaſs from bending downwards by its own weight, the workmen are in the habit of turning the iron pipes they hold in their hands, both the ſame way, as they walk backwards. This is the practice which, by producing undulations and irregularities in the tube, is ſuppoſed to render them unfit for ſpirit-levels. I have remarked, that ſuch tubes as have veins, or extended bubbles, running lengthways very ſtraight through the ſubſtance, have afforded good levels.

---

* In his Aſtronomie; not now at hand. I ſpeak from memory; but have no doubt of the numbers, nor of the fact.

But the moſt regular and accurate levels are obtained by grinding the inſide of the tube. I do not know that this operation requires any particular management capable of being deſcribed in words; though there is no doubt but in this, as well as in every other mechanical proceſs, facility and ſkill will be acquired by practice. A cylindrical piece of wood is turned ſo as to go eaſily through the portion of tube intended to be ground. It is then worked in the tube with water and fine emery in the uſual way. As ſoon as the poliſh has by this means diſappeared on one ſide, the tube is cleaned, filled, and tried; and accordingly as its figure proves to be more or leſs ſtraight or curved, the grinding is either repeated or diſcontinued. Some operators poliſh the inſide again after grinding; but this has not been found to increaſe their ſenſibility.

From the great delicacy of the ſpirit-level, compared with the few obſervations here preſented on the plumb-line, the former inſtrument may appear greatly to deſerve the preference. Aſtronomers are not however agreed on this point. When a ſpirit-level is adjuſted by reverſing, at a certain temperature, and both ends of the bubble marked, it may be allowed that the inſtrument may be ſucceſsfully applied to uſe. But if the temperature be raiſed, the ſpirit will expand, and of courſe the bubble will become ſhorter. Whence it appears neceſſary that a diviſion and adjuſting-piece ſhould be applied, from experiment, to aſcertain the true ſtation of the bubble at different temperatures; and even this application ſeems ſcarcely adequate to ſupply the place of repeated adjuſtments. The variation of the bubble will differ according to the quantity of ſpirit contained in the tube. In two good levels, of nearly the ſame magnitude and figure, I found it amount to one-fifth of an inch for every ten degrees of Fahrenheit. The bubble therefore may be one inch longer in winter than in ſummer, which in theſe individual levels amounts to near one-third of the ſummer length. The curvature of a ſpirit-level will alſo vary from unequal temperature; ſuch, for example, as may ariſe from one end of the tube being touched or breathed upon, while the other end is left at the original temperature. The error from each of theſe cauſes may amount to ſeveral minutes, as is eaſily ſhewn by trial; but I do not find that the preſence or abſence of ſunſhine cauſes any perceptible difference. It is probable that the rays may not ſpeedily alter the temperature, on account of the tranſparency. And with regard to theſe three laſt ſources of error, it muſt be allowed that they are eaſy to be avoided, and indeed not likely to be preſent in the operations of accurate obſervers.

MATHEMATICAL CORRESPONDENCE.

QUESTION I. *Anſwered by the Propoſer.*

LET a be the length of the whole line, and x, y, z, v, w, &c. the parts into which the half of it is to be divided. Then $\frac{a}{x} = \frac{a-x}{y} = \frac{a-x-y}{z} = \frac{a-x-y-z}{v}$ &c. by the queſtion: Or $y = \frac{x}{a}(a-x)$, $z = \frac{x}{a}(a-x-y) = \frac{x}{a}(\overline{a-x} - \frac{x}{a} \times \overline{a-x}) = \frac{x}{a}(\overline{a-x} \times \overline{1-\frac{x}{a}}) = \frac{x}{a}(\overline{a-x} \times \frac{a-x}{a}) = \frac{x}{a^2}(a-x)^2$, $v = \frac{x}{a}(a-x-y-z) = \frac{x}{a}(\overline{a-x} - \frac{x}{a} \times \overline{a-x} - \frac{x}{a^2} \times \overline{a-x}|^2) = \frac{x}{a} \times \overline{a-x}\,(\overline{1-\frac{x}{a}} - \frac{x}{a^2} \times \overline{a-x}) = \frac{x}{a} \times \overline{a-x}\,(\frac{a-x}{a} - \frac{x}{a^2} \times \overline{a-x}) = \frac{x}{a} \times \overline{a-x}\,(\frac{a \times \overline{a-x} - x \times \overline{a-x}}{a^2}) = \frac{x}{a^3} \times \overline{a-x}|^2 \times (a-x) = \frac{x}{a^3} \times \overline{a-x}|^3$, $w = \frac{x}{a}(a-x-y-z-v) = \frac{x}{a}(a-x-\frac{x}{a} \times \overline{a-x} - \frac{x}{a^2} \times \overline{a-x}|^2 - \frac{x}{a^3} \times \overline{a-x}|^3) = \frac{x}{a^4} \times \overline{a-x}|^4$, &c. But $x+y+z+v+w$, &c. $= \frac{a}{2}$, by the queſtion. Whence, alſo, $x + \frac{x}{a}(a-x) + \frac{x}{a^2}(a-x)^2 + \frac{x}{a^3}(a-x)^3 + \frac{x}{a^4}(a-x)^4$ &c. to n terms $= \frac{a}{2}$; which laſt being a geometrical ſeries, whoſe firſt term is x, and the ratio $\frac{a-x}{a}$, its ſum, by the common rule, is

$$\frac{x\,(\overline{\frac{a-x}{a}}|^n - 1)}{\frac{a-x}{a} - 1} = \frac{a}{2},$$

from whence x is found $= a \times (1 - \frac{1}{2^{\frac{1}{n}}})$; and conſequently the ſeveral diviſions of the half line are $a \times (1 - \frac{1}{2^{\frac{1}{n}}})$, $\frac{a}{2^{\frac{1}{n}}}(1 - \frac{1}{2^{\frac{1}{n}}})$, $\frac{a}{2^{\frac{2}{n}}}(1 - \frac{1}{2^{\frac{1}{n}}})$, $\frac{a}{2^{\frac{3}{n}}}(1 - \frac{1}{2^{\frac{1}{n}}})$, &c. where n is any number whatever.

QUESTION II. *Anſwered by the Propoſer.* See Fig. 12. Plate VI.

LET AF $= x$, AC $= r$, $c =$ celerity, or velocity in the circumference, and F $=$ variable force in the ſine EF.

Then CE : FC : : EG : EK by ſin. Δ^s. or $r : r - x :: c : \frac{c}{r}(r-x) = v =$ velocity in FE. But, by the laws of variable forces, $F\dot{t} = \dot{v} = -\frac{c}{r}\dot{x}$, and $\dot{t} = \frac{\text{flux. EF}}{v} = \frac{\text{flux. } \sqrt{2rx - x^2}}{\frac{c}{r}(r-x)} = \frac{r\dot{x} - x\dot{x}}{\sqrt{2rx - x^2}} \times \frac{r}{c\,(r-x)}$; whence $\frac{F\,(r-x)\,\dot{x}}{\sqrt{2rx - x^2}} \times \frac{r}{c\,(r-x)} = -\frac{c}{r}\dot{x}$,

or $\frac{Fr}{c\sqrt{2rx - x^2}} = -\frac{c}{r}$, and conſequently $F = -\frac{c^2}{r^2}\sqrt{2rx - x^2} = -\frac{c^2}{r}\sqrt{\frac{2rx - x^2}{r^2}}$.
But when the body, in revolving, comes to D, x then becomes $= r$; in which caſe, $F = \frac{c^2}{r}$, or F is as $\frac{c^2}{r}$, or as the ſquare of the velocity divided by the radius.

NEW MATHEMATICAL QUESTIONS.

QUESTION V. *By W. SIMPSON.*

A BOTTLE which held exactly 5 oz. 106 gr. of diſtilled water, was capable of containing $5\frac{1}{2}$ oz. and 170 gr. of a ſolution of common ſalt in diſtilled water: Required the weight of the ſalt held in ſolution, its ſpecific gravity being 2.8?

QUESTION VI. *By J. B.*

THE mean annual temperature of any two latitudes, in the ſame hemiſphere, being given, to determine the mean annual temperature of any other latitude in that hemiſphere?

To Mr. NICHOLSON, Editor of "The Journal of Natural Philoſophy, Chemiſtry, and the Arts."

SIR,

I DO not know whether you will conſider the following as of ſufficient importance to merit a place in your valuable publication;—if ſo, I ſhall perhaps occaſionally, in future, trouble you with other attempts of a ſimilar nature.

Being about four years ago engaged in ſome mathematical ſpeculations, in the courſe of which the reſolution of pretty high equations became frequently neceſſary, I was induced to ſet about the inveſtigation of an eaſy arithmetical rule for the ſolution of an adfected quadratic, in order to ſhorten the buſineſs of approximation.—It is, indeed, you know, on account of the prolixity of the ordinary methods by completing the ſquare or exterminating the ſecond term, that Dr. Halley's method of obtaining the approximate value of the root, by taking ſuch terms of the aſſumed equation as involve the ſquare of the converging quantity, is now ſeldom uſed; ſuch only as contain its firſt power being uſually admitted into the calculation. The mode which I adopted for this purpoſe will be beſt underſtood by a ſtatement of the principles from which it is deduced.

Let $x^2 + ax$ be $= b$. Then will the affirmative value of x be $= \frac{1}{2}(\sqrt{a^2 + 4b} - a.) =$
$\frac{1}{2} \times : \frac{4.b}{2.a} - \frac{4^2.b^2.}{2.4.a^3.} + \frac{1.3.4^3.b^3.}{2.4.6.a^5.} - \frac{1.3.5.4^4.b^4.}{2.4.6.8.a^7.} + \frac{1.3.5.7.4^5.b^5.}{2.4.6.8.10.a^9.}$ &c. . . . . . .
$= \frac{b}{a} - \frac{b^2.}{a^3.} + \frac{2.b^3.}{a^5.} - \frac{5.b^4.}{a^7.} + \frac{14.b^5.}{a^9.}$ &c. But in order to obtain this latter ſeries, we have only to divide b by a, adding to the original diviſor a, every time of repeating the diviſion, double the quotient already found, together with the term which ſuch new diviſion ſhall produce, as in the common operation for extracting the ſquare root, as follows:

$a +$.

$$a+\frac{b}{a}\;\Big)\;b\;\Big(\;\frac{b}{a}-\frac{b^2.}{a^3.}+\frac{2.b^3.}{a^5.}-\frac{5.b^4.}{a^7.}+\frac{14.b^5.}{a^9.}-\frac{42.b^6.}{a^{11}.}\ \text{\&c.}$$

$$b+\frac{b^2.}{a^2.}$$

$$a+\frac{2b}{a}-\frac{b^2.}{a^3.}\;\Big)\;\div-\frac{b^2.}{a^2.}$$

$$-\frac{b^2.}{a^2.}-\frac{2.b^3.}{a^4.}+\frac{b^4.}{a^6.}$$

$$a+\frac{2b}{a}-\frac{2b^2.}{a^3.}+\frac{2b^3.}{a^5.}\;\Big)\;\div+\frac{2b^3.}{a^4.}-\frac{b^4.}{a^6.}$$

$$+\frac{2.b^3.}{a^4.}+\frac{4.b^4.}{a^6.}-\frac{4.b^5.}{a^8.}\ \ldots\ \text{\&c.}$$

$$a+\frac{2b}{a}-\frac{2b^2.}{a^3.}+\frac{4.b^3.}{a^5.}-\frac{5b^4.}{a^7.}\;\Big)\;\div-\frac{5.b^4.}{a^6.}+\frac{4b^5.}{a^8.}\ \ldots\ \text{\&c.}$$

$$-\frac{5b^4.}{a^6.}-\frac{10.b^5.}{a^8.}\ \ldots\ \text{\&c.}$$

$$a+\frac{2.b}{a}-\frac{2.b^2.}{a^3.}+\frac{4.b^3.}{a^5.}-\frac{10.b^4.}{a^7.}+\frac{14.b^5.}{a^9.}\Big)\div+\frac{14.b^5.}{a^8.}\ \ldots\ \text{\&c.}$$

$$+\frac{14.b^5.}{a^8.}\ \ldots\ \text{\&c.}$$

$$\div$$

The variations which a change of ſigns in the original equation will produce are too obvious to need deſcription.

Hence we get the following rules for the arithmetical ſolution of the three forms of quadratics, which may be reduced to two caſes.

CASE I. $x^2 \pm ax = b$.

RULE.—*Place the numerical values of a and b as diviſor and dividend, as in common diviſion.—Having found the firſt figure of the quotient, add it in its proper place (which is the ſame as that of ſuch figure of the dividend b as ſtands over the units place of the firſt product of the increaſed diviſor into ſuch quotient figure, when placed for ſubtraction) to the original diviſor a; multiply the ſum by ſuch added figure, and ſubtract the product from the dividend, as in common diviſion. Then, if the next figure of the quotient to be found will fall under any of the figures of the firſt increaſed diviſor, ſo as not to increaſe the number of its digits when added to it, bring down the next figure of the dividend, as in common diviſion; but, if ſuch next figure of the quotient will, when added in its proper place, increaſe the number of figures in the diviſor, bring down the two next figures, as in the extraction of the ſquare root.—Find ſuch next figure of the quotient, and add both the figures of the quotient to the firſt increaſed diviſor for a new diviſor (as in extracting the ſquare root); multiply ſuch laſt increaſed diviſor by the laſt found figure; ſubtract the product; bring down another figure or figures of the dividend, and proceed as before: always adding to the laſt diviſor the two laſt figures of the quotient, including the quotient-figure obtained by each diviſion, for a new diviſor, as in extracting the ſquare root; and ſo on till the operation be finiſhed.*

The quotient ſo obtained is a value of x, whoſe ſign is the ſame as that of the ſecond term of the original equation :—for it's other value add to ſuch quotient the original diviſor a, and prefix to the ſum the contrary ſign.

EXAMPLE I.

Given . . . $x^2 + 5613x = 2579080$. Quære x.

a = ...	5613		
	+ 4..		
1ſt Diviſor	= 6013 . . . .)	2579080	(+ 427. = x
	+ 42	24052''	5613.
2d D. . .	= 6433 . . . .)	17388.	— 6040. = x.
	+ 27	12866.	
3d D. . .	= 6460 . . . .)	45220	
		45220	
		.0.	

EXAMPLE 2.

Given $x^2 + 13.24687x = 123.45678932$. Quære x.

a	= 13.24687.		
	+ 6		
1ſt Diviſor	= 19.24687.)	123.45678932	(+ 6.3120464569 . . &c. = x
	+ 63	115.48122'''	13.24687
2d D. . .	= 2554687.	7975569	— 19.5589164569 . . &c. = x.
	+ 31	3115083	
		5293962	
3d D. . .	= 2585687	1201880.	
	+ 12	167045.	
		11820.	
4th D. . .	= 2586887	1472.	
	20	179.	
		24.	
5th D. . .	= 2587087	2.	
	......		

EXAMPLE 3.

Given $x^2 - 45x = 522.47695432$. Quære x.

a	= 45		
	+ 9		
1ſt Diviſor . .	= 54	) 522.47695432	(— 9 573773622 . . &c. = x
	+ 95	486 ' ' ' '	45
2d D.	= 635	3647	+ 54.573773622 . . &c. = x
	+ 57	47269	
		242054	
3d D.	= 6407.	4962532.	
	+ 73	472263.	
		23236.	
4th D.	= 64143	3992.	
	+ 37.	144.	
		16.	
5th D.	= 641467.	3.	
	.....		

CASE II. $x^2 - ax = - b$.

RULE.—*Proceed as before, except that inſtead of adding the two laſt figures of the quotient to each diviſor, as in the former Caſe, they muſt in this be ſubtracted from it, and the remainder will be the new diviſor for each repetition of the diviſion.*

The quotient is the leſſer value of the root, which, ſubtracted from the original diviſor a, will give the other value, both of them being affirmative. If b be greater than the ſquare of ½ a, the ſolution is however impoſſible.

EXAMPLE.

Given $x^2 - 19.23456x. = - 83.57214567.$ Quære x.

a	=19.23456.		
	— 6		
1ſt Diviſor . .	=13.23456.)	83.57214567. (	+ 6.630654987. &c. = *x*.
	— 66	79.40736.	19.23456.
2d D.	= 663456.	4164785	+12.603905012. &c. = *x*.
	— 63	1840496	
		3912870.	
3d D.	= 600456.	328494.	
	— 30	29796.	
		5900.	
4th D.	= 597456.	524.	
	— 06	46.	
		5.	
5th D.	= 597396		

It is unneceſſary to remark, that the contractions ordinarily made uſe of in the extraction of the ſquare root are equally applicable here; and they are accordingly applied in all the examples except the firſt.

I am inclined to believe that ſuch as uſe this proceſs will find it attended with ſome advantages in point of expedition, eſpecially where a conſiderable number of places is required in the root—and being mathematically correct, it may of courſe be extended to any length.

I am, Sir, your moſt obedient Servant,

9th *May* 1797. J. F—:—:—:—R.

SCIENTIFIC NEWS.

Account of Memoirs publiſhed by the Polytechnic School at Paris.

[Continued from page 95.]

It has long been known, that the ſulphure of potaſh attracts the oxygene in the atmoſphere, and ſeparates it from the azote with which it is combined. Attempts were conſequently made to conſtruct eudiometers with this ſulphure in imitation of Scheele; but none of theſe gave the reſult with the requiſite ſpeed and accuracy. Guyton diſcovered, that by heating the ſulphure the combination is made inſtantly. He therefore adviſes to take a ſmall retort filled with water, to place therein the ſmall piece of ſulphure, then to introduce the air and heat with a taper to that place on which the ſulphure reſts. The abſorption of the oxygene is immediately made, and the deſired reſult aſcertained.

By this invention, which contributes to the accuracy and value of an inſtrument greatly uſeful in meteorological obſervations, Guyton has rendered a real ſervice to philoſophers.

Gottling had maintained, in a memoir publiſhed in Gren's Journal, that phoſphorus burns in azotic gas, forms an acid by this combuſtion, and does not burn in oxygenous gas. Berthollet made ſeveral experiments on the combination of phoſphorus with different gaſes. He found, 1. That the phoſphorus does not combine with oxygene at the ordinary temperature, but requires a more elevated temperature to produce combuſtion. 2. That phoſphorus is ſoluble in the azotic gas; and that, by means of this ſolution, it burns with oxygenous gas at the ordinary temperature, and appears luminous in the dark.

By means of theſe experiments, Berthollet explained the extraordinary phenomena which Gottling, Lemps, and Lampadius, had obſerved; and he imagined that phoſphorus might be uſed to meaſure the quantity of oxygene gas contained in the air of the atmoſphere, or any other gas containing azote.

This new eudiometer has an advantage over other inſtruments of the ſame nature, by ſhewing the preciſe inſtant of the total abſorption of oxygene, which happens when no more white vapour is ſeen in the day light, nor light in an obſcure place; but it has likewiſe the inconvenience of requiring the preſence of a quantity of azote ſufficient to diſſolve the phoſphorus readily before its combination with oxygene.

In the analyſis of the calcedony of Creuſot, of which the conſtituent parts proved to be, ſilex 86.08, iron 7.68, alumine 4.11, lime 1.16, loſs 1.0, Guyton aimed leſs to aſcertain the proportions of ingredients in a ſtone ſo variable, than to preſent to the pupils of the ſchool an example of the analyſis of ſtones. In this reſpect his memoir is one of the moſt inſtructive in the collection.

A great number of chemiſts, with Pott at their head, have laboured at the ſolution of the intereſting problem of the fuſibility of earths, upon which they have made many ſcientific experiments. To theſe Guyton has added a great number of his own. He firſt tried the fuſibilities of the earths with oxygene gas, and afterwards the effect of carbonate of ſoda, calcined borax, pulverized ammoniacal phoſphate of ſoda, and the red oxide of lead. He afterwards mixed the earths two and two with the preceding fluxes, and at laſt terminated by the action of the earths upon each other, and that of calcareous phoſphate on each of them. The ſeries of facts muſt be conſulted in the memoir itſelf, as their connection does not allow of detached extracts.

Clouet had publiſhed in a memoir, that the pruſſic colouring principle might be obtained by paſſing ammoniacal gas through ignited charcoal. The experiments related by Bonjour, in the third Cahier of the Journal of the Polytechnic School, conſiſt merely of the proceſſes he made uſe of, and the trials he made in the preſence of Clouet to verify the phenomenon he had announced, and which, after a moſt careful examination, proved to be in every reſpect agreeable to the account which was publiſhed.

The memoir of Fourcroy and Vauquelin on the ſulphureous acid, conſiſts in an examination or hiſtory of its action on every known ſubſtance. They, in the firſt place, examined this action

upon { caloric
oxygene gas,

next on the acids { ſulphuric
nitric
muriatic

with the alkalis { potaſh
ſoda
ammoniac,

afterwards on the combuſtibles { hydrogen
phoſphorus
phoſphorated hydrogen gas
ſulphurated hydrogen gas
carbon

with the earths { alumine
lime
barytes
magneſia.

In every caſe wherein the ſulphureous acid formed a ſalt by combination, theſe chemiſts have deſcribed the form of its cryſtals, its fuſibility, the affinities and proportion of conſtituent parts.

This memoir preſents a complete table of the binary combinations of the ſulphureous acid, and renders it very deſirable that the authors ſhould continue, for the advancement of ſcience, to publiſh ſimilar accounts of a great number of ſubſtances, on which our notions are yet very incomplete.

The three memoirs of general phyſics: 1. Of the congelation of mercury, by Haſſenfratz, Bonjour, and Welte. 2. The laws of dilatation in elaſtic fluids, by Prony. 3. The influence of ſnow and rain on vegetation, by Haſſenfratz.

It has long been known, that mercury was rendered ſolid at Peterſburg by cold 24 degrees of Reaumur below zero, and that in this ſtate it becomes malleable. The experiment was repeated at London by Cavendiſh, by producing the ſame degree of artificial cold; but this reſult had never been obtained in France.

The memoir of Haſſenfratz on the congelation of mercury contains only a repetition of this experiment, in which the proportion of caloric, abſorbed during the congelation of the metal, is determined, which had not before been done by any philoſopher. This experiment has afforded a very curious reſult. In water the caloric, abſorbed during the tranſition from the ſolid to the fluid ſtate, is ſufficient to have raiſed its temperature 60 degrees Reaumur; and in mercury the quantity to produce the ſame effect would have been ſufficient to raiſe the temperature 64 degrees.

In order to aſcertain the principle and the laws to which a great number of phenomena are referable, it is neceſſary to make a ſeries of reiterated experiments on the ſame facts. This is attended with two kinds of difficulties, namely, the time they require, and the imperfection of the inſtrument.

Neverthleleſs, when the exiſtence of a law in the progreſs of any phenomenon is aſcertained, it is always poſſible, from a ſmall number of experiments, to diſcover by analyſis an equation which may expreſs the law. This is the object of the memoir which Prony has printed in the ſecond Cahier of the Journal of the Polytechnic School. It is divided into two parts. In the firſt he inveſtigates a method of interpolation applicable to the phenomena which depend on elaſtic fluids; in the ſecond he applies this method to aſcertain the law of dilatability in theſe fluids. For this application he avails himſelf of the experiments made by Prieur, and publiſhed by Guyton, on the dilatability of oxygene gas, azote, hydrogenous, nitrous, carbonic acid, and ammoniacal gaſes; and laſtly, on the expanſive force of the vapour of water and that of alcohol.

This memoir contains tables deduced from the equations afforded by the methods made uſe of by the author, and the reſults obtained by Prieur. They are likewiſe expreſſed by engraved curves.

It was formerly obſerved, that ſnow preſerves the vegetative power, and that rain accelerates the growth of plants more than artificial waterings. Haſſenfratz has enquired, in his memoir, into the cauſe of theſe comparative effects. He ſhews, from ſeveral experiments, that the preſerving power of ſnow ariſes from two circumſtances—its imperfect conducting power, and its oxygenation. This philoſopher has proved, that ſnow is water oxygenated and converted into the ſolid form. He has alſo ſhewn, that rain is more oxygenated than any other water, and that a large portion of its good effects is to be attributed to this circumſtance.

The memoir on the arts is written by Chauſſier. Its object is to deſcribe the compoſition of a liquor proper to be ſubſtituted inſtead of wine lees in fulling, to obtain the greateſt effect with the moſt facility and economy.

The liquor indicated by Chauſſier is a ſmall quantity of ſulphuric acid. This acid bath, which experience has aſſured him may be uſed with invariable ſucceſs, likewiſe affords the advantage of obtaining a very white colour in hats, becauſe the felt does not become coloured in the working.

Extract of a Letter from Profeſſor LORENZ DE CRELL.

MR. KLAPROTH[*] is continuing his chemical analyſis, and will ſhortly publiſh a ſecond volume of them. He has found that the newly diſcovered *Titanium* is by no means ſo rare as might be ſuppoſed from its not having been known till lately. He has detected it in a ſingular kind of mineral found at Aſchaffenberg by Prince Gallitzin, and in ſome other minerals, as well as in menakanite, in which it is mixed with iron.—Mr. Weſtrumb's method of obtaining ardent ſpirits from different ſorts of grain, proves to be excellent, and ſucceeds well on a large ſcale as a branch of trade.—Sulphate of barytes does not require more than an equal quantity of well aërated alkali for its decompoſition both in the humid and dry way. Muriate of barytes is obtained perfectly pure by boiling it in ten times its weight of highly rectified alkohol vini, which diſſolves nothing but this muriate.—To ſeparate hepatic gas and carbonic acid gas when united, employ acidulated acetite of ſilver or mercury. Either of theſe metallic ſalts will abſorb the hepatic gas, and leave the carbonic acid gas untouched.

---

[*] This article was forwarded to the learned editor of the Engliſh tranſlation of Crell's Journal; from whom I received it in a letter, containing very encouraging expreſſions of approval and good wiſhes for the ſucceſs of the preſent undertaking.

AIR PUMP by the REV.D JOHN PRINCE

Fig. 11.

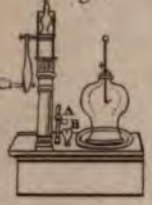


Fig. 10.

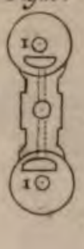


Fig. 8.

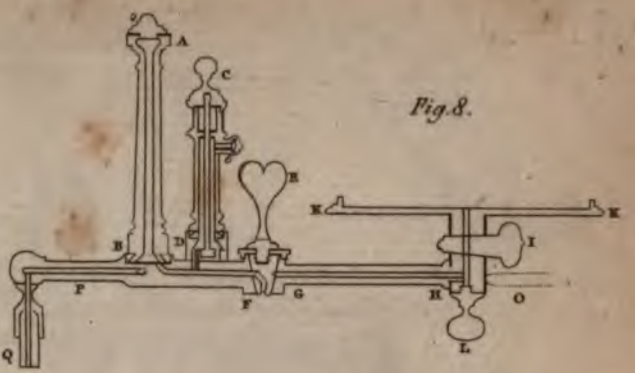


Fig. 6.

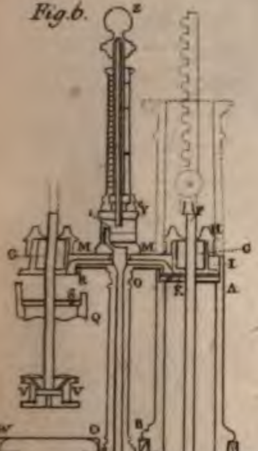
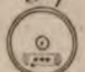


Fig. 7.



Gravimeter

Fig. 4.

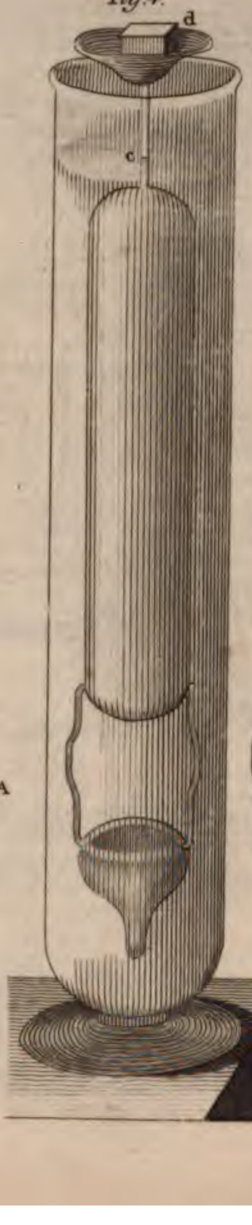


Fig. 2.

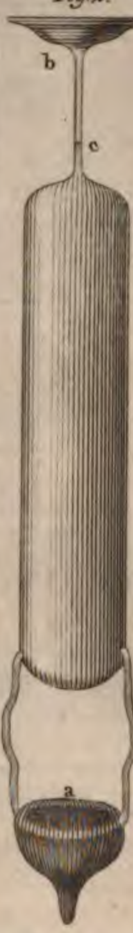


Fig. 5.

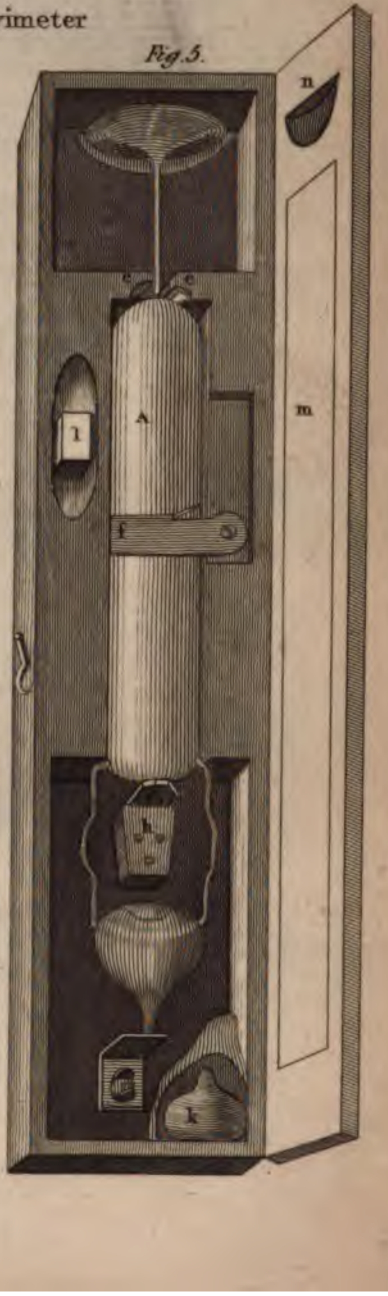


Fig. 9.

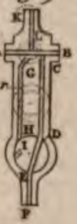
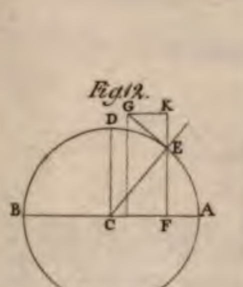


Fig. 13.



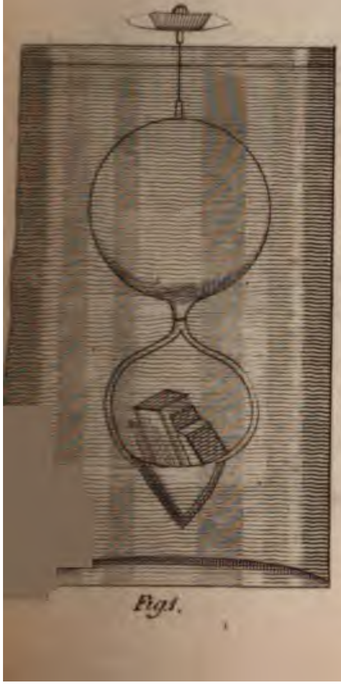
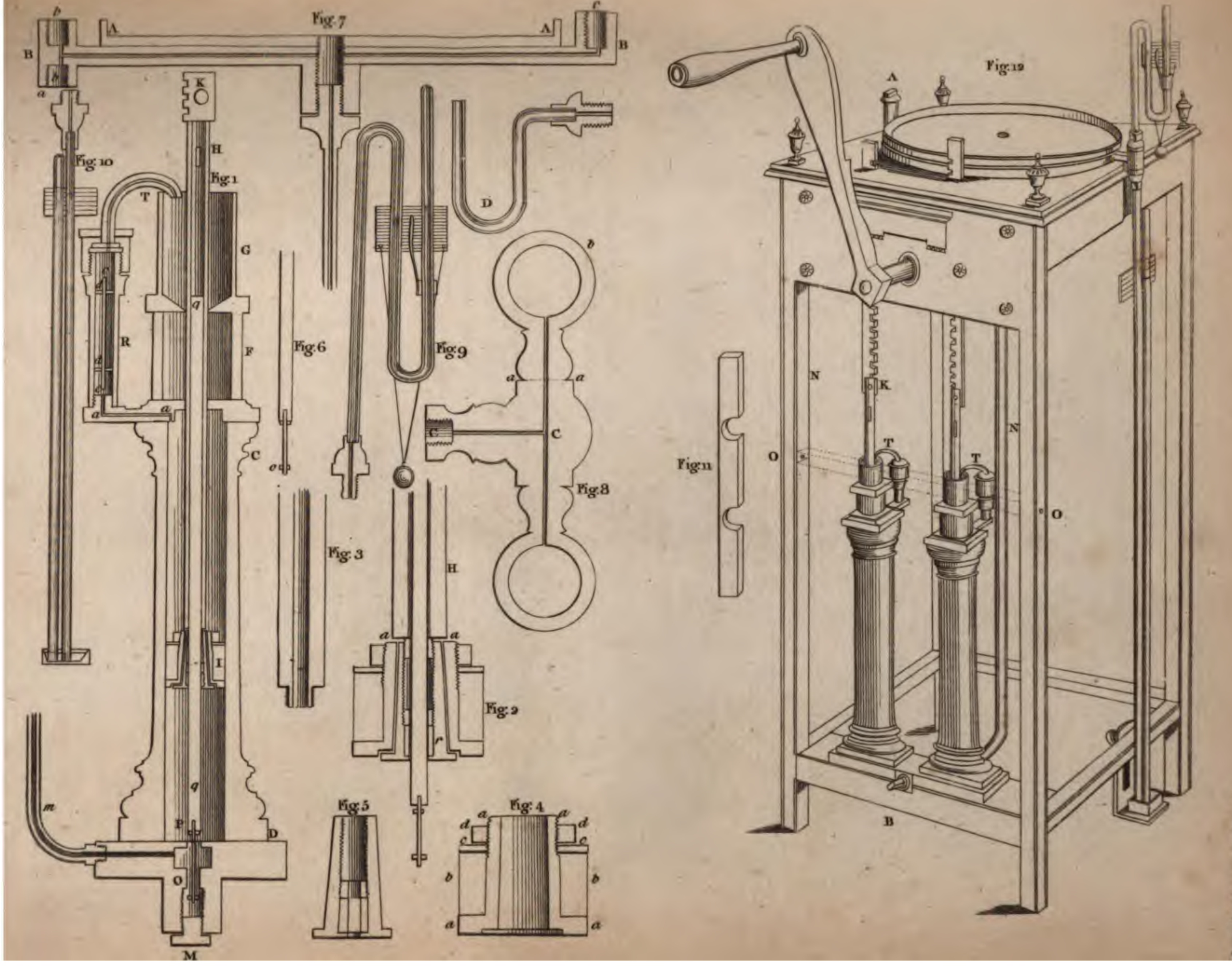


Fig. 1.

Fig. 3.

The AIR PUMP of CUTHBERTSON



Philos Journal Vol. 1. Pl. ; facing

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY 1797.

ARTICLE I.

Obſervations on Horizontal Refractions which affect the Appearance of Terreſtrial Objects, and the Dip or Depreſſion of the Horizon of the Sea. By JOSEPH HUDDART, *Eſq. F. R. S.**

THE variation and uncertainty of the dip in different ſtates of the air, taken at the ſame altitude above the level of the ſea, was the reaſon of my turning my thoughts to this ſubject, as it renders the latitude obſerved incorrect, by giving an erroneous zenith diſtance of a celeſtial object.

I have often obſerved, that lowlands, and the extremity of headlands, or points forming an acute angle with the horizon of the ſea, and viewed from a diſtance beyond it, appear elevated above it, with an open ſpace between the land and the ſea. The moſt remarkable inſtance of this appearance of the land, I obſerved at Macao, for ſeveral days previous to a typhon, in which the Locko loſt her topmaſts in Macao roads; the points of the iſlands and lowlands appearing the higheſt, and the ſpaces between them and the ſea the largeſt, I ever ſaw. I believe it ariſes and is proportional to the evaporation going on from the ſea. And in reflecting upon this phenomenon, I am convinced that thoſe appearances muſt ariſe from refraction, and that, inſtead of the denſity of the atmoſphere increaſing to the ſurface of the ſea, it muſt decreaſe from ſome ſpace above it; and that evaporation is the principal cauſe which prevents the uniformity of denſity and refraction being continued by the general law down to the ſurface of the earth. And I am inclined to believe, though I mention it here as a conjecture, that the difference of ſpecific gravity in the particles of the atmoſphere may be a principal agent in evaporation; for the corpuſcles of air, from their affinity with water, being combined at the ſurface of the fluid from expanſion, form air ſpecifically lighter than the drier atmoſphere; and therefore float or riſe, from that principle, as ſteam from water; and in their riſing (the ſurrounding corpuſcles from the ſame cauſe imbibing a part of the moiſture) become continually drier as they aſcend, yet continue aſcending until they become equally denſe with the air*. However, theſe conjectures I ſhall leave, and proceed to the following obſervations upon refractions:

---

* Philoſophical Tranſactions for 1797.

In the year 1793, when at Allonby in Cumberland, I made ſome remarks on the appearance of the Abbey-head in Galloway, which in diſtance from Allonby is about ſeven leagues; and from my window, at fifty feet above the level of the ſea, at that time of tide, I obſerved the appearance of the land about the Head as repreſented in Pl. VIII. Fig. 1. There was a dry ſand xy, called Robin Rigg, between me and the Head, at the diſtance from my houſe of between three and four miles; over which I ſaw the horizon of the ſea H O; the ſand at this time was about three or four feet above the level of the ſea. The hummock d is a part of the headland; but appeared inſulated or detached from the reſt, and conſiderably elevated above the ſea, with an open ſpace between. I then came down about twenty-five feet, when I had the dry ſand of Robin Rigg xy in the apparent horizon, and loſt all that floating appearance ſeen from above; and the Abbey-head appeared everywhere diſtinct from the ſurface of the ſand. This being in the afternoon, the wet or moiſture on the ſand would in a great meaſure be dried up. I have reaſon therefore to conclude, that evaporation is the cauſe of a leſs refraction near the ſurface of the ſea; and when ſo much ſo as to make an object appear elevated wholly above the horizon (as at d in Fig. 1.) there will from every point of this object iſſue two pencils of rays of light which enter the eye of the obſerver, and that below the dotted line AB (parallel to the horizon of the ſea HO) the objects on the land will appear inverted.

To explain this phenomenon, I ſhall propoſe the following theory, and compare it with the obſervations which I have made. Suppoſe HO, Fig. 2. to repreſent the horizontal ſurface of the ſea, and the parallel lines above it the lamina or ſtrata of corpuſcles, which, next the fluid, are moſt expanded, or the rareſt; and every lamina upwards increaſing in denſity, till it arrive at a maximum (and which I ſhall in future call the maximum of denſity) at the line D C, above which it again decreaſes in denſity *ad infinitum*.

Though this in reality may be the caſe, I do not wiſh to extend the meaning of the word denſity farther than to be taken for the refractive power of the atmoſphere; that is, a ray of light entering obliquely a denſer lamina, to be refracted towards a perpendicular to its ſurface; and in entering a rarer lamina the contrary; which laminæ being taken at infinitely ſmall diſtances, the ray of light will form a curve agreeable to the laws of dioptrics.

In order to eſtabliſh this principle in horizontal refractions, I traced over various parts of this ſhore, at different times, when thoſe appearances ſeemed favourable, with a good teleſcope, and found objects ſufficient to confirm it; though it be difficult at that diſtance of the land to get terreſtrial objects well defined ſo near the horizon, as will afterwards appear.

---

* Mr. Hamilton, in his very curious Eſſay on the Aſcent of Vapours, does not allow of this principle even as an aſſiſtant; though by a remark (page 15) he takes notice of thoſe appearances in the horizon of the ſea, and ſays they ariſe from a ſtrong or unuſual degree of refraction; the contrary of which I hope to illuſtrate in the courſe of this paper.

One day, obſerving the land elevated, and ſeeing a ſmall veſſel at about eight miles diſtance, I from my window directed my teleſcope to her, and thought her a fitter object than any other I had ſeen for the purpoſe of explaining the phenomena of theſe refractions. The teleſcope was forty feet above the level of the ſea: the boat's maſt about thirty-five feet; ſhe being about twenty to thirty tons burthen. The barometer at 29.7 inches, and Fahrenheit's thermometer at 54°.

The appearance of the veſſel, as magnified in the teleſcope, was as repreſented in Fig. 3, and from the maſt-head to the boom was well defined. I pretty diſtinctly ſaw the head and ſhoulders of the man at the helm; but the hull of the veſſel was contracted, confuſed, and ill defined. The inverted image began to be well defined at the boom (for I could not clearly perceive the man at the helm inverted), and from the boom to the horizon of the ſea the ſails were well defined; and I could ſee a ſmall opening above the horizon of the ſea in the angle made by the gaff and maſt; and had the maſt been ſhorter by ten feet (to the height of y), the whole would have been elevated above the horizon of the ſea, and from y to d an open ſpace. This drawing was taken from a ſketch I took at the time, and repreſents the proportion of the inverted to the erect object, as near as I could take it by the eye; the former being about two thirds of the latter in height, and the ſame breadth reſpectively; though at one time, during my obſervation, which I continued for about an hour, I thought the inverted nearly as tall as the erect object. The day was fine and clear, with a very light air of wind, and I found very little tremor or oſcillation in viewing her through the teleſcope.

I have laid down Fig. 4. for the explanation of the above phenomena, in which A repreſents the window I viewed B the veſſel from; HO the curved ſurface of the ſea; CD parallel to HO the height of the maximum of denſity of the atmoſphere; the lines marked with the ſmall letters $a\,a$, $b\,b$, $c\,c$, $d\,d$, the pencils of rays under their various refractions from the veſſel to the eye or object-glaſs of the teleſcope.

The pencil of rays $a\,a$, from a point near the head of the mainſail, is wholly refracted in a curve convex upwards, being everywhere above the maximum of denſity; and the pencil of rays $d\,d$, which iſſues from the ſame point in the ſail, and paſſes near the horizon of the ſea at x, is convex upwards from the ſail to W, where it paſſes the line of maximum of denſity, which is the point of inflection; there it becomes convex downwards, paſſing near the horizon at x to y, where it is again inflected, and becomes convex upwards from thence to the eye. The pencil of rays $b\,b$, from the end of the boom, paſſing nearly parallel to the horizon, and near the maximum of denſity, ſuffers very little deviation from a right line in the firſt part; but in aſcending (from the curvature of the ſea) will be convex upwards to the eye. The pencil of rays $c\,c$, from the ſame point in the boom, may have the ſmall part to c convex upwards: from c to z it will be convex downwards, and from z to the eye convex upwards.

From this inveſtigation it appears that two pencils of rays cannot paſs from the ſame point and enter the eye from the law of refraction, except one pencil paſs through a medium which the other has not entered; and therefore the maximum of denſity was below the boom, and could not exceed ten feet of height above the ſurface of the ſea at the time theſe obſervations were made.

Reſpecting the hull of the veſſel being confuſed and ill defined in the teleſcope, as by Fig. 3, it ariſes from the blending of the rays from the different parts of the object refracted through the two mediums; ſome parts of the hull appearing erect, and ſome inverted. Suppoſe the dotted line *i i*, Fig. 4. an indefinite pencil of rays paſſing from between the inverted and erect parts of the object, or the upper part of the hull of the veſſel to the eye (for the lower part of the hull could not be obſerved), the objects cannot appear inverted, except the angles at the eye *a* A *c* and *a* A *d* exceed the angle *a* A *i*; for the intermediate ſpace could only be contracted by the ſecondary pencil of rays. The lengths of the inverted, compared with the erect image of the ſail, is as the ſines of the angles at the eye *a* A *i* to *i* A *d*; and the angle at the eye *a* A *d*, made by the two pencils of rays from the ſame point near the head of the ſail, muſt be double the angle *a* A *i*, when the inverted image is as tall as the erect. In this caſe the ſines of the angles *a* A *b*, *a* A *c*, *a* A *d*, Fig. 4, are proportional to the altitudes *a b*, *a c*, *a d*, in the magnified view of the veſſel Fig. 3.

Under this conſideration, no inverted image of the ſail will be formed, until the angle at the eye, made by the two refracted pencils of rays *a a* and *d d*, exceed the angle made by *a a* and *b b*, the apparent height of the ſail of the veſſel; for, were thoſe angles equal, the inverted ſail would only be contracted into the parallel of altitude of the boom *b*, and render the appearance confuſed, as in the hull of the veſſel.

Reſpecting the exiſtence of two pencils of rays entering the eye from every point of an object not more elevated than *a*, or leſs than *i*, Fig. 3, in this ſtate of the atmoſphere, I cannot bring a ſtronger proof than that of the ſtrength of a light when the rays paſs near the horizon of the ſea, proved by the following obſervations:

Going down Channel about five years ago in the Trinity yacht, with ſeveral of the elder brethren, to inſpect the light-houſes, &c. I was told by ſome of the gentlemen who had been on a former ſurvey, that the lower light of Portland was not ſo ſtrong as the upper light at near diſtances; but that at greater diſtances it was much ſtronger. I ſuſpected that this difference aroſe from the lower light being at or near the horizon of the ſea, and mentioned it at the time; but afterwards had a good opportunity of making the obſervation. We paſſed the Bill of Portland in the evening, ſteering towards the Start; a freſh breeze from the northward, and clear night. When we had run about five leagues from the lights, during which time the upper light was univerſally allowed to be the ſtronger, (ſeveral gentlemen keeping watch to make obſervations thereon), the lower light drawing near the horizon ſuddenly ſhone with double luſtre. Mr. Strachan, whoſe ſight is weak, had for ſome time before loſt ſight of both lights; but could then clearly perceive the lower light. I then went aloft (as well as others), but before I got half maſt up, the lower light was weaker than the upper one. On coming down upon deck, I found it again as ſtrong as before. We proceeded on, and ſoon loſt the lower light from the deck; and upon drawing the upper light near the horizon, it, like the former, ſhone exceeding bright. I again went aloft, when it diminiſhed in brightneſs; but from the maſt-head I could then ſee the lower light near the horizon as ſtrong as before. This is in conſequence of the double quantity of light entering the eye by the two pencils of rays from every point. To illuſtrate which, we compare the veſſel Fig. 4. to a light-houſe built upon the ſhore, and A the place of the obſerver; and having brought down the light ſo low as to view it in the direction *a a*, another light would appear in the horizon at *x*, from the pencil *d d*; and had the veſſel been ſtill enough to have obſerved it at this time with a good glaſs, I doubt not but the two images might have been diſtinctly ſeen. As the light dropped (by increaſing the diſtance) the two images would appear continually to approach each other, till blended with double light in one, and diſappear at the altitude *i*, above the apparent horizon of the ſea. But, as explained before, if the ſtrength of evaporation did not ſeparate by refraction the pencils *a a* and *d d* to a greater angle than double the angle that the lamps and reflectors appear under, the two images would be blended, and the ſtrong appearance of light would be of ſhorter duration. The diſtance run from the lights during the time each of the lights ſhone bright, would have been uſeful; but this did not occur at the time, nor have I had the like opportunity ſince. However, I recommend to the mariner to ſtation people at different heights in looking out for a light, in order to get ſight of it near the horizon, when it is always ſtrongeſt.

Reſpecting the appearance of the Abbey-head, before mentioned, Fig. 1, the dotted line AB repreſents the limit, or the loweſt points of the land that can be ſeen over the ſea; for, as above ſtated, all the objects appearing below this line are the land above it inverted; and where the land is low, as at *d* and *m*, it muſt appear elevated above the horizon of the ſea.

In Figure 5, let HO repreſent the curve of the ocean, and *d* the extreme top of the mount viſible at A by the help of refraction; the dotted pencil of rays *c c* paſſing from *d* to the eye, in ſome part a little below the maximum of denſity, where inverſion begins; therefore no land lower than this can be ſeen; for any pencil from a point in the land lower than this muſt, in the refraction, have a contrary flexure in the curve, and therefore paſs above the obſerver. Let AD be a tangent to the curve at A, then the object *d* will appear to be elevated by refraction to D; alſo let A *v* be a tangent to the pencil A *x* at A, then the angle DA *x* will appear to be an open ſpace, or between D and the horizon of the ſea. Suppoſe a ſtar ſhould appear very near and over the mount *d*, as at *, two pencils would iſſue from every point of it, and form a ſtar below as well as above the hummock *d*. There are always confuſed or ill defined images of the objects at the height of the dotted line, Fig. 1, above the level of the ſea, as before mentioned; and inſtead of the points of *d* ending ſharp in that line, they appear blunted, and the Abbey-head is frequently inſulated at the neck *m*.

I have viewed from an elevated ſituation a point or headland at a diſtance beyond the horizon of the ſea, forming, as in Fig. 6, a ſtraight line AB, making an acute angle BAO with the horizon of the ſea. Seeing the extreme point blunted and elevated, I deſcended; and though in deſcending the horizon cut the land higher, as at HO, HO, yet the point had always the ſame appearance as *a a a*, Fig. 6, though the land is known to continue in the direction of the ſtraight line AB to beneath the horizon, or nearly ſo, as viewed from the height above.

If then, from a low ſituation, we view this headland through a teleſcope, the inclination of the ſurface A B to the horizon being known to be a ſtraight line, it will appear as in Fig. 7; the dotted line (at the height of the point where a perpendicular *x y* would touch the extreme of the land) being at the limit or loweſt point of erect viſion. And if a tangent to the curved appearance of the land *a b* is drawn parallel to the inclined ſurface of the land A B, Fig. 6, touching it at C, the point C will ſhew the height of the maximum of denſity, where the pencil of the rays of light from thence to the eye approaches neareſt the ſea; for pencils of rays from this land, taken at ſmall diſtances from C, will form parallel curves nearly through the refracting mediums, and C will be the point of greateſt refraction;

tion; for above C, as to B, the refraction ſomewhat decreaſing, will appear below the line *a b*, or the parallel to the ſurface of the land, and the refractions decreaſe below the point C; for, had they increaſed uniformly down to the ſurface of the ſea, it would render the apparent angle of the point of land *z* more acute than the angle C *a* O, contrary to all obſervations.

Thus I have endeavoured to explain the phenomena of the diſtorted appearance of the land near the horizon of the ſea when the evaporation is great, and when at the leaſt I never found the land quite free from it when I uſed a teleſcope, and from thence infer that we cannot have any expectation to find a true correction for the effect of terreſtrial refraction by taking any certain part of the contained arc; for the points *z* CB, Fig. 7, will have various refractions, though they are at nearly the ſame diſtance from the obſerver. And if the obſervations are made wholly over land, if the ground riſes to within a ſmall diſtance of the rays of light, in their paſſage from the object to the eye, as well as at the ſituation of the object and obſerver, the refractions will be ſubject to be influenced by the evaporation of rains, dews, &c.; which is ſufficiently proved by the obſervations of Colonel Williams, Captain Mudge, and Mr. Dalby, Phil. Tranſ. 1795, p. 583.

The appearances mentioned by Colonel Williams, Captain Mudge, and Mr. Dalby (Phil. Tranſ. 1795, p. 586, 587) cannot be demonſtrated upon general principles, as they ariſe from evaporation producing partial refractions. In thoſe general principles it is ſuppoſed that the ſame lamina of denſity is everywhere at an equal diſtance from the ſurface of the ſea, at leaſt as far as the eye can reach a terreſtrial object; but in the partial refractions, the lamina of the expanded or rarefied medium may be of various figures, according to circumſtances, which will refract according to the incidence of the rays, and affect the appearance of the land accordingly, which I have often ſeen to a ſurpriſing degree. But my principal view is to ſhew the uncertainty of the dip of the ſea, and that the effect of evaporation tends to depreſs the apparent horizon at *x* when the eye is not above the maximum of denſity; and from hence the difficulty of laying down any correct formula for theſe refractions whilſt the law of evaporation is ſo little underſtood; which indeed ſeems a taſk not eaſy to ſurmount. The effect indicated by the barometer and thermometer is inſufficient; and ſhould the hydrometer be improved to fix a ſtandard for moiſture in the atmoſphere, and ſhew the variations near the ſurface of the ocean, which certainly muſt be taken into the account (evaporation going on quicker in a dry than a moiſt atmoſphere), the theory might ſtill be incomplete for correcting the tables of the dip. I ſhall therefore conclude this paper, by ſhewing a method I uſed in practice, in order to obviate this error in low latitudes.

When I was deſirous to attain more accurately the latitude of any headland, &c. in ſight, I frequently obſerved the angular diſtances of the ſun's neareſt limb from the horizons upon the meridian both north and ſouth, beginning a few minutes before noon, and taking alternately the obſervations each way, from the poop, or ſome convenient part of the ſhip, where the ſun and the horizon both north and ſouth were not intercepted; and having found the greateſt and leaſt diſtances from the reſpective horizons, which was at the ſun's paſſing the meridian, and corrected both for refraction, by ſubtracting from the leaſt, and adding to the greateſt altitude, the quantity given by the table; and alſo having corrected for the error of the inſtrument, and the ſun's ſemi-diameter, the ſum of theſe two angular diſtances reduced as above—180° is equal to double the dip, as by the following

Example.

The ſun's declination 4° 32′ 30″ and its ſemi-diameter 15′ 58″ took the following obſervation:

	South.	North.
The meridian diſtance of the ſun's neareſt limb from the horizon of the ſea	78° 36′ 30″	= 101° 1′ 20″
Refraction per table	— 0 11	= + 0 11
Diſtances corr. for refraction	=78 36 19	= 101 1 31
Error of the ſextant	+ 1 32	+ 1 32
Sun's ſemi-diameter	+ 15 58	+ 15 58
	78 53 49	101 19 1
Half difference or the dip found	— 6 25	78 53 49
Altitude reduced	=78 47 24	180 12 50
Zenith diſtance	=11 12 36	180
		Diff. 12 50
The ſun's declination N.	= 4 32 30	Half = 6 25
		Dip.
Latitude of the ſhip N.	=15 45 06	

I regret that I cannot in this paper inſert the dip which I have found in my obſervations; for I only retained the latitude of the ſhip determined thereby as is uſual at ſea. I generally rejected the error of the inſtrument, the dip, and ſemi-diameter, as they affect both obſervations with the ſame ſigns; and reduced the obſervation by the following method:

	South.	North.	
Sun's diſtance as before	78° 36′ 30″	101° 1′ 20″	
Refraction	— 0 11	+ 0 11	
Dif. corr. for refraction	78 36 19	101 1 31	101° 1′ 31″
		+ 78 36 19	
		Sum 179 37 50	
Sum of ſemi-diameter, dip, and refraction = half difference	+ 11 5	180	+ 11 5
	78 47 24	Diff. 22 10	
		Half 11 5	101 12 36
	90		90
The zenith diſtance as before	=11 12 36	Zenith diſt.	=11 12 36

It may be obſerved, that neither the dip, ſemi-diameter, or index error can affect the zenith diſtance of the ſun's centre; and the refraction being ſmall near the zenith, the reſult muſt be true, if the angles are accurately taken; and it is only neceſſary to obſerve, that when the ſum of the diſtances is leſs than 180, the half difference muſt be added to the diſtances, as by the laſt reduction. There is a difficulty in making this obſervation when the ſun paſſes the meridian very near the zenith, as the change in the azimuth from eaſt to weſt is too quick to allow ſufficient time: nor can it be obtained by the ſextant when the ſun paſſes the meridian more than 30 degrees from the zenith; for I never could adjuſt the back obſervation of the Hadley's quadrant with ſufficient accuracy to be depended upon [*].

II.

Remarkable Effect of Terreſtrial Refraction on a Diſtant Headland.——Extract of a Letter from ANDREW ELLICOT *to* DAVID RITTENHOUSE, *Eſq. dated at Pittſburg, Nov. 5, 1787, concerning Obſervations made at Lake Erie* [†].

ON the 13th of laſt month, while we lay on the banks of Lake Erie, we had an opportunity of viewing that ſingular phenomenon by ſeamen termed looming. It was preceded by a fine aurora borealis on the evening of the 12th. The 13th was cloudy, but without rain. About 10 o'clock in the morning, as I was walking on the beach, I diſcovered ſomething that had the appearance of land in the direction of Preſqu'iſle; about noon it became more conſpicuous, and when viewed by a good achromatic teleſcope, the branches of the trees could be plainly diſcovered. From three o'clock in the afternoon till dark, the whole peninſula was conſiderably elevated above the horizon, and viewed by all our company with admiration. There was a ſingular appearance attending this phenomenon, which I do not remember to have ſeen taken notice of by any writer. The peninſula was frequently ſeen double, or rather two ſimilar peninſulas, one above the other, with an appearance of water between. The ſeparation and coincidence was very frequent, and not unlike that obſerved in ſhifting the index of an adjuſted Godfrey's quadrant. As ſingular as this may appear, it is not more ſo than the double refraction produced by the Iceland cryſtal. The next morning Preſqu'iſle was again inviſible, and remained ſo during our ſtay at that poſition. Preſqu'iſle was about 25 miles diſtant; its ſituation very low.

The ſame evening the wind began to blow briſkly from about two points weſt of north, and continued to increaſe till the evening of the 14th, when it was more violent than any thing of the kind I had ever been witneſs to before, and continued till the evening of the 16th, without the leaſt intermiſſion. Our tents were all blown down, and we were under the neceſſity of fortifying our camp by driving poſts near to each other firmly into the ground on the windward ſide, and filling up the vacuities with buſhes in the form of an hedge. During the continuance of this wind we frequently obſerved ſmall black clouds hanging over the lake: they had but little velocity, and were ſometimes exhauſted, and diſappeared without reaching the ſhore.

---

[*] The optical phenomena which relate to the conſtitution of the atmoſphere have not yet been much inveſtigated. From the barometrical admeaſurement of heights, as well as from experiments with the macrometer, or marine barometer of Halley (Roy, in Phil. Tranſ. Vol. LXVII.), it is aſcertained that air abounding with humidity is more expanſible by heat, and leſs denſe at like temperatures than air which is more dry. And from Sir Iſaac Newton's table, the refractive power of air, in reſpect to its denſity, compared with that of water in the ſame reſpect, is as 4160 to 7845 (Optics, Part III. Prop. 10.). Theſe and other facts ſhew that the rays of light muſt be variouſly affected, according to the nature and circumſtances of evaporation, condenſation, and other proceſſes carried on in the air, and particularly near the ſurface of water. That a lighter and leſs refractive ſtratum of air may thus be generated near the ſurface of the ſea to a conſiderable elevation, and remain for a time in equilibrio without aſcending, is highly probable; and in this caſe, whenever the quieſcent ſtate is from local or temporary circumſtances deſtroyed, it ſeems a natural conſequence, that ſudden and irregular currents of the lower air, and deſcending blaſts of the upper, may occaſion all the ſtrange effects of the typhon and other phenomena, not well deſcribed in that intereſting tropical combination of ſea and land to the eaſtward of Sumatra and Malacca. The Mediterranean Sea affords ſimilar appearances, particularly the aſtoniſhing ſpectra in the Strait of Reggio, deſcribed by Kircher, Minazi, and others, called Fata Morgana, of which I purpoſe ſhortly to give an account, with an engraving. N.

[†] American Philoſ. Soc. III. 62.

From the large bodies of timber blown down about the lakes, it appears that hurricanes are not uncommon. Coxe obſerves in his Travels through Ruſſia, that the lakes in that country are ſubject to terrible ſtorms.

III.

Extract of a Memoir of M. BENEDICT PREVOST, of Geneva, on the Emanations of Odorant Bodies. By Citizen FOURCROY[*].

CITIZEN Duc la Chapelle, Director of the Society of the Sciences and Arts at Montauban, in the department of Lot, has forwarded to the Firſt Claſs of the Inſtitute, in the name of the Society, a Memoir of M. Benedict Prevoſt, of Geneva, *concerning various means of rendering the emanations of odorant bodies perceptible to the ſight.* The Firſt Claſs of the Inſtitute heard the reading with great intereſt at its ſitting of the 16th Pluvioſe, in the year 5.

The following are the facts conſigned in this Memoir:

1. A concrete odorant ſubſtance, laid upon a wet glaſs, or broad ſaucer, covered with a thin ſtratum of water, immediately cauſes the water to recede, ſo as to form a ſpace of ſeveral inches around it.

2. Fragments of concrete odorant matter, or ſmall morſels of paper or cork, impregnated with an odorant liquor, and wiped, being placed on the ſurface of water, are immediately moved by a very ſwift rotation. Romieu had made this obſervation on camphor, and erroneouſly attributed the effect to electricity. The motion was perceptible even in pieces of camphor of ſeven or eight gros.

3. An odorant liquor being poured on the water ſtops the motion till it is diſſipated by evaporation. Fixed oil arreſts the motion for a much longer time, and until the pellicle it forms on the water is taken off.

4. When the ſurface of the water is cleaned by a leaf of metal, of paper, or of glaſs, plunged in and withdrawn ſucceſſively until the pellicle is removed, the gyratory motion is renewed. If a piece of red wax or of taper be dipped in water, and the drops ſhaken off into a glaſs of water containing odorant bodies in motion, the movement will be ſtopped. The ſame effect is not produced by metal.

5. An atmoſphere of elaſtic fluid is formed round odorant ſubſtances, and is the cauſe of the effects here deſcribed.

6. A morſel of camphor plunged to the depth of three or four lines in water, without floating, excites a movement of trepidation in the ſurrounding water, which repels ſmall

---

[*] Annales de Chimie, XXI. 254—Tranſlation. N.

bodies in its vicinity, and carries them again to the camphor by ſtarts The author concludes that an elaſtic fluid eſcapes from the odorant body in the manner of the fire of a fuſee, or the diſcharge of fire-arms.

7. When there is a certain proportion between the height of the water and that of the ſmall fragment of camphor, the water is briſkly driven off, returns again to the camphor, and again retires, as if by an exploſion, the recoil of which often cauſes the camphor to make part of a revolution on its axis.

8. Fragments of camphor of the ſize of a pea, floated upon water in a diſh of metallic leaf four or five lines in diameter, communicated motion to theſe diſhes, though leſs ſwift than was exhibited by the camphor alone. If the glaſs in which this experiment is made be nearly filled with water, and covered by a (plain) glaſs which intercepts the contact of the air, the motion decays and ceaſes.

9. Camphor alone moves more rapidly than when it is placed upon the metallic plate. The author infers that the immediate contact of the water favours the diſengagement of the fluid which produces the motion.

10. Camphor evaporates thirty or forty times more ſpeedily when placed upon water, than when entirely ſurrounded with air.

11. Camphor, during the act of diſſipation in the air, preſerves its form and its opake whiteneſs; upon water it is rounded, and becomes tranſparent, as if it had undergone a kind of fuſion. It may be inferred that it ariſes from the acquired motion, which cauſes it to preſent a greater ſurface to the air.

12. In fact, out of 12 equal ſmall pieces of camphor, ſix being ſuſpended under a glaſs with quick-lime, and very dry, and ſix others being ſuſpended in a moiſtened glaſs, together with a wet ſponge, the volatilization was alike in each; and the water in this caſe not touching the camphor did not appear to contribute to the effect.

13. It is neceſſary for this purpoſe that the water ſhould directly touch the camphor. Accordingly, fragments of camphor placed on blotting-paper continually wetted, are diſſipated with the ſame ſpeed, and become equally tranſparent, as when actually placed on water, though they exhibit no motion.

14. When ſmall pieces of camphor are plunged in water, the camphor becomes rounded and tranſparent, does not acquire any motion, and its diſſipation is leſs perceptible than in the air. The concurrence of air and water is therefore neceſſary to diſengage the fluid which is the cauſe of the motion and total diſſipation of odorant bodies.

15. The motion of odorant bodies upon water decays and ceaſes ſpontaneouſly at the end of a certain time; becauſe, the water having then contracted a ſtrong ſmell, the volatilization takes place in all the points of its ſurface; and the ſmall maſs being thus ſurrounded by the odorant fluid, which is no longer air, diſſolves, as in the ordinary odorant fluids, without forming the gaſeous jet which is the cauſe of the motion. The author compares the volatilization of the aromatic ſubſtance to a combuſtion excited by water.

In this place the author dwells for a ſhort time on the phenomena he had deſcribed. He obſerves that theſe effects may be rendered palpable by touching the ſurface of the water on which the odorant ſubſtances move, with a pin dipped in oil. At the very inſtant, quick as lightning, theſe particles, as if ſtruck, ceaſe to move. A coloured pellicle, formed by the oil, is ſeen on the ſurface of the water. The water penetrates the pores of the oil, like thoſe of the camphor, and diſengages a fluid which prevents the water from penetrating the odorant ſubſtance. The motion ceaſes, becauſe this ſubſtance is then plunged in a fluid formed by the oil, which fluid is not air.

16. All bodies which are not odorant preſent, when hot, the ſame phenomena as odorant bodies. In fact, the heat gives them a degree of ſmell. To this phenomenon our author refers the bubbles and the motion, which are ſeen when an ignited piece of money is thrown into water. The elaſtic fluid is according to him the caloric, which is diſengaged in proportion as the water penetrates the pores of the metal. It is nevertheleſs much more ſimple to attribute this phenomenon to the water converted into vapour round the ignited metal, and ſometimes to hydrogene gas, when the metal is capable of decompoſing water, eſpecially at this temperature.

After the explanation of theſe experiments, Mr. Prevoſt hopes with reaſon that they will contribute to the theory of odours, which ſo nearly reſembles that of the gaſes. He does not flatter himſelf with having exhauſted this ſubject, but conſiders his diſcoveries as the means of rendering odour perceptible by water not only to the ſight but even to the touch, as are likewiſe the vibrations of ſonorous bodies. Men deprived of the ſenſe of ſmell, and even the blind, according to him, may in this manner diſtinguiſh odorant bodies from thoſe which have no ſmell: "Perhaps," ſays he, "this kind of odoroſcope may, by improvement, become an odorimeter. The exceptions, ſuch for example as that of the cerumen of the ears, which produces much effect on water without being perceptibly odorant, and that of the fingers when hot or moiſt, are merely apparent; for, if our ſenſes do not in thoſe caſes diſcover odour, thoſe of animals more powerfully energetic, ſuch as the dog, perceive and diſtinguiſh individuals by its peculiar character. The odoroſcope may afford the information which is wanting reſpecting theſe effluvia. Thus it is that the fat of game, the ſmell of which is nearly to us imperceptible, is very much ſo to dogs, and exhibits ſenſible marks by the odoroſcope."

Mr. Prevoſt, who profeſſes much brevity in this memoir, affirms, that water placed on the odorant liquor, inſtead of this laſt being put upon the water, affords a phenomenon conſiderably intereſting, but which he has not deſcribed. A ſpeedy communication of the reſt of this intereſting work is much to be deſired.

We ſhall not in this place explain the manner in which it appears neceſſary to conſider the principle of ſmell, which no longer appears to us proper to be regarded as a peculiar matter, and one of the immediate materials of vegetables, always identical, and enjoying the ſame properties. We ſhall have occaſion in future to enter more fully into this ſubject, and perhaps to rectify the notions which have hitherto been formed concerning the ſpiritus rector or aroma of plants. We ſhall content ourſelves with obſerving, that the phenomena deſcribed with clearneſs and preciſion by Mr. Prevoſt, as well as the theory he has given, are referable to the attraction of the odorant matter in the maſs for air and water, and the ſolution which takes place in one or the other, or in both at the ſame time.

IV.

A Method of meaſuring the Force of an Electrical Battery during the Time of its being charged.

By Lieutenant Colonel HALDANE *[*].

LET the battery be inſulated; and at a ſmall diſtance from it place an uninſulated electrical jar; alſo near to the jar place one of Mr. Cuthbertſon's electrometers.

The electrometer being adjuſted according to the degree of force which is intended to be employed as a meaſure of force to be communicated to the battery, connect the electrometer with the jar; make a metallic communication between the interior ſide of the jar and the exterior ſide of the battery, and connect the interior ſide of the battery with the conductor of an electrical machine.

Then, by the operation of the electrical machine, the battery receives a quantity of the electrical fluid, and becomes charged. The fluid which departs from the exterior ſide of the battery, is received by the electrical jar, which alſo becomes charged; but this jar, being connected with the electrometer, explodes as ſoon as it acquires a force ſufficient to put the electrometer into motion.

Now, the quantity of the electrical fluid which is received by this jar, between each of the exploſions, is a meaſure of the quantity of the fluid in the battery; and the number of exploſions or diſcharges of this jar ſhews the number of meaſures which the battery contains, and conſequently the force which it is capable of exerting when diſcharged.

Demonſtration.

THE electrometer remaining under the ſame adjuſtment will require the ſame force to put it in motion: this force reſults from the quantity of electrical fluid received by the jar; and ſince it is admitted, that, when effects are the ſame, the cauſes of them muſt be equal, it is evident that the quantity of electrical fluid contained in the jar at the time of each exploſion is the ſame.

It is alſo obvious, that the ſum of all theſe equal quantities of the electrical fluid which was contained in the jar at the time of each exploſion, is equal to the whole quantity contained in the battery; for, the battery being inſulated, the jar received all the electrical fluid which departed from the exterior ſide of the battery; and that quantity is ſaid (in the theory of Dr. Franklin) to be equal to the quantity in the interior of the battery.

Therefore it is manifeſt that the number of exploſions or diſcharges of the electrical jar, is the number of equal meaſures of the electrical fluid which the battery contains.

But without putting too much confidence in any philoſophical theories, the effects of this operation may be more ſatisfactorily ſhewn by the following experiments:

Experiments.

A PIECE of iron wire, about 0.045 inches in diameter, and about two inches in length, was placed in the circuit through which the diſcharge of a ſmall electrical battery, which contained about ſix feet ſuperficial of coated glaſs, was to paſs.

The electrical jar employed as the meaſure of the charge of the battery contained about 90 ſquare inches; and the adjuſtment of the electrometer was varied in each ſet of experiments, by changing the weight applied to the balance, and alſo the diſtance of the diſcharging balls.

---

[*] Communicated by the Author.

Experiment 1.

THE electrometer being adjuſted with its leaſt weight; the diſcharging balls placed at the diſtance of one inch; and the other parts of the apparatus arranged as before deſcribed; the electrical machine was put in motion, the battery and alſo the jar began to receive a charge, as was ſhewn by the repulſion of a pith-ball on a graduated quadrant placed upon the electrometer:

1. After the firſt exploſion of the electrical jar, that is, after the battery had received one meaſure of the electrical fluid, a diſcharging rod was applied to complete the circuit in which the iron wire was placed; but, upon the diſcharge of the battery, no change of appearance was viſible in the wire.

2. The operation of the electrical machine being continued, the diſcharging rod, after two exploſions of the jar, that is, after the battery had received two meaſures, was applied as before; but, upon the diſcharge of the battery, no change appeared on the wire.

3. The battery was then charged with three meaſures; and upon diſcharging it as before, luminous particles of the wire were thrown off.

4. The battery, having received four meaſures, the wire, upon the diſcharge, exhibited nearly the ſame appearance as before.

5. The battery, having received five meaſures, was diſcharged; the wire was red-hot, and ſeparated.

6. The battery, having received ſix meaſures, was diſcharged; the wire was diſperſed in red-hot globules.

The battery, upon receiving between nine and ten meaſures, made a ſpontaneous diſcharge.

Experiment 2.

IN the ſecond ſet of experiments, the apparatus was arranged as before, and the electrometer adjuſted with the ſame weight, but the diſcharging balls were placed at the diſtance of two inches. The reſults were, upon diſcharging the battery, after having received

1 Meaſure,	-	No alteration in the wire.
2 Meaſures,	-	Luminous particles thrown off.
3 Meaſures,	-	The ſame, with ſmoke.
4 Meaſures,	-	Red-hot, and ſeparated.
5 Meaſures,	-	Diſperſed in red-hot globules.
Between 7 and 8 Meaſures,	-	A ſpontaneous diſcharge of the battery.

Experiment 3.

THE apparatus being arranged as before, the electrometer was adjuſted with its greateſt weight, and the diſcharging-balls placed at the diſtance of one inch: the reſults were, upon diſcharging the battery, after having received

1 Meaſure,	-	No alteration in the wire.
2 Meaſures,	-	Luminous particles thrown off.
3 Meaſures,	-	The ſame.

4 Meaſures, - Red-hot, and ſeparated.
5 and 6 Meaſures, - Red-hot, and diſperſed in globules.
Between 8 and 9 Meaſures, - A ſpontaneous diſcharge.

Experiment 4.

THE electrometer being adjuſted with the greateſt weight, the diſcharging-balls were placed at the diſtance of two inches. The reſults were:

1 Meaſure, - Luminous particles thrown off.
2 Meaſures, - The ſame, with ſmoke.
3 Meaſures, - Red-hot.
4 Meaſures, - The wire was diſperſed in red-hot globules.
Between 6 and 7 Meaſures, - A ſpontaneous diſcharge of the battery.

Experiment 5.

THE apparatus remained as in the fourth experiment, with the addition of another battery, containing 12 feet ſuperficial of coated glaſs; and the iron wire placed in the circuit of the diſcharge of this battery was about 0.08 inches in diameter, and two inches in length. The reſults were:

1 Meaſure, - No alteration in the wire.
4 Meaſures, - Luminous particles thrown off.
6 Meaſures, - The ſame, with ſmoke.
8 Meaſures, - Red-hot, and ſeparated.
10 Meaſures, - Diſperſed in red-hot globules.
Between 15 and 16 Meaſures, - A ſpontaneous diſcharge.

Harley-ſtreet; May 27, 1797. HENRY HALDANE.

The electrical machine employed in theſe experiments has a glaſs cylinder of nearly 18 inches in diameter. It was conſtructed by Mr. Nairne, and is a moſt powerful inſtrument, particularly in exhibiting all the phenomena in which the negative, or electricity of the rubber is concerned.

V.

On the Mechanical Conſtruction and Uſes of the Screw.

ELEMENTARY writers on mechanics, from demonſtrations founded on the leading property of the lever, teach us that the action of any power to raiſe a weight or overcome an obſtacle is facilitated by the inclined plane in the proportion of its length to its perpendicular height, when the power operates in the direction of the plane itſelf, or as the horizontal baſe to the height when the power acts in an horizontal direction. They alſo ſhew, by the eaſy proceſs of applying a right-angled triangle to the ſurface of a cylinder, that the thread of a ſcrew, when its axis is placed in the perpendicular, is in effect an inclined plane, whoſe length in one turn is meaſured by the portion of the thread itſelf, and height is the diſtance between two contiguous threads: that is to ſay, it would be juſt as eaſy to raiſe a weight by ſliding it along this thread, as along an inclined plane of the ſame length and height. The female ſcrew, or nut containing an hollow ſcrew, is in fact a weight adapted to this ſliding proceſs, and does accordingly correſpond with the weight on the ſimilar inclined plane, provided the power be applied at the very thread of the ſcrew; but if the advantage of a lever be taken to move the nut, the reſiſtance will be leſs in proportion as the radius of the ſcrew is leſs than the length of the lever.

Hence it is ſeen that the ſcrew affords a convenient and powerful application of the inclined plane to mechanical purpoſes. To compute its effect, whether a lever be uſed or not, is equally ſimple; for the power, wherever it be applied, is directed in a plane at right angles to the axis of motion; and the agent itſelf deſcribes the thread of a ſcrew conſtantly riſing through the ſame quantity in each turn. The proportion of the power to the weight, will therefore be as the diſtance between two contiguous threads to the circumference of the cylinder in which the power moves. Or in the form of a rule. Multiply the length of the lever by 44, and divide the product by 7; the quotient will bear the ſame proportion to the diſtance between thread and thread as the weight does to the power when in equilibrio.

In mixed hydraulic engines, ſuch as water-mills, pumps, and the like, the uſual addition allowed to overcome frictions, and give the velocity of working, is three-eighths of the power in equilibrio. The ſcrew requires more than this, but the quantity muſt vary according to the obliquity of the thread.

The obliquity is greater the coarſer the thread, and the ſmaller the diameter of the generating cylinder. For ſlow and ſtrong preſſures a fine thread is to be preferred; but for a quick ſtroke and ſpeedy return, as in the printing-preſs and coining-engine, the thread muſt riſe with conſiderable ſpeed.

The man of ſcience is moſt particularly intereſted in the conſideration of this mechanical organ, from the extreme accuracy with which it may be applied in the meaſurement of lines or ſpaces. Thus, for example, if the threads of a ſcrew be one-fiftieth of an inch apart, every turn in a fixed nut will cauſe its extremity to advance or recede that quantity: and if a graduated circular plate of one foot in diameter be fixed to the other extremity, the parts of each turn may be aſcertained. One quarter of a degree, or the 1440th part, on ſuch a circle, will be a very conveniently viſible diviſion, and will correſpond with the 1440th part of one-fiftieth of an inch, which is leſs than the ſeventy-thouſandth part of an inch.

A queſtion of no ſmall importance immediately preſents itſelf on this occaſion. Does the practice juſtify this theoretical concluſion? or, in other words, Do not the irregularities of workmanſhip ſhew themſelves to be much greater than the quantities here attempted to be meaſured? The ſolution, which will ſhew the true value of the meaſuring ſcrew, muſt be deduced from experiment.

The moſt obvious method of forming a ſcrew conſiſts in tracing helical lines upon the ſurface of a cylinder, and filing or cutting away the part between the intended threads. In this way ſome large ſcrews are actually made; but it is immediately perceived on reflection, that the original tracing is ſubject to all the inaccuracies of diviſion by hand; to which muſt alſo be added the ſtill greater errors of filing or cutting. By this proceſs alone it would be impracticable to make a ſcrew even of moderate correctneſs.

If a number of deep marks, ſuch as c d, be cut obliquely acroſs the thread with a three-ſquare file, in a ſteel ſcrew AB Figure 1. Plate IX. of the kind here deſcribed, and the ſcrew be then hardened, and tempered to the pale ſtraw-colour, it may be uſed to form the inſtrument Fig. 2. called a ſcrew-tool. For the ſcrew-tool being rendered quite ſoft by annealing, and fixed in a proper handle, will be cut into teeth by preſſing the end A againſt the ſcrew AB Fig. 1, while turning in the lathe. It is ſcarcely neceſſary to ſay that the ſcrew-tool will be ſupported by the Reſt, and that oil or tallow muſt be uſed to prevent the work from heating.

It is evident that the ſcrew-tool will be much more regular than the ſcrew it was made from; becauſe all the irregularities of the ſcrew will paſs in ſucceſſion through each of its teeth. If the ſcrew therefore be now ſoftened, and the ſcrew-tool hardened and tempered to ſtraw-colour, and whetted on the upper face, the former operation in the lathe may be repeated; but with this difference, that the ſcrew-tool will now cut the ſcrew, and render it ſtill more regular, for the reaſon juſt mentioned. After this rectification, it is uſual to deepen the groove between the threads by applying an acute angled file or tool to the ſcrew while turning.

The proceſs of rectification naturally ſuggeſts an improvement in the order of working. It is certainly much more eaſy to make a regular ſcrew-tool by hand than a ſcrew; more eſpecially when the thread is fine. Suppoſe Fig. 2. to be ſuch a ſcrew-tool, and the ſcrew part of AB Fig. 1. to be merely cylindrical. If the hard ſcrew-tool be then ſteadily applied againſt the ſoft cylinder while turning in the lathe, it will cut a number of equidiſtant circular grooves; but if, inſtead of being held in one place, the tool be regularly moved along the reſt, the grooves will be helical; and the ſucceeding teeth, falling into the marks made by thoſe which went before, will render the threads equidiſtant, and tend to produce more and more of regularity. Such a ſcrew may nevertheleſs vary in the obliquity of its threads on different ſides, and this imperfection will be very little amended by continuing the action of the ſcrew-tool. It would require too long a detail to ſtate the manipulations by which workmen acquire the facility of making ſcrews in this way with conſiderable regularity; and it is beſides probable that they would occur to any ingenious beginner as he proceeds.

Whether the tap (which is the name given to a ſcrew reſerved for making or tapping female ſcrews) be made and rectified in this or the other way, the next ordinary ſtep is to tap a pair of dies. Figure 5. Plate IX. repreſents a die for making ſcrews. Its thickneſs is ſhewn in Fig. 6; and in Fig. 3. a pair of ſuch dies are ſeen properly diſpoſed in the frame ABCD, called a ſtock. The thickneſs of the ſtock is preciſely equal to that of the dies, which are well fitted to ſlide in the opening GHIK. On the lower ſide of the ſtock, as it now lies, is fixed, by rivets or ſcrews, a flat plate of the dimenſions of Fig. 4, the width of the opening PPPP being ſomewhat leſs than that of GHIK Fig. 3. It therefore affords a ſupport to prevent the dies from falling through; and the plate Fig. 4. being applied on the upper ſide of the ſtock ſerves effectually to prevent any motion whatever in the dies, except the direct approach or receſs with regard to each other by means of the ſcrews EF. The top plate is ſecured by four ſcrews LLLL, which paſs through holes of the Figure repreſented in the ſketch. The convenience of theſe holes is, that the plate may be taken off to change the dies without actually drawing the four ſcrews. A variety of faſhions prevail in the form of ſtocks, which are ſuppoſed to have their reſpective advantages.

tages. Steadineſs, accurate fitting, and a regular fair action of the end preſſure, are the eſſential requiſites of a good ſtock. The drawing here given is one of the moſt ſimple. When it is uſed for ſtrong work, the ſtock is fixed in a piece of wood terminating in a handle on each ſide.

Suppoſe the dies NN Fig. 3. to be ſoft; and a notch A Fig. 5, of about ſixty degrees of the circumference of the tap E Fig. 1, not reckoning the thickneſs of the thread, filed in each. Let them be placed in the ſtock, two pieces of braſs OO being put in to defend them againſt the thumb-ſcrews, and the whole ſecured by the face plate. The dies are then ready for tapping. The hardened tap AB muſt be fixed in the vice, with its end B uppermoſt, and the dies be then made to bite gently upon its ſcrew. When the ſtock in this ſituation (the cutting parts being previouſly well oiled) is moved backwards and forwards on the axis of the ſcrew, the tap cuts an hollow thread in the dies, which becomes deeper and deeper as the ſcrews E and F are driven inwards. The dies muſt be taken out and examined from time to time during the proceſs, which is to be continued until the thread is found to be as deep and perfect as may be expected. A notch C Fig. 6. is then to be filed in the bottom of the thread, the dies hardened and tempered, and the face A Fig. 5. whetted on the Turkey hone. The dies are in this ſtate ready for tapping other ſcrews.

For the ſame reaſon why an irregular ſcrew will cut a more regular ſcrew tool (Fig. 2.), it happens that the female ſcrew is more regular than the male. The dies will therefore rectify the tap to a ſtill greater degree of accuracy; and this tap, if thought neceſſary, may be again uſed to rectify the dies.

Excellent ſhort ſcrews have been made by very ſlow and careful tapping with new ſharp dies. The principal ſources of error in this proceſs are the following:

1. The ſtop from change of hands produces a wave in the thread. This is frequently perceptible to the eye, and may be partly remedied by the workman walking round his work, or frequently ſhifting the ſcrew in the vice.

2. Preſſure in the direction of the axis, by one hand more than the other, occaſions a periodical change in the obliquity of the thread, and the ſcrew is what the workmen call *drunk*. It is difficult to avoid this error when the ſcrew is very ſhort, or the dies thin.

Mechanical means have been uſed to remove theſe two ſources of error, by fixing the ſtock, and confining the tap to move by a regular rotation in the direction of its own axis.

3. Since the corners of the dies cut firſt, there will be little diſpoſition in the firſt ſtrokes to form the required ſcrew. If the operation be not favoured by preſſure in the direction of the axis, the dies will follow accidental irregularities of the ſurface or of the material; and as probably cut circular rings, or a left-handed ſcrew, as the right-handed ſcrew ſuppoſed to be in the dies. In theſe circumſtances, therefore, the thread frequently proves undulated, with very little riſe in the run of each corner of the die, until it ſuddenly falls into the cut made by the corner it follows. Each turn conſiſts accordingly of four waves, which are amended as the dies ſink deeper, and are led by their own ſlope. In extreme ſtrictneſs it may be queſtioned whether a tapped ſcrew be ever begun without this irregularity, or perfectly corrected by the ſubſequent operation.

4. If the dies be not well fitted in the ſtock, or the material to be cut be veiny or unequally hard, they will recede from the hard parts, and produce an irregular thread. Long dies are leaſt ſubject to this imperfection.

5. The oppoſite extremes of the thread in the dies incline towards different regions, and therefore in effect croſs each other. For this reaſon, it is impoſſible for two dies to be made to approach each other in the direction of the thread. In fact, they approach in a plane at right angles to the axis. This is the circumſtance which limits the diameter and the depth and inclination of the cut, beyond which dies cannot operate. Hence alſo it is, that a true flat-thread ſcrew cannot be cut in dies, and a many-threaded ſcrew, or ſcrew of great obliquity, can only be cut in the common ſtock by two or three pair of dies in ſucceſſion.

6. As it is certain that a pair of well fitted dies can never run both along the ſame ſtroke till perfectly home to their natural place of coincidence, the cut made by one die will tend to draw the other along the cylinder. So that while one die cuts the upper ſide of the thread, the other die will cut the oppoſite or under ſide. In this croſs action, the frame and the dies themſelves will yield from elaſticity, and that the more where the material is the hardeſt, or the work forced. Hence with a like preſſure the ſoft ſide will have the wideſt cut, and be ſooneſt cut down, and the ſides of the thread will be waving. This ſeems to be the chief reaſon why tapping a ſcrew renders it leſs ſtraight and round than before.

7. The dies operate, rather in conſequence of the force of preſſure by means of the ſcrews of the ſtock than by the effect of their edge. This force muſt not only be productive of an oval figure and other irregularities, from the uncertain ſpring and yielding of the inſtrument and material, but muſt probably occaſion a change, of the nature of lamination or hammering. Whenever this laſt event happens the thread will be thinner, the cut wider, and the ſcrew itſelf coarſer than it would otherwiſe have been. I have three flat-thread ſteel ſcrews, each 11 inches long, 0.6 in. outſide diameter, and depth of thread near 0.1 inch. They were tapped in dies 0.9 inch deep produced by an original tap containing 13 threads in 1.4 inch. The motion was ſlow and uniform between the centres of a kind of lathe conſtructed for the purpoſe. None of the ſcrews are of the ſame uniform fineneſs from end to end. One ſcrew is ſcarcely coarſer than the original tap; another is half a thread coarſer in 102 threads, or 11 inches; and the third is a whole thread coarſer in the ſame quantity. Theſe differences ſeemed to ariſe from the different degrees of ſoftneſs in the ſteel; the laſt having been more annealed than the ſecond, and the firſt not at all.

Hence is deduced the important concluſion, that however we may by mechanical expedients diminiſh the irregularities, of obliquity and diſtance of contiguous threads, in a tapped ſcrew, yet in ſcrews of ſeveral inches long there is an irregularity growing out of the nature of the materials, which does not appear to admit of any other remedy in this method than extreme care and ſlowneſs of working.

The regular inclination of a ſhort tapped ſcrew of about forty threads in the inch, was found, by experiments at the end of a very ſhort lever or radius ſet by a ſpirit-level, to be diſcernible in much leſs quantities than the hundred thouſandth part of an inch.

Since all theſe ſources of irregularity in tapping would be much diminiſhed by increaſing the number of dies, I have noted among my Agenda a ſtock with four pair of dies acting at once with eight edges. In theſe the error of obliquity in coming up, as remarked in the numbered paragraph 5, and of the firſt run (paragraph 3.) are incomparably leſs than in a ſingle pair of dies. As I have not yet made it up, and every new inſtrument is likely to be altered and ſimplified during the actual conſtruction, I ſhall not particularly ſtate either the dimenſions, or ſuch circumſtances as are neceſſary to inſure the truth of workmanſhip.

Fig. 7. Plate IX. repreſents a ſtock containing eight dies. HHHH repreſents a braſs ring with parallel faces. It is perforated ſtraight through at the circle DDD. Upon a plate of braſs of the ſame diameter as the ring, are fixed the eight pieces marked with the letter A, which afford grooves for eight dies marked B to ſlide in. When the braſs plate is applied to the ring H, the pieces A with the dies occupy the internal part of the ring except the central hole at C; and in this ſituation the oppoſite or upper face of H forms one plane with the pieces A and the dies B. Every die is thruſt forward when required by the action of a nut F upon a ſcrew EK, which is prevented from turning by the form of the inner end K. The nuts F are properly ſecured in the ring H, ſo as to turn fairly and without ſhake, at the ſame time that they are ſupported againſt the re-action of the dies. A covering plate LLL is ſcrewed on to ſecure the dies and nuts in their places. The nuts F are provided with equal and ſimilar ſmall wheels, which juſt riſe above the plane of the ring, and are driven by a contrate wheel, attached to a ſecond covering plate, rather larger in diameter than the ring H. The edge of this laſt plate is milled, and it turns upon and is ſecured in its place by a central ring GGG fixed on the firſt covering plate by three binding ſcrews.

The uſe of this ſtock and dies needs very little explanation. Suppoſe the eight dies to be regularly numbered and put into their places; the pinions F all accurately ſet with a marked tooth uppermoſt in each; the upper plate ſcrewed on, and the contrate wheel piece applied to the pinions in a ſituation marked by the coincidence of diviſions made for that purpoſe on the central piece G. Imagine the ſtock in this ſituation to be well ſecured againſt the face of an upright poppet head, and another head at a proper diſtance to ſuſtain a metallic hole lying truly in the axis of this apparatus. Let the finiſhed and hardened tap be provided with a cylindrical tail to move in this hole, while the ſcrew part is ſupported by the faces of the eight dies. In this ſituation let the tap be gently turned by a handle of ſuch a conſtruction, that by means of a joint near the centre, and another near the holding part, it ſhall not be poſſible for the hand to exert any bearing in the direction of its axis. By a careful continuation of this proceſs, uſing oil, and ſetting up the ſyſtem of back ſcrews a very little at a time, the dies will at length receive the ſcrew, and may be rendered perfect by the ſeveral proceſſes of rectification before mentioned.

With ſuch an inſtrument, in ſkilful hands, I think the proceſs of tapping might be fairly tried in the conſtruction of fine meaſuring ſcrews, even in the hardened and tempered ſtate. A leading ſcrew might be uſed to drive the cylindrical part, if thought neceſſary, for the firſt taking of the dies. For notwithſtanding all the difficulties of the method by dies, and the improvements of ingenious artiſts in the method of turning ſcrews by a tool mechanically carried along the axis, there are ſufficient reaſons to wiſh that the tapping proceſs, on which they are originally founded, ſhould be brought to perfection. But the preſent communication is already of ſuch a length, as to require that the value and importance of thoſe methods ſhould be diſcuſſed in a future paper.

VI. *The*

VI.

The Method of obtaining the Fixed Alkalis in Cryſtals of the greateſt Purity. By LOWITZ, *Profeſſor, &c. at Peterſburg* [*].

1. THE cauſtic alkalis, as obtained by the ordinary proceſſes, are ſufficiently pure for the purpoſes of pharmacy and the arts; but for the experiments of philoſophical chemiſtry the greateſt purity is required in every ſubſtance made uſe of.

2. Cauſtic alkalis have hitherto been very ſeldom uſed in the analyſis of mineral ſubſtances. M. Klaproth [†] has ſhewn their utility in analyſes, not only in effecting a more eaſy ſeparation of aluminous earth, but likewiſe to diſunite the principles of the moſt refractory foſſils. This circumſtance has induced me to give a more particular deſcription of the diſcovery I made ſome years ago of the cryſtallization of the cauſtic alkalis, as the principal means of obtaining them in a ſtate of abſolute purity. I have likewiſe embraced this opportunity of rectifying the miſtake of various chemiſts, who have imagined that cryſtallization cannot take place but during the moſt extreme cold of winter.

3. My firſt experiments on vegetable alkali were made in the hotteſt days of ſummer, and the moſt beautiful cryſtallization was formed even over the fire.

4. The cryſtallization of ſoda does not ſucceed but in winter; but the ordinary cold of five degrees (R.) is ſufficient for this effect. The reaſon why this alkali requires ſo low a temperature ariſes from the property its cryſtals poſſeſs of being ſoluble at the ſlighteſt heat in their own water of cryſtallization. The ſame thing is obſervable in the muriate of ſoda or common ſalt cryſtallized by cold [‡].

5. Before the diſcovery of this method, no one had ſucceeded by the ordinary proceſſes to obtain a colourleſs lixivium of cauſtic alkali. Theſe ſolutions were always more or leſs brown. By repeated cryſtallizations this lixivium may be had in the moſt concentrated ſtate, as limpid as the pureſt water; which inconteſtably proves that the common lixivia are contaminated by heterogeneous matters, which charcoal and the ſtrongeſt calcination are incapable of deſtroying.

6. The whole of the operation for obtaining a cauſtic alkali of the greateſt purity and without the leaſt colour, conſiſts in this: A cauſtic lixivium of pot-aſh is evaporated to a thick pellicle. After the cooling, the foreign ſalt which has cryſtallized is to be ſeparated, and the evaporation of the lixivium continued in an iron pot, as in the preparation of the lapis cauſticus. During this ſecond evaporation the pellicle of foreign ſalts, particularly of carbonate of pot-aſh, which continues to be formed, muſt be carefully taken off with an iron ſkimmer. When no more pellicle is formed, and the matter ceaſes to boil up, it is removed from the fire, and ſuffered to cool, with continual agitation by an iron ſpatula. It is then to be diſſolved in double the quantity of cold water, the ſolution filtered,

---

[*] Von Crell's Chemiſche Annalen auf 1796, b. i. ſ. 306.

[†] Beytrage zur Chemiſchen Kenntniſs der Mineral Koerper, b. i.

[‡] Lowitz obtained this ſingular cryſtallization of the muriate of ſoda by expoſing a ſolution of this ſalt to intenſe cold. The cryſtals, which are hexagonal, are two inches in diameter, and one line thick. They liquefy at a few degrees below the freezing point of water, and fall into a very fine white powder at an extremely low temperature. Note of Van Mons in the Annals of Chemiſtry, XXII. 27.

and evaporated in a glaſs retort, till it begins to depoſit regular cryſtals. If the maſs ſhould conſolidate ever ſo little by cooling, a ſmall quantity of water is to be added, and it muſt be heated again to render it fluid. After the formation of a ſufficient quantity of regular cryſtals, the fluid, which is very brown, is to be decanted, and the ſalt, after being ſuffered to drain, muſt be re-diſſolved in the ſame quantity of water. The decanted liquor muſt be kept in a well cloſed bottle, and ſuffered to become clear by ſubſidence for ſeveral days. It muſt then be decanted for a ſecond evaporation and cryſtallization. The proceſs muſt be repeated as long as the cryſtals afford, with the leaſt poſſible quantity of water, ſolutions perfectly limpid. Theſe ſolutions are to be preſerved in well cloſed bottles, to defend them from the acceſs of air.

7. The greateſt difficulty of this proceſs ariſes from the facility with which the lixivium aſſumes the ſolid form. To obviate this inconvenience, a ſmall portion of the lixivium may be concentrated to the point at which it becomes converted into a ſolid maſs by cooling. The ſaturation of a lixivium conſiderably evaporated may be aſcertained by throwing ſmall pieces of this maſs into it during its cooling. When theſe are no longer diſſolved, it is a proof that the lixivium is at the required point. I have ſhewn, in a particular article on the cryſtallization of ſalts, the principles on which this practice is founded *.

8. This method of proceeding is abſolutely indiſpenſable in the cryſtallization of ſoda, which in other reſpects is made in the ſame manner as that of pot-aſh. The alkali would not fail, without this management, to form a ſolid maſs during its cooling.

9. With regard to the foreign ſalts which are mixed with the pot-aſh, the greateſt portion ſeparates by cryſtallization after the firſt evaporation of the lixivium. The reſt is ſeparated during the ſecond concentration by the continual ſkimming of the pellicle. The little which may remain with the pot-aſh muſt precipitate, for want of water of ſolution, in a lixivium wherein the alkali itſelf is no longer diſſolved but by its own water of cryſtallization.

10. The property of cauſtic alkaſis to diſſolve in highly rectified alcohol, with the excluſion of every foreign ſalt, would afford an excellent means of obtaining this ſalt very pure, if their mutual action did not afford a new ſource of impurity. For when an alkali abſolutely pure and cryſtallized is diſſolved in ſpirit of wine, even without heat, the fluid aſſumes a very brown colour, which becomes ſtill deeper after decantation from the ſaline maſs.

11. The cryſtallization of pot-aſh is very different, accordingly as the cryſtals are formed with cold or heat. In the firſt caſe, the cryſtals obtained are octahedrons in groups, which contain 0.43 water of cryſtallization, and excite by their ſolution in water, even in the ſummer, a degree of cold very near the point of aqueous congelation. In the ſecond caſe, cryſtalline tranſparent very thin blades, of extraordinary magnitude, are formed, which, by an aſſemblage of lines, in directions that croſs each other to infinity, preſent an aggregate of cells or cavities, moſt commonly ſo perfectly cloſed that the veſſel may be inverted without the eſcape of the ſmalleſt drop of the lixivium, though ſometimes included to the quantity of an ounce or two. For this reaſon it is neceſſary to break this fine cryſtallization, that the fluid may run off. The cryſtals preſent in their regular formation rectangular tetragonal blades, which, as they contain little water of cryſtallization, produce a conſiderable degree of heat when diſſolved in water.

---

* Chem. Annal. auf 1795, b. i. ſ. 6.

12. By expoſing ſuch alkaline cryſtals to a red heat in a very clean crucible, they become fuſed; and, after cooling in proper moulds, afford a lapis cauſticus as white as ſnow, and extremely cauſtic and deliqueſcent.

13. As the cryſtals and the lixivium, during the length of time required to drain the ſalt, may frequently become charged with a portion of carbonic acid, it is adviſable, in order to avoid this inconvenience as much as poſſible, that the lixivium, as ſoon as it is brought to the requiſite point of ſaturation, ſhould be poured into a narrow-necked bottle, and well cloſed therein to cryſtallize. After the cryſtals are formed, the bottle is to be reverſed, without opening, and kept at a temperature rather warm until the cryſtals are well dried. During the winter, the liquor, after the firſt cryſtallization, continues to cryſtallize without being ſubmitted to a new evaporation, provided only that it be expoſed to a temperature ſomewhat colder than that wherein the firſt cryſtals were formed *.

VII.

Experiments on Eau de Luce. By a Correſpondent.

To Mr. NICHOLSON, *Editor of* "*The Journal of Natural Philoſophy, Chemiſtry, and the Arts.*"

SIR,

OBSERVING in your ſecond number a paper on the compound called Eau de luce, I here ſend you the reſult of ſome curſory experiments, which I was induced, for the benefit of my female friends, to make ſome years ſince on this article. You will perhaps yourſelf proſecute them farther than I have had an opportunity of doing.

1. Macquer's recipe, and that in the London Pharmacopeia, were both tried; but neither of them ſucceeded. I was however informed by a very intelligent apothecary, that the beſt eau de luce might be, and always was, made without any other ingredient than oil of amber, ſoap, alcohol and ammoniac: he added, that thoſe who are judges of this article detect in a moment the preſence of maſtic, and eſteem ſuch as contains it of very little value. The proportions were accordingly varied. Different ſpecimens of the oil were uſed—ſome of the firſt diſtillation, others twice rectified—but with no better ſucceſs.—The beſt product was too highly coloured. When daſhed againſt the ſides of the phial, it did not flow back uniformly, but preſented a ſomewhat greaſy appearance, not much unlike that of a common emulſion with oil and an alkali; and after ſome days the oil ſeparated like a cream on the top, and the liquor became ſemi-tranſparent.

2. Being aſſured from a number of enquiries, that maſtic was the moſt ordinary baſis of the emulſion, this was alſo tried, and ſucceeded tolerably at firſt. The milky appearance is

---

* Cryſtals of cauſtic pot-aſh may alſo be obtained by the ſucceſſive evaporation of a ſolution of the pot-aſh of commerce until the appearance of a light pellicle. The firſt cryſtals which appear are the alkaline carbonate, or mild alkali, and the latter the pure pot-aſh. In this way the lixivium is reduced into very regular cryſtals, to the very laſt portions of ſalt which it contains. Half an ounce, the reſidue of a ſolution of ſeveral pounds, ſtill affords very regular cryſtals. It is even remarkable, that the latter cryſtallizations are formed with infinitely more facility than the firſt. I inſerted this obſervation in a Memoir on the preparation of the carbonate of pot-aſh for the alkaline mephitic water of Colburn, which I addreſſed at the beginning of 1793 to the Société Philomatique de Paris, and has in part been printed in L'Eſprit des Journaux for the ſame year. Note of Van Mons.

however not ſufficiently permanent. After a certain time—which is longer in proportion to the weakneſs of the reſinous ſolution, and the ſtrength and cauſticity of the ammoniac—this alſo ſeparates, and the maſtic forms a hard adheſive coagulum which floats on the ſurface of the liquor. This effect is much accelerated by frequently opening the phial, but retarded by the addition of alcohol.

3. Elemi ſucceeds as well, or better. The ſame obſervations are applicable when this is uſed, as in the preceding caſe. The precipitate, however, when formed, ſubſides to the bottom of the liquor. It is more bulky, and not ſo hard as that of the maſtic. It is not unlike that which is formed in ſuch ink as contains much gum and alum.

4. A mixture of elemi and maſtic in ſolution ſeems much ſuperior to either; and would perhaps remain ſuſpended any length of time, if the ingredients were ſo apportioned as to make the compound precipitate as nearly as poſſible of the ſame ſpecific gravity as the liquor. The beſt product was obtained nearly as follows; but the ingredients not having been accurately weighed, the quantities are only given by eſtimation.—Digeſt ten or twelve grains of the whiteſt pieces of maſtic, ſelected for this purpoſe and powdered, in two ounces of alcohol; and, when nearly diſſolved, add twenty grains of elemi. When both the reſins are diſſolved, add ten or fifteen drops of rectified oil of amber, and fifteen or twenty of eſſence of bergamot: ſhake the whole well together, and let the fæces ſubſide. The ſolution will be of a pale amber colour. It is to be added in very ſmall portions to the beſt aqua ammoniæ puræ, until it aſſumes a milky whiteneſs—ſhaking the phial well after each addition, as directed by Macquer. The ſtrength and cauſticity of the ammoniac are of moſt eſſential conſequence. If, upon the addition of the firſt drop or two of the tincture, a denſe opake coagulated precipitate is formed—not much unlike that which appears on dropping a ſolution of ſilver into water ſlightly impregnated with common ſalt—it is too ſtrong, and muſt be diluted with alcohol. A conſiderable proportion of the tincture, perhaps one to four, ought to be requiſite to give the liquor the proper degree of opacity. I have frequently ſince made an eau de luce in this way, in which no depoſition has afterwards appeared.

5. The reſin formed from oil of amber by the nitric acid was, after boiling it in water, tried for this purpoſe. It was itſelf of a light chocolate colour, and produced an opake fawn-coloured liquor. Common yellow reſin formed a coagulum like that of maſtic, but almoſt inſtantly. Its colour was bad, and its fœtor too ſtrong to be diſguiſed.

6. Balſam of copaiba was tried, and appeared to be a very good ſubſtance for this purpoſe; but ſome other objects of enquiry intervening, the experiments were diſcontinued. Copal is, it is ſaid, equal, if not ſuperior, to any of the reſins; particularly on account of its having a leſs unpleaſant odour than moſt of them. I have not tried it myſelf; but, from the deſerved reputation of ſome trading chemiſts who I know make their eau de luce of it, I have no doubt of its anſwering extremely well [*].

I am, Sir, your moſt obedient Servant,

12th *June* 1797. J. F—:—:—:—:—R.

---

[*] The theory of opake fluids depends on conſiderations of an optical, mechanical, and chemical nature. For, 1. Such a fluid muſt conſiſt of parts that differ in their refractive power, and are not ſmaller than a determinate magnitude. Newton's Optics, III. Prop. 4. 2. The denſer particles will deſcend with velocities nearly uni-

form,

VIII.

An Account of Experiments deſcribed, and in part repeated, at the Sitting of the National Inſtitute of France, on the 15th Germinal, in the Year 4. By Citizens FOURCROY *and* VAUQUELIN*: On Detonations produced by Concuſſion*[*].

THE energy and rapidity with which the ſuper-oxigenated muriate of pot-aſh inflames and burns moſt combuſtible ſubſtances, is the ſource from which the Citizens Fourcroy and Vauquelin have derived the new facts which they have communicated to the National Inſtitute of France, and exhibited at the public ſitting of the 15th Germinal, in the 4th year of the Republic. Several chemiſts had obſerved, that this ſalt, diſcovered by Berthollet about nine years ago, and which ſeems to contain the elements and phenomena of thunder, decrepitated and emitted ſmall electrical ſparks by friction, and ſpontaneouſly took fire with ſulphur. And an unfortunate experiment made in October 1788, at Eſſone, had proved, that when ground with ſulphur and coal, to convert it into gunpowder, it takes fire ſpontaneouſly. Such were the firſt obſervations made by Lavoiſier, Pelletier and Van Mons, when Citizens Fourcroy and Vauquelin undertook to examine the ſeries of effects of the ſuper-oxigenated muriate of pot-aſh on the ſeveral known combuſtible bodies. The following are the principal facts which they have diſcovered:

1. Three parts of ſuper-oxigenated muriate of pot-aſh, and one part of ſulphur in powder, triturated in a metallic mortar, cauſed numerous and ſucceſſive detonations to be heard, reſembling the cracks of a whip, the report of a piſtol, or even that of muſquetry, according to the rapidity of the motion, or force of preſſure applied. A few centigrams (or fifth parts of a grain) of the ſame mixture, ſtruck briſkly with a hammer on an anvil, exploded with a noiſe ſimilar to that of a muſket, and torrents of purple light were ſeen round the anvil. The ſame mixture thrown into concentrated ſulphuric acid immediately takes fire, and burns without noiſe with a white dazzling flame.

2. Three parts of the ſalt, half a part of ſulphur, and half a part of charcoal, afford ſtronger detonations than the foregoing, when triturated in the mortar, and a more conſiderable noiſe when ſtruck on the anvil. The flame of this mixture, when cauſed to detonate, or when thrown into the ſulphuric acid, is more rapid, ſtrong and red than that of the preceding mixture.

3. Equal parts of ſuper-oxigenated muriate of pot-aſh, and of antimony in powder, fulminate by the ſtroke, and produce only reddiſh ſparks with the ſulphuric acid. Zinc at a light doſe alſo fulminates with a white flame, but ſuffers no change by the ſulphuric acid.

4. Metallic arſenic detonates very ſtrongly by the ſtroke of the hammer, and takes fire with rapidity and extraordinary brilliancy by the contact of ſulphuric acid. In this laſt

---

form, and cæteris paribus in the direct ſubduplicate ratio of their diameters. Atwood on Motion, Sect. V. Prop. 10. And 3. The magnitude, and perhaps denſity, of all chemical depoſitions are affected not only by the nature of the ingredients and the quantity of ſolvent, but very much by the order of mixture and other circumſtances of manipulation. This laſt obſervation ſerves to account for the various reſults of theſe and many other experiments in which tranſparency, opacity, or colour, are required to be produced. N.

[*] Annales de Chimie, XXI. 235.

experiment a ſmoke ariſes which aſſumes the form of a crown, in the ſame manner as phoſphorated hydrogenous gas when ſpontaneouſly inflamed in a calm atmoſphere.

5. The ſulphure of iron, or martial pyrites, inflames rapidly, but without noiſe, when triturated in a metallic mortar with the ſuper-oxigenated muriate of pot-aſh. This mixture, when ſtruck on the ſteel anvil, detonates ſtrongly with a red flame.

6. The red ſulphure of mercury, or cinnabar, and the ſulphurated oxides of mercury, detonate by the ſtroke with the ſuper-oxigenated muriate of mercury (q. pot-aſh); but they do not take fire with the ſulphuric acid. Charcoal alone mixed with this ſalt has the ſame effects.

7. Sugar, the gums, fixed and volatile oils, alcohol, or ether mixed with the ſuper-oxigenated muriate of pot-aſh, ſo that theſe laſt liquid combuſtible ſubſtances formed a paſte with the ſalt, have the property of fulminating very ſtrongly by the ſtroke of the hammer. They all emit a very ſtrong flame on detonation. Theſe mixtures do not detonate nor take fire by ſimple trituration. Some of them take fire in the concentrated ſulphuric acid. Their combuſtion in this caſe is ſlow and ſucceſſive.

8. All the ſubſtances before mentioned, which, when mixed with the ſuper-oxigenated muriate of pot-aſh, take fire and burn in an inſtant, and with conſiderable noiſe, by the rapid preſſure of the ſtroke of the hammer, produce a much ſtronger detonation when wrapped up in double paper, which compreſſes them together before the ſtroke.

9. The electric ſhock from a ſtrong machine, by the charge of a battery of large ſurface, cauſes all the foregoing mixtures to detonate in the ſame manner as concuſſion; and in this caſe alſo they emit a ſtrong light.

To all theſe facts the authors add, that it was already known that gunpowder detonates by a violent blow or ſudden preſſure; but they obſerve, that the ſtroke muſt be much ſtronger than is neceſſary to produce fulmination in all the mixtures of combuſtible ſubſtances with the ſuper-oxigenated muriate of pot-aſh, and that its detonation is far from being equally remarkable with thoſe produced by the new ſalt.

With regard to the theory of this ſingular phenomenon, it appears to them to be the ſame as that publiſhed by Berthollet relative to the detonation of fulminating ſilver by the ſlighteſt contact. Preſſure, and more particularly that which is effected in a very ſhort time, as by a blow, favours the union of oxigene with the combuſtible body. This combination effected by the oxigene, ſeparated all at once from the ſuper-oxigenated muriate of pot-aſh, is accompanied by a ſudden expanſion, and inſtantaneous formation of gaſeous matters, which ſtrike and compreſs the ſurrounding air with ſuch velocity as to produce a conſiderable noiſe. The light, the vapour, and the odour peculiar to each combuſtible body made uſe of, prove that a true inflammation takes place, and that the ſtrong detonation is owing to its violence and rapidity.

The inflammation produced by the concentrated ſulphuric acid ariſes from the diſengagement of the ſuper-oxigenated muriatic acid in gas, by means of which the combuſtible matters mixed with the ſalt take fire ſtill more rapidly than in the ordinary oxigenated muriatic acid gas.

IX.

A Memoir on the Combination of Oils with Earths, Volatile Alkali, and Metallic Subſtances.

By M. BERTHOLLET[*].

SOAP agrees ſo far in its properties with neutral ſalts, that oil muſt form in the manner of acids a great number of combinations hitherto neglected. This notion led me to make the experiments I am about to relate.

The action of oil upon calcareous earth has long been known; but Mr. Coſtel was the firſt who deſcribed the method of accurately combining theſe two ſubſtances, by pouring a ſolution of ſoap into lime water[†].

The lime unites to the oil of the ſoap, and forms a combination which is inſoluble, and may be retained on the filter while the cauſtic alkali is ſet at liberty, and may be ſeparated by evaporation. It retains a ſmall quantity of oil, which, according to Mr. Thouvenel, may be waſhed off with ſpirit of wine[‡]. This chemiſt remarks, in his analyſis of the waters of Contrexeville, that the cauſtic alkali cannot decompoſe calcareous ſoap; ſo that we may rigorouſly affirm, that oil has a ſtronger affinity to calcareous earth than to fixed alkali; but according to the ſame chemiſts, if the efferveſcent fixed alkali be poured on calcareous ſoap, this laſt is decompoſed, the alkali unites with the oil, and the calcareous earth becomes diſengaged at the ſame time that it acquires the property of efferveſcence. The diſcoveries made ſince this diſſertation was written, explain what happens upon this occaſion. A double decompoſition and recompoſition take place. The cretaceous acid unites with the calcareous earth of the oleo-calcareous combination, and the oil of the ſame combination unites with the alkali thus deprived of its cretaceous acid.

M. Thouvenel on this occaſion makes a reflection of too great importance to be omitted. Phyſicians often preſcribe the uſe of ſoap and lime together, or lime water, without attending to the change and decompoſition which reſult from the mixture of theſe two ſubſtances. For, in this caſe, it is the cauſtic alkali diſengaged from the ſoap which becomes the active medicament; and conſequently that the effects of ſuch mixtures muſt vary according to the proportions and other concomitant circumſtances.

I have likewiſe examined the effects of volatile alkali upon calcareous ſoap. The cauſtic volatile alkali had no ſtronger action upon this combination than the cauſtic fixed alkali; but the volatile efferveſcent alkali decompoſed it in the ſame manner as the efferveſcent fixed alkali. The volatile alkali aſſumed the appearance of an oil, and the earth remained at the bottom in the efferveſcent ſtate.

After having decanted this ſaponaceous ſubſtance, I evaporated the ſuperfluous volatile alkali at a gentle heat, and there remained a ſoap of a more pungent taſte than common ſoap, and ſomewhat leſs conſiſtent. It became decompoſed by long expoſure to the air. Spirit of wine diſſolves it well, but the quantity taken up by water is extremely ſmall. This laſt property convinced me that there was no need of ſo complicated and ſo long a proceſs to make this ſoap; and that, for this purpoſe, I might make uſe of the action of double affinities in a different manner.

---

[*] Acad. Par. 1780.

[†] Analyſe des Eaux de Pongues.

[‡] Qu. M. Berthollet has ſince ſhewn the cauſtic alkali is alſo ſoluble in that fluid. See alſo Philoſ. Journal, I. 165. N.

I therefore mixed a ſolution of common ſoap with a ſolution of ſal ammoniac, and I ſaw clots formed at the ſame inſtant, which conſiſted of the ammoniacal ſoap, and were retained on the filter; ſo that the fixed alkali of the ſoap united with the marine acid, while the volatile alkali combined with the oil. Strongly perſuaded as I am, that new medicines ought not to be propoſed but with extreme circumſpection; and that we ſhould rather endeavour to diminiſh than increaſe the liſt of materia medica, I am nevertheleſs tempted to propoſe the medical uſe of this ſoap, which muſt poſſeſs more active virtues than the common ſoap, and which has the advantage over that of Starkey, of a very eaſy and ſpeedy compoſition, uniformity of properties, and is capable of being preſerved in cloſe veſſels. I am aware that uſe has been made in medicine of a mixture of volatile alkali and oil, of which a combination is attempted to be made by agitation or triturating; and that a mixture of this kind is to be found in the London Pharmacopeia, under the name of volatile liniment. But the combination obtained in this way is very imperfect, and totally different from the ſoap here deſcribed, as may be eaſily ſeen by inſpection.

When common ſoap is mixed with ſelenitic water, two decompoſitions and two new combinations take place, as Mr. Coſtel has proved. The alkali of the ſoap unites with the acid of the ſelenite, and the earth of the ſelenite combines with the oil of the ſoap, forming an inſoluble oleo-calcareous matter in flocks, which cannot anſwer the purpoſes of ordinary ſoap. Hence the ſelenitic waters have been diſtinguiſhed by the name of hard water. Selenite, however, is not the only ſubſtance proper to form the oleo-calcareous combination; for every ſolution of this earth is equally proper. The ſolution in the marine or nitrous acid may be uſed for this purpoſe. When ſoap is therefore decompoſed in hard waters, the effect depends not only on the ſelenite and the calcareous earth held in ſolution by carbonic acid, but likewiſe upon all the ſalts with baſis of lime, or even magneſia, which may exiſt in the water, as will hereafter be ſhewn.

The mixture of a ſolution of ſoap and a ſolution of Epſom ſalts afforded a combination of the utmoſt whiteneſs. It is unctuous, dries with difficulty, and preſerves its white colour after deſiccation. It is inſoluble in boiling water, but has nevertheleſs a decided taſte of ſoap. Expreſſed oil, as well as ſpirit of wine, diſſolves it in conſiderable quantity; when water is added to the ſolution in the latter fluid, it becomes milky. This combination melts with a moderate heat, and forms a tranſparent maſs ſlightly yellow and very brittle. The oleo-calcareous combination cannot be fuſed but very imperfectly, and at a much ſtronger heat.

I have combined oil with clay by mixing a ſolution of alum with a ſolution of ſoap. The reſult of this mixture was a flexible combination, ſoft to the touch, which preſerves its ſuppleneſs and tenacity in drying. It appeared to me to be inſoluble in water, ſpirit of wine, and oil. It very readily enters into fuſion, after which it exhibits a maſs of a beautiful tranſparence rather yellow.

The ſolution of ponderous earth in marine acid, afforded with ſoap a combination nearly the ſame in appearance and properties as the calcareous compound.

The very ſimple method uſed in theſe experiments to form combinations of oils and earths, was attended with equal ſucceſs when applied to metallic ſubſtances.

When a mixture of the ſolutions of ſoap and corroſive ſublimate was made, the fluid aſſumed the appearance of milk, but ſoon after exhibited ſmall coagula. It is almoſt impoſſible to filter this liquor, but the greateſt part of the mercurial combination is ſlowly depoſited. This depoſition may be accelerated by means of ſpirit of wine. The ſame combination may be much more readily effected with the nitrous ſolution of mercury *.

The oleo-mercurial combination is viſcid, dries difficultly, diſſolves very well in oil, and very ſparingly in ſpirit of wine. It loſes its white colour by expoſure to the air, and acquires a ſlate colour, which gradually becomes deeper, more eſpecially if it be expoſed to the ſun or any other heat. It readily becomes ſoft and fluid. It muſt be diſtinguiſhed from the mercurial ointment uſed in medicine, for in this the mercury poſſeſſes the metallic ſtate; whereas, in our combination, the mercury is in the ſtate of calx, and forms with the oil a true combination, which perhaps might prove uſeful in medicine.

The combination of oil and zinc, which I made by means of white copperas, is of a white colour inclining to yellow. It dries ſpeedily, and becomes friable. The combination of cobalt and oil, made by means of the ſolution of regulus of cobalt in aqua fortis, is of a dull leaden colour, and dries with difficulty, though its parts are not connected together. Towards the end of the precipitation ſome coagula of a green colour, and much more conſiſtent, fell down. I apprehend that this was a combination of oil and nickel; for it is known that this ſemi-metal is almoſt always contained in the regulus of cobalt, and that it forms green ſolutions with acids, while thoſe of cobalt are red. I could not eſtabliſh my conjecture, becauſe I was unable to procure nickel. But if it ſhould prove well founded, this proceſs will afford an eaſy method of ſeparating the two metallic ſubſtances.

I made the combination of oil and tin by means of the ſolution of this metal in aqua-regia. It is white, not fuſible when expoſed to heat, like all the other oleo-metallic combinations; but it is decompoſed without any change in the form of its parts. I attribute this circumſtance to the great quantity of metal contained in this combination, as we ſhall ſee.

The oleo-martial combination is of a reddiſh brown colour, tenacious, and eaſily fuſible. When ſpread upon wood it ſoaks in and dries. It is eaſily ſoluble in oil, more particularly oil of turpentine, to which it gives a good colour, which may prove uſeful as a varniſh.

The oleo-cupreous combination which I have made by means of blue vitriol, is reſinous to the touch while moiſt, is of a green colour, and becomes dry and brittle. When digeſted in ſpirit of wine its colour becomes deeper, and it liquefies, but does not diſſolve in the cold. Ether renders its colour deeper and more beautiful, inſtantly liquefies it, and diſſolves a conſiderable quantity. This combination is abundantly ſoluble in oils, to which it communicates a pleaſant green colour.

The combination of oil and lead, made by means of the ſolution of ſugar of lead, is white, tenacious, and very adheſive when heated. The union of oil and the lead is not ſo intimate in the diachylon (or direct ſolution of litharge in oil) as in this combination; for this laſt, when fuſed, is tranſparent, and becomes rather yellowiſh if the heat be ſomewhat increaſed; but the diachylon is opake, and the oil which enters into its compoſition

---

* Probably with different effects, accordingly as the ſolution, ſuppoſed to be ſaturated, is made with or without heat. N.

has acquired an acrid property from the heat it has been ſubjected to. It is probable, therefore, that the combination I have formed might in ſome caſes be advantageouſly ſubſtituted for that compound. Geoffroy formerly remarked, in the Memoirs of the Academy for 1741, that the combination of oil which he made in the manner of plaſters, formed a kind of ſoap.

The combination of oil and ſilver is white when firſt made; but after a few inſtants' expoſure to the air, it aſſumes a red tinge, which no doubt depends on the facility with which this metal yields its oxigene to all combuſtible matters *. The change of colour in the oleo-mercurial combination appears to me to depend on the ſame principle. When the combination of ſilver is fuſed, its ſurface becomes covered with a very brilliant iris; and beneath the ſuperficies it is black.

The combination of gold and oil partly floats on the mixture in the form of a cream, which is at firſt white, ſoon after which it aſſumes a dirty purple colour. It dries with difficulty, and adheres to the ſkin, ſo that it is difficult to efface the impreſſion.

I have combined the metallic principle of mangaueſe by mixing a ſolution of ſoap with a ſolution of manganeſe in the marine acid. This combination is at firſt white. It aſſumes in the air a reddiſh colour of peach bloſſoms, which becomes more and more deep. It ſpeedily dries to a hard brittle ſubſtance, and by liquefaction it aſſumes a brown blackiſh colour.

In order to aſcertain whether eſſential oil had likewiſe the property of combining with metallic ſubſtances, I mixed a ſolution of Starkey's ſoap newly made with a ſolution of vitriol of copper. The ſame thing happened as with the common ſoap, excepting that the combination was rather a lighter green, and more friable. Black ſoap, which is ſaid to be made with whale-oil, afforded me, with the ſolution of vitriol of copper, a combination which, compared with that obtained by means of common ſoap, is rather of a deeper green, remains ſomewhat ſofter, and poſſeſſes a very diſagreeable ſmell.

I was deſirous of forming a ſoap with cauſtic alkali and rectified animal oil, with the intention of forming other combinations afterwards; but this oil formed no union with alkali.

We have ſeen that the calcareous earth (and it is the ſame with ponderous earth) has more affinity with oil than fixed alkali, and this has more than magneſia; but the combination of magneſia is not decompoſed by the cauſtic volatile alkali, ſo that magneſia follows the fixed alkali in the order of affinity. Afterwards comes the volatile alkali, which decompoſes all the metallic combinations with more or leſs facility. It totally diſſolves the combination of ſilver; but the mercurial combination is that which appeared to me moſt ſtrongly to reſiſt decompoſition. With regard to clay, its combination is decompoſed by the cauſtic volatile alkali, even more readily than the metallic combinations. Whence I infer that it may be placed after the metallic ſubſtances.

Oils by expreſſion did not appear to diſſolve the calcareous and argillaceous combinations; oil of turpentine diſſolved only a ſmall portion of the calcareous combination, but rather more of that of clay, with which it formed a jelly. Ardent ſpirit diſſolves ſome of the oleo-metallic combinations without heat. It requires heat to diſſolve ſome others;

---

* The learned author will certainly approve my tranſlating the only theoretical paſſage in his memoir according to the ſimple doctrine he now maintains. In the original the effect is aſcribed to the attraction of ſilver to phlogiſton. N.

but though by this means it attacks them all, it nevertheleſs diſſolves much leſs than the oils, particularly the oil of turpentine.

I calcined part of the combinations here deſcribed to determine the quantity of earth or metallic calx they contained. I employed half an ounce of each. The reſidue of magneſia amounted to 32 grains, which did not efferveſce with an acid; that of calcareous earth was 36 grains, which efferveſced; that of clay 28 grains; of iron 48 grains; of copper 33; of zinc 42; of manganeſe 40; the reſidue of ſilver amounted to 30 grains in the metallic ſtate; that of tin 1 gros 7 grains of the reduced metal; that of lead formed by calcination a pyrophorus. When theſe combinations are to be made, it is proper to uſe the ſolutions in a ſaturated ſtate; for, if there be an exceſs of acid, part of the ſoap is decompoſed by this exceſs; a portion of the oil floats above; but part of the oil enters the combination which is formed, and alters its properties. In whatever acid an earth or a metal may be diſſolved, the ſame combination is always formed by means of ſoap; nevertheleſs this combination ſometimes exhibits different appearances. Thus the oily compound of mercury is much more tenacious and adheſive when corroſive ſublimate is uſed, than when the nitrous ſolution of the metal has been employed [*].

When the filtered liquor, after forming the oily combination, is evaporated, a ſalt is obtained of the particular kind which reſults from the alkali of the ſoap, and the acid of the ſolution made uſe of. M. Coſtel made the experiment with ſelenite, and I repeated it with Epſom ſalt and vitriol. I firſt evaporated to perfect dryneſs, after which I diſſolved the reſidue in filtered water, and then evaporated and cryſtallized [†].

X.

Analyſis of a Memoir of Citizen BONHOMME *on the Nature and Treatment of Rachitis, or the Rickets* [‡].

SINCE the object of the Society of Medicine, in the offer of its prizes, is to bring our knowledge of the healing art to the greateſt degree of preciſion and accuracy of which it is ſuſceptible, this company muſt neceſſarily direct its attention to the progreſs of the chemical analyſis of animal matters, and the information which ſooner or later it muſt afford reſpecting the nature and treatment of many diſorders.

Thoſe diſorders which alter the compoſition of the fluids and the conſiſtence of the ſolids, are aſſuredly the firſt which ought to be determined by chemical reſearch. The rachitis is of this number. The change which the bones undergo in this diſorder, has long been attributed to the action of an acid on their ſubſtance; but this opinion was grounded on mere ſuppoſition and remote analogy. The ſubject is treated in a new manner in the memoir, of which the Society has directed us to preſent the reſults; and the experiments as well as the obſervations upon which the author grounds his inferences, preſent to view matters of fact ſufficiently remarkable to afford a preſumption that new experiments will confirm their truth. We announce them on the preſent occaſion with expreſſions of doubt, becauſe theſe are the expreſſions which the wiſdom and modeſty of the author have ſuggeſted, and becauſe we think with him, that facts of this nature cannot be ſtated as if fully proved, but in conſequence of multiplied experiments.

---

[*] Subſequent experiments have ſhewn that a much greater quantity of oxigene is preſent in the former than in the latter caſe. N.

[†] I ſuppoſe M. Berthollet's experiments were made with ſoap of vegetable expreſſed oil. The common ſoaps of this country contain animal fat. N.

[‡] This Memoir was read to the National Society of Medicine at Paris. The analyſis was made by Hallé, and is inſerted in the ſeventeenth volume of the Annales de Chimie, whence this tranſlation is made.

The principal notions which conſtitute the baſis of this memoir are the following:

1. According to the author, the nature of the rachitic diſorder ariſes on the one hand from the development of an acid approaching in its properties to the vegetable acids, particularly the oxalic, and on the other from the defect of phoſphoric acid, of which the combination with the animal calcareous earth forms the natural baſis of the bones, and gives them their ſolidity.

Whence it follows, that the indication reſulting from this propoſition, if once adopted, would be, that the treatment of rachitis muſt depend on two principal points, namely, to prevent the development of the oxalic acid, and to re-eſtabliſh the combination of the phoſphoric acid with the baſis of the bones to which they owe their ſolidity.

2. The author proves by experiments and obſervations, in the firſt place, that alkaline lotions of the parts affected with rachitis contribute to their cure; next, that the calcareous phoſphate taken internally is really tranſmitted by the lymphatic paſſages, and contributes to oſſification; and laſtly, that the internal uſe of calcareous phoſphate, whether alone or combined with the phoſphate of ſoda, powerfully contributes to reſtore the natural proportions in the ſubſtance of the bones, and accelerate the cure of rachitis.

On the preſent occaſion we ſhall only collect the proof of theſe fundamental facts, which form the abſolutely new part of this memoir, in which the author has beſides inſerted an excellent abridgement of all that had been aſcertained before him on the nature of the bones, the rachitis, and the treatment of this diſorder.

With regard to the firſt parts, the author endeavours to eſtabliſh theſe two propoſitions: 1. That the calcareous phoſphate is wanting in the bones of thoſe who are diſordered with rachitis. 2. That the development of oxalic acid is the cauſe of this alteration.

We muſt not conceal, that this ingenious part of his memoir contains rather views than abſolute proofs of the nature of the rachitic acid. The author himſelf declares, that he was not provided with the neceſſary means to eſtabliſh an exact and complete analyſis. He therefore preſents his ideas in this reſpect, merely as conjectures approaching to the truth.

The effect of the action of acids upon bones, was before known; that is to ſay, that when deprived of calcareous phoſphate, and reduced to the gelatinous parenchyma which forms one of their elements, they loſe their conſiſtence and become flexible. Hence it was already conjectured by various phyſicians, that the rachitis was the effect of a peculiar acid.

A diſpoſition to aceſcence in the firſt paſſages is obſervable in all infants. The odour which characterizes this aceſcence is often manifeſt in their breath, and even their perſpiration. The bile corrects this diſpoſition; but in general the bile is wanting in rachitic infants. It does not colour their excrements, and the acids accordingly are developed in a very decided manner. They diſturb the circulation, and attack and ſoften the bones. As it is by defect of animalization that theſe acids develop themſelves, it follows that their character is analogous to the fermenteſcible vegetable acids, and more or leſs to the oxalic acid; and that, on the contrary, the animal acid or phoſphoric acid ceaſes to be formed, and to unite with the animal calcareous earth; whence they are deprived of the principle of their ſolidity. This is the theory of Citizen Bonhomme.

In order to eſtabliſh this doctrine upon preciſe experiments, it was requiſite to analyſe rachitic bones comparatively with thoſe of healthy individuals of the ſame age; and as it is known that the urine of rachitic ſubjects depoſits a great quantity of a ſubſtance of ſparing ſolubility and earthy appearance, it would have been advantageous to have joined a complete analyſis of this urine and its ſediment.

Citizen Bonhomme not being provided with the means ſufficient to make theſe analyſes, and being beſides of opinion that ſuch rachitic bones as are deſtroyed by this malady exiſt in a progreſſive ſtate of change, which might render their analyſis ſcarcely ſuſceptible of compariſon, limited himſelf to a collection of ſome of the moſt remarkable phenomena of the urine of the aged, the adult, and infants in the healthy ſtate, of infants in the rachitic ſtate, and of patients after the perfect cure of this diſorder. From theſe obſervations he has deduced ſeveral important reſults.

It is known that when the urine contains diſengaged phoſphoric acid, as happens to aged individuals, and in ſome peculiar circumſtances of the ſyſtem, if lime water be poured in there is a ſpeedy depoſition of calcareous phoſphate. It is alſo known, that when a ſolution of the nitrate of mercury is poured to the freſh urine of adults, a roſe-coloured precipitate is formed, which is a phoſphate of mercury produced by the decompoſition of the phoſphates contained in the urine. Theſe two proofs are therefore extremely proper to aſcertain the preſence of phoſphoric acid, whether free or combined, in a fluid which in its natural ſtate contains a remarkable proportion. Beſides this principle, the urine depoſits more or leſs of ſediment, either gelatinous or of an earthy appearance; and laſtly, by evaporation, a ſaponaceous and ſaline extract, in greater or leſs abundance, is obtained by evaporation. By means of theſe four methods of examination, the author has aſcertained the following facts:

1. In the healthy ſtate, the ſediment naturally depoſited by urine is almoſt totally gelatinous in the infant and the adult, and in the aged individual it is ſurcharged with an abundant ſediment of an earthy appearance ſimilar to the earth of bones, which conſequently is calcareous phoſphate.

2. The quantity of brown ſaponaceous ſaline extract afforded by evaporation is greater in proportion to the age.

3. The preſence of diſengaged phoſphoric acid, as ſhewn by lime water, is none in the urine of infants, ſcarcely perceptible in that of adults, but very remarkable in that of old men. For two ounces of this laſt urine afforded by this means ten grains of phoſphate of lime.

4. The decompoſition of the phoſphates by nitrate of mercury is not ſeen in the urine of infants; an abundant precipitate of a light roſe-colour is produced in this way from the urine of adults; and in that of old men this precipitate is always of a grey colour, and very abundant.

Hence Citizen Bonhomme concludes, that the phoſphoric acid, whether at liberty or combined, does exiſt in the urine of healthy individuals in proportion to the deſtruction of the ſolids by age, and that it increaſes with the age.

With regard to the urine of rachitic ſubjects, the moſt remarkable facts are, 1. The abundant and apparently earthy ſediment it depoſits (ſpontaneouſly) is different from that of old men, by its colour, which is grey and does not reſemble phoſphate of lime, and alſo by its much greater quantity. For a pound of this urine let fall two gros, whereas the ſame quantity of the urine of old men depoſited only 45 grains.

2. The extract left by evaporation is likewiſe much more conſiderable than in other urine. It is one third more in quantity than the extract afforded even by the urine of aged perſons.

From theſe two firſt obſervations it follows, that the ſolids in rachitic ſubjects are deſtroyed with much more rapidity than even in old men; and that they afford a much more abundant portion of waſte to the urine.

3. The light depoſition occaſioned by lime water in the urine of rachitic ſubjects is very ſmall in quantity, brown, gelatinous when freſh, and pulverulent when dry. It does not at all reſemble calcareous phoſphate.

4. The depoſition formed by the ſolution of mercurial nitrate is not abundant, neither of a roſe colour as in the urine of adults, nor grey like that of old men. It is always white, and conſequently has no external reſemblance to the phoſphate of mercury. The author affirms that it reſembles a mercurial oxalate.

Laſtly, the urine of the ſame rachitic ſubjects when cured, exhibits again all the characters obſerved in the urine of healthy children.

We ſhall not add to the reflections of the author. In effect, though theſe firſt obſervations are curious, they are incomplete. We offer them to phyſicians ſimply as the elements of an inveſtigation which it is of importance to continue and bring to perfection. We ſhall therefore proceed to the curative and experimental parts of the memoir.

We muſt remark, however, in this place, that the author preſents in his work a judicious and methodical expoſition of the whole treatment of the rachitis by other phyſicians; that he eſtimates the value of each remedy, and every part of the treatment, from the circumſtances to which they are applicable, the degrees of the malady in which they are to be admitted, and the indications they anſwer. Setting aſide, therefore, every thing which is not peculiarly his own, we ſhall in this place attend only to that which relates to the uſe and effects of calcareous phoſphate and alkaline lotions.

One of the facts which it was of the utmoſt importance to eſtabliſh, was the tranſition of the calcareous phoſphate from the inteſtinal paſſages, into thoſe of circulation and ſecretion. Fourcroy had already well aſcertained that the ſerum of milk contains this ſalt naturally. Vauquelin had proved its exiſtence, as well as that of pure ſoda, in the ſeminal fluid; but was it poſſible that it could paſs unaltered from the ſtomach and inteſtines into the veſſels which contain the blood and lymph? Could it by this means apply itſelf to the bones? This was to be aſcertained by experiments.

[*To be continued.*]

XI.

On the Nature of the Diamond. By SMITHSON TENNANT, *Eſq. F. R. S.* *

SIR Iſaac Newton having obſerved that inflammable bodies had a greater refraction in proportion to their denſity than other bodies, and that the diamond reſembled them in this property, was induced to conjecture that the diamond itſelf was of an inflammable nature. The inflammable ſubſtances which he employed were camphor, oil of turpentine, oil of olives, and amber; theſe he called "fat, ſulphureous, unctuous bodies;" and uſing the ſame expreſſion reſpecting the diamond, he ſays, "it is probably an unctuous body coagulated." This remarkable conjecture of Sir Iſaac Newton has been ſince confirmed by repeated experiments. It was found, that though the diamond was capable of reſiſting the effects of a violent heat when the air was carefully excluded, yet, that on being expoſed to the action of heat and air it might be entirely conſumed. But as the ſole object of theſe experiments was to aſcertain the inflammable nature of the diamond, no attention was paid to the products afforded by its combuſtion; and it ſtill therefore remained to be determined, whether the diamond was a diſtinct ſubſtance, or one of the known inflammable bodies. Nor was any attempt made to decide this queſtion till M. Lavoiſier, in 1772, undertook a ſeries of experiments for this purpoſe. He expoſed the diamond to the heat produced by a large lens, and was thus enabled to burn it in cloſe glaſs veſſels. He obſerved that the air in which the inflammation had taken place had become partly ſoluble in water, and precipitated from lime-water a white powder, which appeared to be chalk, being ſoluble in acids with efferveſcence. As M. Lavoiſier ſeems to have had little doubt that this precipitation was occaſioned by the production of fixed air, ſimilar to that which is afforded by calcareous ſubſtances, he might, as we know at preſent, have inferred that the diamond contained charcoal; but the relation between that ſubſtance and fixed air was then too imperfectly underſtood to juſtify this concluſion. Though he obſerved the reſemblance of charcoal to the diamond, yet he thought that nothing more could be reaſonably deduced from their analogy, than that each of thoſe ſubſtances belonged to the claſs of inflammable bodies.

---

* Philoſophical Tranſactions, 1797, p. 123.

As the nature of the diamond is ſo extremely ſingular, it ſeemed deſerving of further examination; and it will appear from the following experiments that it conſiſts entirely of charcoal, differing from the uſual ſtate of that ſubſtance only by its cryſtallized form. From the extreme hardneſs of the diamond, a ſtronger degree of heat is required to inflame it, when expoſed merely to air, than can eaſily be applied in cloſe veſſels, except by means of a ſtrong burning lens; but with nitre its combination may be effected in a moderate heat. To expoſe it to the action of heated nitre free from extraneous matters, I procured a tube of gold, which, by having one end cloſed, might ſerve the purpoſe of a retort; a glaſs tube being adapted to the open end for collecting the air produced. To be certain that the gold veſſel was perfectly cloſed, and that it did not contain any unperceived impurities which could occaſion the production of fixed air, ſome nitre was heated in it till it had become alkaline, and afterwards diſſolved out by water; but the ſolution was perfectly free from fixed air, as it did not affect the tranſparency of lime-water. When the diamond was deſtroyed in the gold veſſel by nitre, the ſubſtance which remained precipitated lime from lime-water, and with acids afforded nitrous and fixed air; and it appeared ſolely to conſiſt of nitre partly decompoſed, and of aërated alkali.

In order to eſtimate the quantity of fixed air which might be obtained from a given weight of diamonds, two grains and a half of ſmall diamonds were weighed with great accuracy, and, being put into the tube with a quarter of an ounce of nitre, were kept in a ſtrong red heat for about an hour and a half. The heat being gradually increaſed, the nitre was in ſome degree rendered alkaline before the diamond began to be inflamed; by which means almoſt all the fixed air was retained by the alkali of the nitre. The air which came over was produced by the decompoſition of the nitre, and contained ſo little fixed air as to occaſion only a very ſlight precipitation from lime-water. After the tube had grown cold, the alkaline matter contained in it was diſſolved in water, and the whole of the diamonds were found to have been deſtroyed. As an acid would diſengage nitrous air from this ſolution as well as the fixed air, the quantity of the latter could not in that manner be accurately determined. To obviate this inconvenience, the fixed air was made to unite with calcareous earth by pouring into the alkaline ſolution a ſufficient quantity of a ſaturated ſolution of marble in marine acid. The veſſel which contained them, being cloſed, was left undiſturbed till the precipitate had fallen to the bottom; the ſolution having been previouſly heated, that it might ſubſide more perfectly. The clear liquor, being found by means of lime-water to be quite free from fixed air, was carefully poured off from the calcareous precipitate [*]. The veſſel which was uſed on this occaſion was a glaſs globe, having a tube annexed to it, that the quantity of the fixed air might be more accurately meaſured. After as much quickſilver had been poured into the glaſs globe containing the calcareous precipitate as was neceſſary to fill it, it was inverted in a veſſel of the ſame fluid. Some marine acid being then made to paſs up into it, the fixed air was expelled from the calcareous earth, and in this experiment, in which two grains and a half of diamonds had been employed, occupied the ſpace of a little more than 10.1 ounces of water.

The temperature of the room when the air was meaſured was at 55°, and the barometer ſtood at about 29.8 inches.

From another experiment made in a ſimilar manner with one grain and a half of diamonds, the air which was obtained occupied the ſpace of 6.18 ounces of water; according to which proportion, the bulk of the fixed air from two grains and a half would have been equal to 10.3 ounces.

The quantity of fixed air which was thus produced by the diamond does not differ much from that which, according to M. Lavoiſier, might be obtained from an equal weight of charcoal. In the Memoirs of the French Academy of Sciences for the year 1781, he has related the various experiments which he made to aſcertain the proportion of charcoal and oxigene in fixed air. From thoſe which he conſidered as moſt accurate, he concluded that 100 parts of fixed air contain nearly 28 parts of charcoal and 72 of oxigene. He eſtimates the weight of a cubic inch of fixed air, under the preſſure and in the temperature abovementioned, to be .695 parts of a grain. If we reduce the French weights and meaſures to Engliſh, and then compute how much fixed air, according to this proportion, two grains and a half of charcoal would produce, we ſhall find that it ought to occupy very nearly the bulk of 10 ounces of water.

M. Lavoiſier ſeems to have thought that the aërial fluid produced by the combuſtion of the diamond was not ſo ſoluble in water as that produced from calcareous ſubſtances. From its reſemblance, however, in various properties, hardly any doubt could remain that it conſiſted of the ſame ingredients; and I found, upon combining it with lime, and expoſing it to heat with phoſphorus, that it afforded charcoal in the ſame manner as any other calcareous ſubſtance.

---

[*] If much water had remained, a conſiderable portion of the fixed air would have been abſorbed by it. But by the ſame method as that deſcribed above, I obſerved that as much fixed air might be obtained from a ſolution of mineral alkali, as by adding an acid to an equal quantity of the ſame kind of alkali. T.

XII.

Uſeful Notices reſpecting various Objects.—Improvement of Teleſcopes.—Imperfections of Optical Glaſs.—Purification of Mercury.

THE eye has often been deſcribed with admiration by opticians as a perfect inſtrument, as well as by others in a more looſe way, in illuſtration of the argument from final cauſes. When the correction of the aberrations from ſphericity and colour began to be inveſtigated, it was preſumed that the arrangement and figure of its parts were directed to remedy theſe defects; a notion which has ſince been ſufficiently refuted by Drs. Maſkelyne * and Blair †. If the general remarks had ever been extended to the improvement of optical inſtruments, inſtead of having been applied to them ſubſequent to the periods of actual diſcovery; it might afford matter for ſurpriſe, that the moſt variable of all its adjuſtments, namely, that of aperture, has never yet been introduced into our artificial combinations. When we look towards a window or any luminous object, the iris immediately contracts; and on the contrary, when we direct our ſight to the inner part of a room, or to any obſcure place, the aperture for the reception of light is as ſpeedily enlarged. To what extent this enlargement may be carried in ſituations of extreme darkneſs, cannot be obſerved; but it is certain, that in the obſervable variations of the human eye, the aperture is thirty times as large at one time as at another; and that in the cat the proportion is much greater than an hundred to one. Hence we might reaſonably infer, on general principles, that the diſtinctneſs of viſion through teleſcopes would be improved, if this adjuſtment alſo were added to that of mere focal diſtance.

Every attentive obſerver muſt have taken notice, that light is of as much conſequence to artificial viſion as magnifying power. Diſtant woods and other land objects are inviſible to an high magnifying power for want of light, when the ſame objects may be diſtinctly ſeen with a lower. Luminous objects are ſeen very perfectly by a ſmall aperture, though coloured by a greater: but objects leſs enlightened will receive all the advantage of ſuperior diſtinctneſs from greater light, when the fringes of colour may be too faint to be of any conſequence. From theſe circumſtances it frequently happens, that an achromatic perſpective, admitting plenty of light, may exhibit land objects, eſpecially in the evening, to much greater advantage than a reflector, admitting leſs light but performing its office with more accuracy; whereas, on the other hand, the latter inſtrument ſhall have greatly the advantage when tried upon a planet.

An ingenious mechanic will find little difficulty in contriving an artificial iris. Suppoſe a braſs ring to ſurround the object end of the teleſcope, and upon this let eight or more triangular ſlips of braſs be fixed, ſo as to revolve on equi-diſtant pins paſſing through each triangle near one of its corners. If the triangles be ſlided inwards upon each other, it may readily be apprehended that they will cloſe the aperture; and if they be all made to revolve or ſlide backwards alike, it is clear that their edges will leave an octagonal aperture, greater or leſs according to circumſtances. The equable motion of all the triangles may be produced either by pinions and one concave toothed wheel, or by what is called ſnail-work. Another kind of iris more compact may be made by cauſing thin elaſtic ſlips of braſs to ſlide along parallel to the tube, and be conducted each through a ſlit in a braſs cap which ſhall lead them acroſs the aperture in a radial direction. It is probable alſo that the artiſt who ſhall carry theſe hints into effect, may alſo think of ſeveral other methods.

---

* Philoſ. Tranſ. LXXIX. Art. 21.

† Edinburgh Tranſ. II.

2. *Imperfections of Optical Glaſs.*

THE only ſure method of aſcertaining whether glaſs be fit for optical purpoſes, is to give it the true figure of a lens, and examine this by the teſt of a high magnifying power. It is deſirable however to make ſome ſelection by trial previous to this labour. A lens may be ſuppoſed to conſiſt for the moſt part of a ſubſtance of uniform refractive denſity, but more or leſs mixed with glaſs of another kind, which exhibits the appearance of ſpots or veins which interrupt the regular courſe of the light. Where this interruption is produced by conſiderable maſſes, the imperfections may be ſeen by the naked eye; but where the maſſes are ſmaller, the glaſs may appear very clear, and yet be leſs fit for optical purpoſes than ſuch as may contain ſeveral viſible imperfections. From theſe conſiderations it appeared probable to me, that glaſs might be advantageouſly examined by the magnifier applied near its ſurface.

An exceedingly clear, brilliant, yellow-tinged, plano-convex lens, of about four inches focus, was examined by looking at a candle through it, and a lens of half an inch focus next the eye. When the candle was about two feet diſtant, I obſerved a prodigious number of ſmall erect images of candles of various ſizes, which became ſpots at a greater diſtance of the large lens from the eye. Removing to the diſtance of ſeven or eight feet from the candle, the ſmall images became luminous ſpots, ſome of them ſurrounded by circles of alternate light and darkneſs. At this diſtance, the ſpots, by removing the large lens further from the eye, were rendered much more perceptible.

Several other lenſes were viewed, which exhibited a few images; but in a white common ſpectacle glaſs of twelve inches focus, the images appeared ſo numerous, that the lens reſembled the waved window-glaſs, and, at a diſtance, appeared prodigiouſly ſpotted. This lens had nothing of an opake appearance, but the light paſſed through it too irregularly to give a diſtinct picture in the camera obſcura.

A good greeniſh object-glaſs of ſix feet focus, which has a large vein or two acroſs it, produced no more effect in theſe circumſtances than if it had not been interpoſed between the eye and the candle.

From the circumſtances of theſe experiments, we may infer that the images are produced by ſpherical particles of leſs denſity than the reſt of the glaſs. We may ſuppoſe them to have been ſilex, the ſpecific gravity of which is about 2.7, which is nearly the ſame as that of green or crown glaſs, but is one-fifth leſs than that of flint-glaſs taken at 3.2.

When the full moon was looked at in this manner, through the firſt-mentioned lens, the images were conſiderably diſtinct, and all nearly of the ſame ſize. Their magnitude was about one two-hundredth part of an inch, or one-fifth part of the ſize of a bubble in the glaſs, which, by eſtimate, might have a diameter of one-fortieth of an inch.

3. *Purification of Mercury.*

MERCURY which is conſiderably adulterated may be moſt conveniently and effectually purified by diſtillation. But many perſons, ſuch as mathematical inſtrument-makers, water-gilders, and others, who have occaſion to uſe pure mercury, may reject ſuch as contains very little adulteration for want of the chemical apparatus to diſtill it. The proceſs by agitation, firſt diſcovered by Dr. Prieſtley, is far from being generally known or uſed; but will, no doubt, prove an uſeful acquiſition to many operators.

This proceſs is grounded on the fact, that the metals with which mercury is uſually contaminated, become converted into a black powder by partial calcination when agitated with reſpirable air. Take a glaſs vial with a ground ſtopper * (ſuch being generally pretty ſtrong), capable of containing 10 or 12 ounces of water, and fill about one-fourth of it with the foul quickſilver; then putting on the ſtopper, let the bottle be held inverted with both hands, ſhaking it violently by ſtriking the hand that ſupports it againſt the knee. After twenty or thirty ſtrokes, take out the ſtopper, and blow into the vial with a pair of bellows to change the air. For the purer the air the faſter the proceſs advances. After a ſhort time, if the mercury be very foul, the ſurface will not only become black, but a great quantity of the upper part will appear as if coagulated. In this ſituation the vial is to be inverted; and covering the mouth of it with the finger, all the mercury that will flow eaſily is to be let out, and the black coagulated part put into a cup by itſelf. The running mercury may be ſeparated from the black powder, by preſſing this maſs repeatedly with the finger. This mercury is to be added to the reſt, in order to be agitated again.

The proceſs muſt be repeated till no more black matter can be ſeparated; and it is not a little remarkable, that the operator will be at no loſs to know when the operation is completed. For the ſame quantity of lead ſeems to come out of it in equal times of agitation, and conſequently the whole becomes pure at once. The ſound of the pure mercury is louder and more harſh than while it contained the adulteration; ſo that by this criterion the end of the proceſs is eaſily diſtinguiſhed.

Dr. Prieſtley purpoſely diſſolved both lead and tin in a large quantity of mercury, which he afterwards purified in this way, and found by ſubſequent diſtillation, that it had been rendered quite pure. I have myſelf uſed this proceſs, which I find eaſy and convenient. The laſt portions of black matter, which give the mercury a duſty look, are readily ſeparated by the paper funnel as uſual.

XIII.

A Memoir containing ſome Reſults ariſing from the Action of Cold on the Volatile Oils, and an Examination of the Concretions found in ſeveral of thoſe Oils †. *By* Cit. MARGUERON, *Member of the Societé des Pharmaciens at Paris.*

THE volatile or eſſential oils obtained from vegetables owe their fluidity, in the ſame manner as water, to the portion of caloric they contain. Several of them loſe this ſtate at 8° above ‡ the term of aqueous congelation (50° Fahrenheit), and aſſume the concrete form;

---

* Experiments and Obſervations on Air, abridged and methodized, by Joſeph Prieſtley, LL. D. F. R. S. III. 439.

† Annales de Chimie, XXI. 174.

‡ The temperatures in this paper are according to the ſcale called Reaumur's, in which the freezing point is marked 0°, and the boiling water point 80°. N.

others, on the contrary, preſerve their fluidity at a much lower term. It is upon theſe laſt chiefly that I have directed the operation of cold, with two objects of reſearch: the firſt, to obſerve the cryſtallization which theſe oils might exhibit by the loſs of their caloric; the ſecond, to aſcertain the other phenomena which might ariſe during the experiments.

Theſe experiments, though apparently ſimple, are not however without their difficulties.

When we reflect on the different characters of volatile oils, we find in general, that the oils obtained from the plants of hot climates are heavier and leſs limpid than thoſe of colder regions, and that they contain a ſalt capable of cryſtallization; that the oils extracted from the ſame plants of our climates vary in colour and fluidity, according to the nature of the ſoil in which the plants have vegetated, the care in cultivation, the influence of light, the different ſtates of growth, and whether they be recent or dry when ſubjected to diſtillation: that when extracted from the vegetable, they are expoſed by the nature of their principles to different alterations, according to the more or leſs effectual action of the air, the contact of light, whether kept in bottles entirely or only in part filled, or cloſed with a ground ſtopper or with a cork. Theſe differences will eaſily ſhew that the reſults muſt be ſubject to variation: and I have accordingly been very careful to uſe only ſuch volatile oils as were newly extracted from freſh vegetables; with the exception of thoſe from exotic plants.

Concerning the Action of Cold on ſeveral Volatile Oils.

I HALF filled ſeveral very thin bottles with the volatile oils of peppermint, orange flowers, lemon-peel, bergamot, buds of lavender and of thyme, being plants of our climates; oil of turpentine, a reſinous ſubſtance, and oil of cinnamon, a foreign bark.

Theſe bottles, cloſed with ground ſtoppers, were expoſed for ſeveral days on a terrace where a mercurial thermometer marked 11° beneath the freezing point (7° Fahrenheit).

At this degree of cold I obſerved that there was formed at the upper internal part of the bottles a cryſtallization in different ramifications, ſimilar to thoſe ſeen on the windows of apartments in very cold weather.

Oil of bergamot exhibited a number of ſmall elliptical laminæ in its ſubſtance; that of lemons had depoſited ſmall cryſtals; oil of orange-flowers exhibited a diminiſhed fluidity; that of cinnamon was partly congealed on the augmentation of the cold, when the thermometer in my laboratory ſtood at 15° below freezing (— 1° Fahrenheit). I took advantage of this time to expoſe the ſame oils to the action of an artificial cold, which I procured by a mixture of ammoniacal muriate and ice, which cauſed the thermometer to ſink to 22 degrees (— 17° Fahrenheit). I kept up the ſame degree of cold for two hours, by alternately adding the ammoniacal muriate and ice. During the action of this cold upon the oils, the ſtoppers were raiſed by the expanſion and diſengagement of a gaſeous ſubſtance, which perfumed the laboratory and its environs more ſtrongly than when theſe oils were diſtilled in ſummer. The upper part of the bottles was lined with needles, of which the ramifications formed dendrites. When the ſurrounding ice began to change its temperature, I proceeded to examine the oils as ſpeedily as poſſible, and remarked:

1. The interior ſurface of the bottle which contained the oil of peppermint was covered with ſmall needles which formed a capillary vegetation. They were white, and liquefied on the fingers. When applied to the tongue, they afforded the freſh and pungent ſmell of the

oil. Their ſolution in alcohol became white by the addition of water. The oil poſſeſſed an imperfect fluidity; its ſmell was leſs intenſe, and its colour deeper. It was ſoluble in alcohol, and had loſt by congelation one * part of its weight †.

2. The bottle containing volatile oil of orange flowers exhibited different ramifications in its ſuperior internal cavity. Upon drawing the ſtopper a diſengagement of air took place, together with the other phenomena obſerved in the oil of peppermint, with this difference, that the maſs of the oil was more coloured; that it had loſt its fluidity, ſo as to adhere to the bottle like turpentine; and when poured into water ſome particles ſeparated, which remained conſtantly at the bottom of the fluid.

3. The artificial cold produced no other effect on the oil of bergamot than the appearance of ſome cryſtalline laminæ of an elliptical form. This oil reſumed its fluidity at four degrees below 0 (or 23 Fahrenheit). Its properties were not perceptibly changed.

4. The volatile oil of lemon-peel, when taken out of the bath of ice and ſalt, appeared to have loſt its fluidity. At the expiration of ſome days, I perceived that an amber-coloured liquor had ſeparated from this oil, with ſeveral ſmall cryſtals. I decanted theſe ſeveral products from each other, and obtained the following reſults:

A. The oil from which the amber-coloured liquor had been ſeparated poſſeſſed the colour and tranſparency of the common oil, but its ſmell and taſte were leſs pungent. Its ſolution in alcohol was ſimilar to that which had not been ſubjected to cold.

B. The amber-coloured liquor afforded by the volatile oil of lemon-peel by the action of cold, poſſeſſed an empyreumatic ſmell. Its taſte was bitter and ſlightly acid. It mixed with water, reddened the tincture of tournſole, did not precipitate lime-water, and cauſed an efferveſcence with the carbonate of pot-aſh. This liquor was afforded from the oil of lemons in the proportion of one-tenth part of the whole.

C. The concretions or ſmall cryſtals obtained from the volatile oil of lemons did not poſſeſs a well determined form. They were white, ſcarcely conſiſtent, and became opake and friable by the contact of the atmoſpheric air. They emitted an odour of oil of lemons, were inſoluble in cold water, liquefied in boiling water, and formed a pellicle at its ſurface while cooling. When heated in a capſule, they melted, and cryſtallized in needles by cooling. They did not take fire by the flame of a candle. Their ſolution in alcohol reddened the tincture of tournſole.

5. The bottle containing the volatile oil of turpentine had its ſuperior internal ſurface covered with a ſlight efflorescence, produced by a portion of the oil which had riſen by evaporation. Concretions were formed in the maſs of the oil adherent to the ſides of the bottle in the form of flattened tears. Theſe were white, opake, and more firm than turpentine.

---

* This fraction in the original is rendered imperfect by the type for denominator having fallen out. N.

† Citizen Pelletier has ſome oil of peppermint which cryſtallizes in long needles at the temperature of ſix degrees below congelation (or 18 Fahrenheit), and almoſt totally at a few degrees lower.

In a package of the volatile oil of peppermint, which he received from London, he found a pint bottle broken, and the oil ſpilled among the hay made uſe of for the package. He reſerved this hay for diſtillation, in order to recover the oil. At the end of eight days, perceiving that the hay began to ferment, he immediately ſubjected it to diſtillation, and obtained a very fine oil poſſeſſing the property of cryſtallization. Citizen Pelletier is of opinion that it acquired this property by combining with the elaſtic fluid produced in the incipient fermentation of the hay. M.

tine. They reſumed the fluid ſtate at ſeven degrees below congelation (or 16° Fahrenheit). This fluid, being poured into water, ſunk to the bottom.

The oil from which the concretions were obtained did not appear to be changed.

6. The volatile oil of lavender, newly diſtilled, roſe ſlightly in vapour, and formed a congelation in the upper part of the bottle. The portion of oil that remained had loſt its fluidity, and acquired a deeper colour.

7. The oil of thyme, recently obtained, afforded the ſame reſults.

8. Volatile oil of cinnamon, ſubjected to the action of artificial cold, became very thick, and exhibited an irregular cryſtallization; and, as Citizen Baumé has obſerved, this oil reſumed its fluid ſtate at four degrees below 0 (or 23° Fahrenheit) without alteration.

Concerning the Action of Cold and of Water, during its Tranſition to the Glacial State, upon the Volatile Oils.

WATER, in its paſſage to the ſtate of ice, loſes part of its caloric, increaſes its volume, and exerts a preſſure on ſurrounding bodies.

Being deſirous of knowing what would be the reſult of theſe phenomena on the volatile oils, I made the following experiment:

I poured diſtilled water into ſeveral bottles, and added volatile oils which had not been ſubjected to any experiment. I placed theſe bottles in capſules of glaſs, and expoſed them at a window where the thermometer marked 11 degrees below freezing (or 7 of Fahrenheit). At the moment when the water aſſumed the ſtate of ice, ſeveral of the ſtoppers were forced up by the diſengagement of an aromatic principle. The frozen water ſpeedily afterwards increaſing its volume, cauſed ſeveral of the oils to flow over the external ſurface of the bottles.

The bottle containing volatile oil of peppermint was covered with a capillary vegetation, as well as the capſule. The other oils exhibited nothing particular.

Prouſt had occaſion to obſerve a ſimilar phenomenon on pouring out the oil of lavender which had been evaporated by cold: part of the volatile oil which had been thus expoſed to cold having eſcaped out of the bottle, he ſaw a few inſtants afterwards a kind of ſnow on the whole ſurface of the bottle which had been covered with the oil.

The action of cold and of water on the oil of peppermint had augmented the intenſity of its colour, and weakened its ſmell. This oil had preſerved its ſolubility in alcohol. The needles which had ſeparated from the volatile oil of peppermint during its eſcape, were white, ſilky, and brittle; they had the ſmell of mint. When applied to the tongue, they left the cool and pungent taſte of the peppermint drops or paſtils. They ſuffered no alteration by the contact of the air; were fuſed without inflammation by the flame of a candle, and became conſiſtent and tranſparent by cooling. When theſe needles were triturated in diſtilled water, they communicated to the fluid the property of reddening the tincture of tournſole; their ſolution in alcohol was attended with a ſlight noiſe and agitation (fremiſſement), and did not become white by the addition of water, as uſually happens with ſuch ſolutions. From theſe various experiments it follows, that a temperature of 11 degrees below 0 of the thermometer of Reaumur (or 7° of Fahrenheit) produces no change in ſeveral of the volatile oils, but thickens thoſe of cinnamon and bergamot, and determines a cryſtallization more or leſs regular; that the intenſe cold obtained by a mixture of muriate of ammoniac and ice diſengages from the volatile oils part of their aroma; renders their colour more intenſe, thickens them, produces concretions in the oils of turpentine and lemon, and ſeparates from the latter an amber-coloured fluid, which appears to poſſeſs ſaline properties; and laſtly, that the action of the cold from water, with the concurrence of the air on the volatile oil of peppermint, afforded a cryſtalline matter diſpoſed in ſilky needles, which various experiments prove to be a peculiar ſalt; whence it may be concluded, that the needles of camphor, which are ſaid to have been found in the diſtilled water of peppermint, are of the nature of this ſalt.

[*To be concluded in the next Number.*]

MATHEMATICAL CORRESPONDENCE.

QUESTION III. *Anſwered by J. F...... R.*

LET A, B, C and D, be the players in the order of ſeniority. Then, the laſt card being a trump, there will, when D begins to deal, be 12 trumps out of 51 cards, to be diſtributed among them. The chance that A's firſt card is not a trump, will, therefore, be $\frac{39}{51}$. This having happened, the chance that the next card will not be a trump, will become $\frac{38}{50}$, and the next $\frac{37}{49}$. Then A's chance that his firſt card will be one, will, of courſe, be $\frac{12}{48}$ or $\frac{1}{4}$; which having happened, A's chance of failure becomes $\frac{36}{47}$, and ſo on. By proceeding in this way, we find the whole value of D's chance for all the trumps =

$$\frac{39.\,38.\,37.\,36.\,35.\,34.\,33\ \&c.\ldots\ldots 4}{51.\,50.\,49.\,47.\,46.\,45.\,43\ \&c.\ldots\ldots 5} \times \left.\frac{1}{4}\right]^{12},$$

which, by ſtriking out common multipliers, becomes $\frac{1}{17.\,10.\,7.\,47.\,46.\,5.\,43.\,7.\,41} = \frac{1}{158753389900}$: ſo that the odds againſt it are 158753389899 to 1.

The ſame. Anſwered by W. T. of Bath.

SUPPOSING a pack of cards to be properly ſhuffled, and the bottom card to be afterwards turned up, and conſequently known; it is required, in the ſtrict ſenſe of the queſtion, to find the probability that every fourth card, dealt off from the top, ſhall be of the ſame ſuit with the trump card which is expoſed. But as the chance for ſome one of the trumps lying in the 4th, 8th, 12th, &c. place, is evidently the ſame as for its lying in any other place; it amounts to the ſame thing as to determine the probability of a perſon's drawing the 12 remaining trumps, one by one, at random, in 12 trials. To effect this, it is neceſſary to obſerve, that as there are now 51 cards in all, and only 12 in his favour, the probability of his drawing one of the trumps, the firſt trial, is $\frac{12}{51}$; and if this be ſuppoſed to be done, and the card taken away, there will now remain 50 cards in the pack, and only 11 in his favour; whence the probability of his ſucceeding the next trial is $\frac{11}{50}$. In like manner, the probability of his ſucceeding the third time is $\frac{10}{49}$, the fourth time $\frac{9}{48}$, the fifth $\frac{8}{47}$, the ſixth $\frac{7}{46}$, the ſeventh $\frac{6}{45}$, the eighth $\frac{5}{44}$, the ninth $\frac{4}{43}$, the tenth $\frac{3}{42}$, the eleventh $\frac{2}{41}$, and the twelfth $\frac{1}{40}$. And ſince the probability of the happening of any number of events, is equal to the product of the ſeveral probabilities of each of them happening, when conſidered ſeparately, we ſhall have $\frac{12}{51} \times \frac{11}{50} \times \frac{10}{49} \times \frac{9}{48} \times \frac{8}{47} \times \frac{7}{46} \times \frac{6}{45} \times \frac{5}{44} \times \frac{4}{43} \times \frac{3}{42} \times \frac{2}{41} \times \frac{1}{40} = \frac{1}{158753389900}$ for the probability required. So that the chances againſt the dealer, at the game of whiſt, having all the 13 trumps in his own hand, are above a hundred thouſand million to one. And if the chances thus determined be ſubſtituted in the analytical formula, given by Demoivre, in his Doctrine of Chances, 3d edit. Prob. III, it will be eaſy to ſhew that the number of trials in which this event may probably take place is 11112737229; whence, reckoning every game, one with another, to take 10 minutes, it would be neceſſary, in order to make it an equal chance for the event happening or failing, that the gameſters ſhould play on, uninterruptedly, for above two hundred thouſand years.

QUESTION IV. *Anſwered by* ANALYTICUS.

IT is laid down, by Newton, as a law of refrigeration, that the quantities of heat loſt, in given ſmall ſpaces of time, are always proportional to the heat remaining in the body; or that, if the times be taken in arithmetical progreſſion, the remaining heats will be in geometrical progreſſion; reckoning theſe heats to be the exceſs by which the body is warmer than the ſurrounding atmoſphere.

Suppoſe, therefore, that h is the temperature of any body above that of the atmoſphere, and that it cools d degrees in one minute. Then, by the above law, its temperature at the end of one, two, three, &c. minutes, will be $h - d$, $\overline{h-d}\,(\frac{h-d}{h})$, $\overline{h-d}\,(\frac{h-d}{h})^2$ &c. or $h-d$, $h\,(\frac{h-d}{h})^2$, $h\,(\frac{h-d}{h})^3$ &c. Whence, univerſally, its temperature at the end of m minutes will be $h\,(\frac{h-d}{h})^m$, and at the end of n minutes $h\,(\frac{h-d}{h})^n$.

And if theſe temperatures be determined by experiment, and denoted by a and b reſpectively, we ſhall have $h\,(\frac{h-d}{h})^m = a$, and $h\,(\frac{h-d}{h})^n = b$; from which equations h is found $= \frac{a^{\frac{n}{n-m}}}{b^{\frac{m}{n-m}}}$, or log. $h = \frac{1}{n-m}$ (n log. $a - m$ log. b), and $d = h \times (1 - \overline{\tfrac{b}{a}}|^{\frac{1}{n-m}})$

$$= \frac{a^{\frac{n}{n-m}}}{b^{\frac{m}{n-m}}} \times \frac{a^{\frac{1}{n-m}} - b^{\frac{1}{n-m}}}{a^{\frac{1}{n-m}}}.$$ Alſo $\frac{h-d}{h} = \overline{\tfrac{b}{a}}|^{\frac{1}{n-m}} = r =$ rate of cooling.

Again, becauſe $h\left(\frac{h-d}{h}\right)^m$ is the temperature of the body at the end of m minutes, and $h\left(\frac{h-d}{h}\right)^n$ its temperature at the end of n minutes, either of theſe forms will expreſs the temperature of any body at the end of any given time, from its firſt beginning to cool; and their difference $h\left(\frac{h-d}{h}\right)^m - h\left(\frac{h-d}{h}\right)^n$ or $h \times \left(\frac{h-d}{h}\right)^m - \left(\frac{h-d}{h}\right)^n$ is the heat which the body would loſe between the times m and n; h and d being found as above.

In like manner, ſince $h\left(\frac{h-d}{h}\right)^m = n$, or $\left(\frac{h-d}{h}\right)^m = \frac{a}{h}$, we ſhall have $m \log. \frac{h-d}{h} = \log. \frac{a}{h}$, or $m(\log. \overline{h-d} - \log. h) = \log. a - \log. h$; and conſequently $m = \frac{\log. a - \log. h}{\log. \overline{h-d} - \log. h}$ = time which a body takes in cooling, till it arrives at the obſerved temperature a.

And if it be required, as in the queſtion, to find the degree of heat to which any body has been expoſed, we ſhall have, in this caſe, hr^m = number of degrees between that temperature and the heat to which the body has been raiſed in the time m. Hence $h - hr^m$ = number of degrees the body has been heated, which call t; then $h - hr^m = t$; from which equation h is found $= \frac{t}{1-r^m}$, where r, m and t are known terms.

Theſe formulæ will be found of conſiderable uſe in ſeveral branches of chemiſtry, and are capable of a number of practical applications, which are left, for the preſent, to the conſideration of the reader.

NEW MATHEMATICAL QUESTIONS.

QUESTION VII. *By W. T. of Bath.*

AS a proper ſequel to Queſtion III, it is propoſed to determine the odds againſt the dealer and his partner, at the game of whiſt, having between them all the thirteen trumps.

QUESTION VIII. *By S. S. of Reading.*

Given $a^x + b^x = c$, to determine the value of x, either by logarithms or a converging ſeries.

SCIENTIFIC NEWS.

Letter from Sir BENJAMIN THOMPSON, *Kt. Count of Rumford, F. R. S. to the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S. announcing a Donation to the Royal Society for the Purpoſe of inſtituting a Prize Medal* *.

AT the anniverſary of the Royal Society, held the 30th of November 1796, the Preſident acquainted the Society that Count Rumford had transferred one thouſand pounds three per cent. conſolidated Bank Annuities to the uſe of the Society, on certain conditions ſtated in a letter to the Preſident; which was read as follows:

---

* From the Tranſactions for 1797.

"SIR,

"DESIROUS of contributing efficaciouſly to the advancement of a branch of ſcience which has long employed my attention, and which appears to me to be of the higheſt importance to mankind; and wiſhing at the ſame time to leave a laſting teſtimony of my reſpect for the Royal Society of London, I take the liberty to requeſt that the Royal Society would do me the honour to accept of one thouſand pounds ſtock in the three per cent. conſolidated public funds of this country; which ſtock I have actually purchaſed, and which I beg leave to transfer to the Preſident, Council, and Fellows of the Royal Society; to the end that the intereſt of the ſame may be by them and their ſucceſſors received from time to time for ever, and the amount of the ſame applied and given, once every ſecond year, as a premium to the author of the moſt important diſcovery or uſeful improvement which ſhall be made and publiſhed by printing, or in any way made known to the public, in any part of Europe during the preceding two years, on Heat or on Light; the preference always being given to ſuch diſcoveries as ſhall, in the opinion of the Preſident and Council of the Royal Society, tend moſt to promote the good of mankind.

"With regard to the formalities to be obſerved by the Preſident and Council of the Royal Society, in their deciſions upon the comparative merits of thoſe diſcoveries, which, in the opinion of the Preſident and Council, may entitle their authors to be conſidered as competitors for this biennial premium, the Preſident and Council of the Royal Society will be pleaſed to adopt ſuch regulations as they, in their wiſdom, may judge to be proper and neceſſary. But in regard to the form in which this premium is conferred, I take the liberty to requeſt, that it may always be given in two medals, ſtruck in the ſame die, the one of gold and the other of ſilver, and of ſuch dimenſions that both of them together may be juſt equal in intrinſic value to the amount of the intereſt of the aforeſaid one thouſand pounds ſtock, during two years; that is to ſay, that they may together be of the value of ſixty pounds ſterling.

"The Preſident and Council of the Royal Society will be pleaſed to order ſuch device or inſcription to be engraved on the die they ſhall cauſe to be prepared for ſtriking theſe medals, as they may judge proper.

"If, during any term of years, reckoning from the laſt adjudication, or from the laſt period for the adjudication of this premium, by the Preſident and Council of the Royal Society, no new diſcovery or improvement ſhould be made in any part of Europe, relative to either of the ſubjects in queſtion (Heat or Light), which in the opinion of the Preſident and Council of the Royal Society ſhall be of ſufficient importance to deſerve this premium, in that caſe it is my deſire that the premium may not be given; but that the value of it may be reſerved, and, being laid out in the purchaſe of additional ſtock in the Engliſh funds, may be employed to augment the capital of this premium; and that the intereſt of the ſame, by which the capital may from time to time be ſo augmented, may regularly be given in money with the two medals, and as an addition to the original premium, at each ſucceeding adjudication of it. And it is further my particular requeſt, that thoſe additions to the value of the premium, ariſing from its occaſional non-adjudications, may be ſuffered to increaſe without limitation.

"With the higheſt reſpect for the Royal Society of London, and the moſt earneſt wiſhes for their ſucceſs in their labours for the good of mankind, I have the honour to be, &c.

"*London, July* 12, 1796. (Signed) "RUMFORD.

"To Sir JOSEPH BANKS, Bart. K. B. Preſident
of the Royal Society of London."

The Society hereupon reſolved, that they accept of the donation, and accede to the conditions annexed to it by the Count; and alſo directed, that a letter be written to the Count, acquainting him of this acceptance; returning him thanks for the liberal donation, and aſſuring him that the conditions annexed to it will be ſtrictly adhered to.

NEW PUBLICATIONS.

VOYAGE dans les Alpes, &c. Tom. V. VI. VII. & VIII.; or Travels in the Alps, with a Preliminary Eſſay on the Natural Hiſtory of the Environs of Geneva. By Horace Benedict de Sauſſure, Emeritus Profeſſor of Philoſophy in the Academy of Geneva, and Member of various other Academies; two volumes in quarto, or four in octavo. Printed at Neuchatel, and imported by De Boffe, Gerrard-ſtreet. Price of the former , and of the latter 1l. 1s.

The former volumes of the Travels of this celebrated author, ſo rich in geological and philoſophical facts and obſervations, are too well known and eſteemed to require any general character in this place. Theſe volumes are equally intereſting, and contain a treaſure of information, which, nevertheleſs, from its peculiar nature and locality in many reſpects can admit of no abridgement.

The Firſt Report of the Society for bettering the Condition and increaſing the Comforts of the Poor. Octavo, 68 pages. Becket, London, 1797. Price one ſhilling.

The contents of this valuable pamphlet are enumerated with ſo much perſpicuity and effect by Thomas Barnard, Eſq. in the Preliminary Addreſs, that I ſhall have the pleaſure of giving them in his own words:

"Friendly ſocieties are the objects of the firſt paper, which preſents an intereſting detail reſpecting one at Caſtle Eden, upon a ſcale capable of general adoption: it contains an important illuſtration of the true principle of action with regard to the poor, and proves how much they may in a ſhort time learn to do for themſelves, and to what a degree of kindneſs and affection they may be habituated to extend their intereſt in the welfare of each other. The manner in which the poor and induſtrious member of that ſociety has been aſſiſted in the purchaſe of his cow, and its beneficial conſequences both to the individual and to the property with which he is connected, by increaſing and improving the ſtock upon it, is deſerving of attention and imitation.

"The ſecond is an account of a village ſhop:—a ſubject, the importance of which will be felt by all who intereſt themſelves in the domeſtic concerns of the poor, when it is known that a ſaving of 20 per cent. may be thereby made to the labourer in the purchaſe of the neceſſaries of life; that it is the moſt effectual means to prevent his running in debt; that the expence and trouble to the charitable founder of the ſhop are inconſiderable; and that it is liable to no objection but what may be eaſily obviated.

"The next communication is upon workhouſes of united hundreds, an enquiry of no ſmall importance at the preſent moment. The mode of their management *, and the objections and inconveniences that attend them under the beſt regulations and management, are ſtated with clearneſs and perſpicuity. The rules of a ſpinning-ſchool, eſtabliſhed with ſucceſs at Oakham, upon the principles of Count Rumford, is the next in order;—a ſchool where the poor attend with pleaſure and regularity, and thankfully receive the benefit of a cheaper and more nouriſhing diet ſupplied to them at a very ſmall price; and for theſe reaſons ſimply:—becauſe they are allowed to continue free agents, and to retain an option on the ſubject; and becauſe they have the whole of their earnings inviolably at their own diſpoſal. May the example be ſpeedily followed in other parts of England!

---

* See Sir William Young's Obſervations, publiſhed in 1788, and his Conſiderations on the Subject of Poor-houſes and Work-houſes 1796.

“ The fifth is an account of the jail and houſe of correction at Dorcheſter. When we conſider the important conſequence of what has been effected there, in annually ſaving, to the public and to themſelves, many perſons otherwiſe abandoned to deſtruction, we cannot help lamenting that ſo very few ſimilar inſtances are to be found in the whole kingdom. The principle of this reform will apply, with ſtill greater force, to every meaſure that regards the local and domeſtic concerns of mankind; in all of which it will invariably be found, that in proportion as coercion is given up, and the intereſt of the party is made the ſpring of action, temptations to vice will be excluded, and habits of labour and honeſty will be gradually acquired.

“ In the next paper, upon fuel, the reader will find a very gratifying proof that the poor may be eaſily reconciled to incloſure, or to any other meaſure of public benefit, where their own feelings and intereſts are only properly conſulted.

“ The laſt communication is on parochial relief, and the mode and principle on which it has been adminiſtered by the magiſtrates of the hundred of Stoke.”

Philoſophical Tranſactions of the Royal Society of London for the Year 1797, Part I. quarto, 218 pages, with 26 pages of Mineralogical Journal, and four plates. Sold by Elmſly, London, price 8s. 6d.

This Part contains: 1. The Croonian Lecture, in which ſome Morbid Affections of the ſtrait Muſcles and Cornea of the Eye are explained, and their Treatment conſidered. By Everard Home, Eſq. F. R. S. 2. Obſervations on Horizontal Refractions, &c. By Joſeph Huddart, Eſq. F. R. S. (See p. 145 of this work). 3. Recherches ſur les principaux Problemes de l'Aſtronomie. Par Don Joſef de Mendoza y Rios, F. R. S. 4. On the Nature of the Diamond. By Smithſon Tennant, Eſq. (ſee p. 177). 5. A Supplement to the Meaſures of Trees printed in the Philoſophical Tranſactions for 1759. By Robert Marſham, Eſq. F. R. S. 6. On the Periodical Changes of Brightneſs of two Fixed Stars. By Edward Pigott, Eſq. 7. Experiments and Obſervations made with the View of aſcertaining the Nature of the Gas produced by paſſing Electric Diſcharges through Water. By George Pearſon, M. D. F. R. S. 8. An Experimental Enquiry concerning Animal Impregnation. By John Haighton, M. D. 9. Experiments in which, on the third Day after Impregnation, the Ova of Rabbits were found in the Fallopian Tubes; and on the fourth Day after Impregnation in the Uterus itſelf; with the firſt Appearance of the Fœtus. By William Cruikſhank, Eſq. 10. Letter from Sir Benjamin Thompſon, Knt. Count of Rumford, F. R. S. &c. (for which ſee p. 189.). Appendix, containing the Meteorological Journal kept at the Apartments of the Royal Society.

Geometrical and Graphical Eſſays; containing a general Deſcription of the Mathematical Inſtruments uſed in Geometry, Civil and Military Surveying, Levelling, and Perſpective, with many new practical Problems; illuſtrated by thirty-four Copper-Plates. By the late George Adams, Mathematical Inſtrument Maker to the King, &c. Second edition, corrected and enlarged by William Jones, Mathematical Inſtrument Maker. Octavo, 515 pages. Sold by Jones in Holborn, price 14s. boards.

The character of this work is already well known to the public. It contains a large quantity of uſeful matter, treated with much perſpicuity, and illuſtrated by a number of accurate and beautiful engravings. Mr. Jones has conſiderably augmented and improved the preſent edition.

Count Rumford's Experimental Eſſays, Political, Economical, and Philoſophical. Eſſay VI. of the Management of Fire, and the Economy of Fuel. Octavo, 196 pages, with ſix plates. Price 3s. 6d. in boards. Cadell and Davies, 1797.

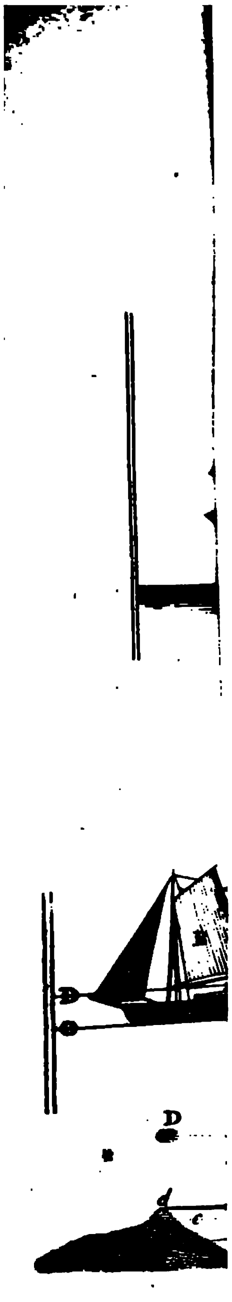
The reputation of this author, as a philoſopher and earneſt promoter of the immediate intereſts of humanity, is fully eſtabliſhed. The preſent eſſay is diſtinguiſhed by the ſame clear and perſpicuous detail of intereſting facts and inferences as the former Eſſays which appeared before the commencement of this Journal. It is divided into ſix chapters: 1. General View and Importance of the Subject. 2. Of the Generation of Heat in the Combuſtion of Fuel. 3. Of the Means of confining Heat and directing its Operations. 4. Of the Manner in which Heat is communicated by Flame to other Bodies. 5. Experiments with Boilers and Fire-Places of different Conſtructions. Relative Quantities of Heat from various Fuels. Eſtimates of the Total Quantities and Loſs under various Circumſtances. 6. An Account of a Number of Kitchens, Public and Private, and Fire-Places for various Uſes, which have been conſtructed under the Direction of the Author, in different Places.

⁂ J. F . . . . . . R is reſpectfully deſired to reconſider his ſolution of the fifth queſtion (p. 138)—more eſpecially becauſe he has inadvertently ſuppoſed the ſecond table on p. 39 to be capable of affording the ſpecific gravities of mixtures of common ſalt and water. For the reaſons there mentioned, inſtead of attempting to give the ſpecific gravities anſwering to degrees from a repetition of Baumé's experiment, I took the extreme number from the inſtrument, as found by De Morveau, and calculated the intermediate numbers, as they correſpond with equal differences of immerſion, of a cylindrical ſtem; which was the caſe above the firſt fifteen degrees, and probably alſo in thoſe very nearly. Various authors have given the ſpecific gravities of the ſolutions of common ſalt. The following table was formed by experiment with very fine dry cryſtals of marine ſalt, by Dr. Watſon, now Biſhop of Landaff. I have extracted it from a paper inſerted in the ſupplement to the thirteenth volume of the Journal de Phyſique, page 76, for the year 1778. I ſuppoſe it to be taken from the Philoſophical Tranſactions, but have not at this time the opportunity of conſulting the whole ſet.

A TABLE of the Specific Gravity of various Solutions of common Salt, at the Temperature of between 46° and 55° of Fahrenheit. Quantity of Salt by Weight = 1.

Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.
2	1.206	11	1.059	26	1.027	47	1.014	143	1.004	847	1.0008
3	1.160	13	1.050	27	1.025	53	1.013	161	1.003	1023	1.0006
4	1.121	14	1.048	29	1.024	55	1.012	191	1.0029		
5	1.107	15	1.045	31	1.023	71	1.009	255	1.0023		
6	1.096	17	1.040	35	1.020	83	1.007	319	1.0018		
7	1.087	20	1.032	38	1.019	107	1.006	447	1.0017		
8	1.074	23	1.029	41	1.015	125	1.005	511	1.0014		

facing p. 192.



Stock for tapping Screws. Fig 3.

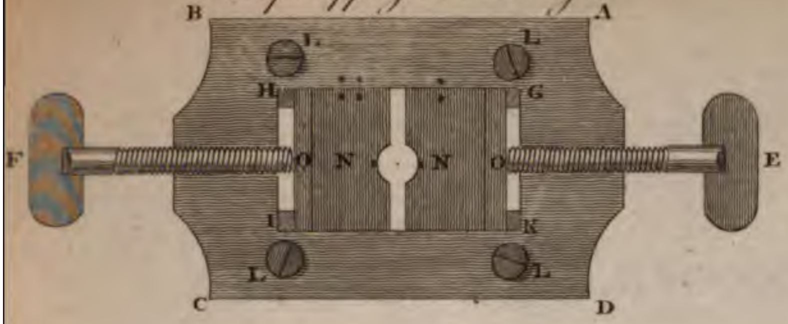
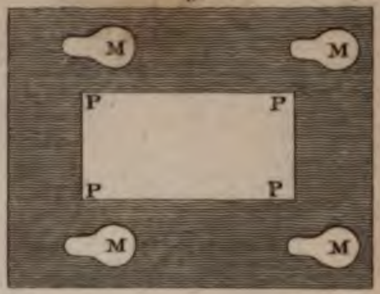
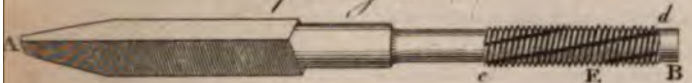


Fig. 4.



Tap. Fig. 1.



Screw Tool. Fig. 2.

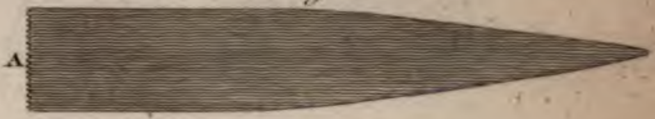


Fig. 5.

Compound Stock, for four pair of Dies. Fig. 7.

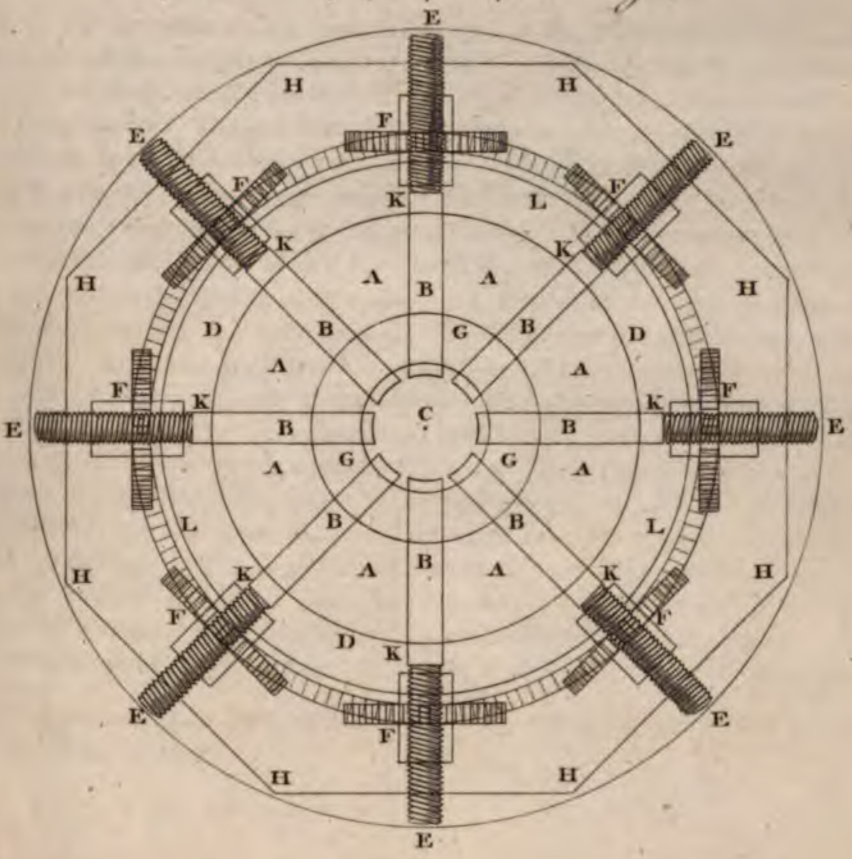


Fig. 6.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

AUGUST 1797.

ARTICLE I.

An Account of the New Syſtem of Meaſures eſtabliſhed in France. By CH. COQUEBERT[*].

FOR ſeveral centuries, reaſon, good faith, and all the principles of ſocial order have exclaimed againſt the diverſity of meaſures in France. Nothing was wanting for the deſtruction of this abuſe, already condemned by the public opinion, but a favourable concurrence of circumſtances. The members of the Conſtituent Aſſembly, charged by their electors to eſtabliſh at laſt that uniformity which had conſtantly been demanded by the States General, and no leſs frequently promiſed by Miniſters, gave charge to the Academy of Sciences to direct their reſearches to this great change. There was no doubt of finding in the learned a ſufficient degree of zeal for a work that muſt facilitate the communication of intelligence, extend the dominion of reaſon, and economiſe that time of which men the moſt inſtructed are ever the moſt ſparing. From this rare and valuable concurrence between the ſciences and the legiſlation has reſulted the ſyſtem we are about to explain to our readers. In this ſyſtem they will diſcern the character of a work independent of peculiar times or individual locality. Its authors have been aware that they could not worthily ſerve the French nation in this reſpect, without embracing in their plan the intereſts of the whole human race. They were deſirous that their labours ſhould be uſeful to all nations; and that the commerce which ſerves to unite them might everywhere ſpeak the ſame language. It is more particularly on this account, that inſtead of confining themſelves to render the meaſures of Paris of general uſe throughout France, they have ſought in the nature of things for the invariable baſis of a complete ſyſtem connected in all its parts.

The pendulum was propoſed in the laſt century to ſerve as the original of all meaſures;

---

[*] Journal des Mines, publié par le Conſeil des Mines de la République, N°. XIV. 73.

but it preſented two inconveniences: the firſt, that the diviſion of time is in itſelf arbitrary*; the ſecond, that the meaſures thence reſulting, and geographical meaſures depending on the magnitude of the earth, would have no mutual agreement†.

The preference was given to refer all meaſures large and ſmall to the terreſtrial globe. Among the great circles which meaſure its circumference, the preference was not given to the equator. The nature of the countries over which it paſſes, renders the obſervations too difficult. The determination was made in favour of meridians‡.

The whole meridian was not taken as the unity for ſubdiviſion, for two reaſons: the firſt, becauſe the quadrant is a true unity for mathematicians and aſtronomers, as is well known to all that cultivate the ſciences; ſecond, becauſe the magnitude of the circumference of the earth is known to us only by conjecture. All the great aſtronomical and geodeſial undertakings have been performed in the northern hemiſphere. We have yet no correct notion of the figure of the ſouthern hemiſphere. It would even reſult from the obſervations of Lacaille, at the Cape of Good Hope, that the terreſtrial ſpheroid is more flattened on this ſide. No hypotheſis can be admitted in a ſubject of this nature. The learned could therefore propoſe as a fundamental unity that only which was well known to them, namely, the diſtance from the north pole to the equator.

This diſtance is obtained with certainty from the meaſure of an arc of the meridian interſected by the forty-fifth parallel. France alone offers in a civiliſed country an arc of the requiſite magnitude, which, if we comprehend a ſmall part of the Spaniſh territory, terminates in the ſea at both extremities. This arc has beſides the advantage to have been repeatedly meaſured with the utmoſt care. Two celebrated aſtronomers are at this moment employed on this operation; not that it is preſumed that the reſult of their labour can have any ſenſible influence on the uſual inſtruments of meaſure, but to preſent to Europe, in all the parts of this great operation, a degree of preciſion honourable to the Sciences and to the Republic.

---

* We reckon 86400 ſeconds in a day; becauſe the day is divided into 24 hours, and the hour into 60 minutes. Every other diviſion would render the element of time longer or ſhorter, and accordingly influence the length of the pendulum. The pendulum which ſhould make one hundred thouſand oſcillations in a day, would be only 2.283 feet in length, whereas the pendulum for ſeconds is three feet 7.21 lines under the equator. C.

All the meaſures in this paper muſt be underſtood to be French, as I have thought it unneceſſary to make any other reductions but thoſe in the table p. 199. N.

† Under the name of geographical meaſures we denote in this place the degrees of latitude and longitude. It is commonly ſuppoſed that a degree of the meridian is twenty-five common leagues; but in order to afford this accurate quotient in round numbers, it is neceſſary to reckon the league at from 2281 to 2283 toiſes. The league cannot be at the ſame time a multiple of the pendulum in round numbers, and an aliquot part of a degree. C.

‡ This notion ſeems to have preſented itſelf to the ancients. There are ſatisfactory proofs that the cubit of the Nilometer in Egypt, the length of which is 1.712 foot, was contained four hundred times in a ſtadium, two hundred thouſand times in a degree, and conſequently ſeventy-two millions of times in the circumference of the earth. It is thought that the great Pyramid is a monument of this ſyſtem, and that its baſe is exactly the length of the ancient Egyptian ſtadium of five hundred to the degree. The ancients had nevertheleſs neglected to adopt an uniform method of diviſion, and in this their notion required improvement. The ſame obſervation may be made on the ſyſtem of meaſures likewiſe propoſed to be deduced from the magnitude of the earth, in the year 1670, by Mouton, an aſtronomer of Lyons. He thought it neceſſary to preſerve the diviſion of a circle into three hundred and ſixty degrees, and the degree into ſixty minutes; ſo that he does not propoſe the decimal diviſion but from the minute of the meridian. The thouſandth part of the minute of the meridian affords a meaſure of five feet eight inches and a half, which he propoſes for the uſual unity. C.

It was not thought neceſſary to wait the termination of their work previous to aſcertaining the diſtance from the pole to the equator in the ancient meaſures. It is ſufficiently known, by the obſervations of learned men, that the length of a degree of the meridian, taken at an equal diſtance from the pole and the equator, is 57027 toiſes.

It is likewiſe known, that from this mean degree the degrees become longer each by about twelve toiſes in approaching the pole, and ſhorter in the ſame proportion by advancing towards the equator, on account of the oblate figure of the terreſtrial globe towards the poles. This degree, interſected by the forty-fifth parallel, is therefore the mean term of an arithmetical progreſſion, which multiplied by 90 will give the diſtance from the pole to the equator.

57027 multiplied by 90 produces 5132430 toiſes, or 30794580 feet.

This is the fundamental unity expreſſed in ancient meaſures from the iron toiſe of the Academy, ſuppoſed to be at the temperature of 13 degrees of Reaumur's thermometer.

If the new obſervation ſhould give this diſtance a minute quantity larger or ſmaller, it will not be neceſſary on that account to change the number of toiſes which repreſent it. It will be ſufficient to vary in the proper direction the aſſumed temperature of the iron toiſe *.

The unity being thus determined, the ſecond ſtep was to divide and ſubdivide it in ſuch a manner as to obtain linear meaſures appropriated to the different uſes. An eſſential condition was to chooſe a proper number to be employed, and to employ it conſtantly and excluſively, in order to reduce the arbitrary elements of the ſyſtem as much as poſſible. Some learned men were deſirous that the number 12 might be adopted, becauſe diviſible immediately into the half, the quarter, and the ſixth; others, on the contrary, being impreſſed with the inconvenience of theſe fractions of variable denominators, would have preferred ſome prime number, ſuch as 7 or 11, which would have rendered the uſe of all ſuch fractions impracticable. But very effectual conſiderations proved deciſive in favour of the number 10. For, 1. This number is in ſome reſpect pointed out by nature, ſince we find the decimal numeration among nations which do not appear to have had any communication.

2. Nothing can be better adapted to facilitate calculations, than to ſubject the diviſion of meaſures to the ordinary laws of arithmetic; whence the converſion of one meaſure into another of a ſuperior or inferior order is effected by a ſingle ſtroke of the pen, and fractions may be worked with the ſame eaſe as whole numbers. This laſt advantage could not be united to thoſe which were ſuppoſed to exiſt in the numbers 7, 11 and 12, without entirely changing the ſyſtem of oral and written numeration; an enterpriſe which perhaps is out of the power of any government to accompliſh.

The diſtance from the pole to the equator being taken as the fundamental unity, and the number 10 for the only diviſor, conſtitute the ſimple and productive principles on

---

* Men of ſcience agree that the meaſure of the arc of the meridian may be depended on to the five thouſandth part (or barely to four places of figures. N.). If therefore it ſhould happen that the error amounts to the utmoſt poſſible quantity, it would be entirely remedied by ſuppoſing the toiſe to be lengthened or ſhortened one five-thouſandth part by the effect of temperature. Every degree of variation, according to the ſcale of Reaumur, produces on the length of an iron bar a variation of one-75000th part. C.

which this ſyſtem is eſtabliſhed. They are ſo little arbitrary, that poſterity, even if our books and monuments were loſt, might eaſily recover them; whereas, for want of a clue of this nature, the learned are not yet agreed concerning the true dimenſions of moſt of the Greek and Roman meaſures. Magnitudes arbitrarily taken cannot be preſerved but by means of metallic ſtandards, which are altered by time or negligence, and in the revolution of empires are totally loſt. The French nation will have no variation to fear in its meaſures, becauſe it has referred them not to the periſhable works of man, but to every thing which is moſt durable in nature.

Names were to be affixed to each of the meaſures reſulting from the ſyſtem we have expoſed. It was not enough to denote them by their relation to the fundamental unity, and to ſay, for example, the hundred thouſandth, the millionth, or the ten millionth part of the quarter of the meridian. Theſe are definitions; but the mind has need of particular ſigns, to give, as it were, a body to each idea.

The words primes, ſeconds, thirds, and the like, might have been uſed to denote each degree of the decimal diviſion. And theſe words would have poſſeſſed the advantage of always denoting the proportion of each of theſe inferior unities to the prototype which is the baſis of the ſyſtem.

It was thought proper to follow another method, perhaps leſs exact, but more conformable to the ordinary practice. Having arrived, after ſeven ſucceſſive diviſions, to a meaſure of three feet eleven lines and forty-four hundredth parts, which is the ten millionth part of the fundamental unity, a name was given to this uſual and portative meaſure, adapted to take place of the foot, the toiſe, and the ell. It was called metre. Proceeding afterwards from this meaſure as the common unity, its multiples have been named by prefixing to the word metre, one of the Greek words deca, hecaton or hecto, chilio or kilo, and myria, which ſignify ten, a hundred, a thouſand, and ten thouſand; and its ſubmultiples by means of the Latin prefixes deci, centi, milli.

Conſequently a decametre is a meaſure conſiſting of ten metres (30 feet nine inches), proper to be ſubſtituted in place of the meaſuring chain for land.

The kilometre and the myriametre are itinerary meaſures; the one of 1000, and the other of 10,000 metres (513 and 5132 toiſes), the firſt being equal to a ſmall quarter of a league, and the ſecond two medium leagues, or one poſt.

Ten myriametres make the 100th part of the quarter of the meridian, which may, if thought fit, be called the decimal degree.

The decimetre, or 10th part of a metre, anſwers to three inches eight lines and four points.

The centimetre, or 100th part of a metre, is four lines and five points.

And laſtly, the millimetre is five points and a third.

Every other kind of meaſure is derived from the metre and its parts; ſuch as the meaſures of land, of ſolids, of liquids, of grain, and even of weights and coin. The whole is therefore derived from the ſame origin, and the ſyſtem exhibits a complete whole.

A portion of land of one ſquare decametre, or one hundred metres ſquare, has received the name of are.

A ſquare hectometre, or ten thouſand ſquare metres, conſtitutes one hundred ares, or an hectare. Theſe are the agrarian meaſures, ſubſtituted in the place of the perch and the acre (arpent).

The cubic metre receives the name of ſtere when employed to meaſure wood for fuel, in which it is ſubſtituted for the cord and other ancient meaſures. The tenth part of the ſtere, or deciſtere, correſponds very well with the meaſure commonly called ſolive in the commerce of wood for carpentry.

A veſſel of a cubical form, having for its ſide one decimetre, or a cylindrical veſſel of the ſame ſolid contents, has received the name of litre. It contains about two pounds of water, or twenty-five ounces of wheat. It is aſſumed as the element for meaſures of capacity, and every other meaſure of this kind is conſidered as the decimal multiple or ſubmultiple of this. So that we have the decalitre of ten litres, containing ſixteen pounds of wheat, or four-fifths of the buſhel of Paris; the hectolitre, which contains near 160, or two-thirds of the ſetier. One thouſand litres are equal in capacity to the cubical metre.

With regard to weights, the original weight is taken to be equal to the quantity of diſtilled water contained in a cubical veſſel, the ſide of which is one-hundredth part of the metre. This water weighed in vacuo, and at the temperature of melting ice, is equal to eighteen grains and eight hundred and forty-one thouſandth parts. The denomination gramme is given to this weight, and from the gramme are deduced by multiplication or diviſion all the weights ſuperior and inferior.

So that in aſcending we have the decagramme equal to $2\frac{2}{3}$ gros.

The hectogramme equal to $3\frac{1}{4}$ ounces.

The kilogramme equal to 2 pounds 5 gros 49 grains.

The myriagramme equal to about $20\frac{1}{2}$ pounds.

And by deſcending we have the decigramme, which is nearly 2 grains.

The centigramme, $\frac{1}{5}$th of a grain nearly.

And the milligramme, which is nearly the 1-50th of a grain.

Laſtly, the diviſion may be carried to the ten-thouſandth part of a gramme, which may be ſubſtituted for the eſſayer's weight of the 512th part of a grain.

We ſee therefore that the gramme is placed between two ſeries of like number of terms; the one, which increaſes to the myriagramme, with which all heavy weights will be aſcertained; and the other, which deſcends to the ten-thouſandth part of the gramme.

It is this conſideration which has cauſed the radical name to be given to a weight ſo ſmall and ſo little adapted to the ordinary uſes of life. It is not pretended, that on this account it ſhould be conſidered as the ſole or even the principal unity. Like the others, it is merely a term in the ſeries of weights; any one of the unities of which may be taken according to convenience. The word radical could not be applied to a unity of a ſuperior order, without ſacrificing much of the regularity and method of this nomenclature.

The pieces of money being adjuſted to a certain number of grammes, are connected with the general ſyſtem of meaſures.

Pieces of one centime will be formed of copper, weighing one gramme; of five centimes, or a ſou, weighing five grammes; of one decime, weighing ten grammes; and of two decimes, weighing twenty grammes.

In ſilver there will be pieces of one franc weighing five grammes, and of five francs weighing twenty-five grammes.

Laſtly, in gold, there will be pieces of ten grammes.

This explanation is ſufficient to exhibit the ſyſtem in its true light, and to ſhew its beauty, ſimplicity, and numerous advantages. Thoſe who are deſirous of more extenſive developments, will find them in the works publiſhed by the temporary Commiſſion of Weights and Meaſures, and by the Agency inſtituted to conduct this great and uſeful operation to its end. We have thought it neceſſary to annex to this memoir, 1. A table of the new ſyſtem. 2. Tables of the relation between the old (French) meaſures and the new; by which it is eaſy to reduce the one to the other. The object of theſe tables is to reduce all the calculations relative to this transformation to ſimple addition. They will likewiſe ſerve to determine the price of the new meaſures from that of the old. In order to render them leſs voluminous, the ſimple unities are given from one to nine only. From the value of the unities it is eaſy to infer the value of decimal multiples or fractions, by ſhifting the decimal point.

To theſe tables we have added the logarithms of the ratios between the old and the new meaſures.

By this means every one who is engaged either in the art of mining, or the ſciences relating to the ſame, will find collected in one view all that is wanting to facilitate the ſtudy and the practice of the new ſyſtem.

⁂ The table numbered 2 in the original, occupies conſiderable ſpace, and, though of great utility to the inhabitant of France, is comparatively of ſmall importance to any other nation. It is merely a table for facilitating reductions of the old and new meaſures of France into each other, and does not convey any additional information to that in the memoir. For theſe reaſons I have not copied it.

In the way of curſory obſervation on the objects of this honourable and uſeful undertaking, it may be remarked, that arguments deduced from conſiderations of the immediate numerical application of an original meaſure, are of little conſequence as to the choice of the means of obtaining it. The chief, and indeed the ſole, motive of preference muſt conſiſt in the greater degree of accuracy, or more perfect agreement of the reſults wrought out by different obſervers. In experiments to determine the length of a pendulum vibrating through any known portion of the time employed by the earth in its rotation, we have to enquire, 1. Whether that rotation be theoretically uniform? and if not, what are the quantities and periods of irregularity, perceptible in ſuch an experiment? 2. What are the beſt methods and limits of error in meaſuring the length of a pendulum between its centres of oſcillation and ſuſpenſion? 3. Whether the errors from temperature can be rendered inſenſible in this meaſure of time and length; and if not, what are their limits? And above all, 4. What are the effects of the eſcapement part, or ſimpleſt maintaining power in a clock; and what are the beſt means of diminiſhing or removing them altogether.

In the method of deriving an original ſtandard from the meaſurement of lines and angles upon the earth, the objects of enquiry are certainly not leſs numerous than in the pendulum; but the ſuperiority of one method beyond the other would require a complete treatiſe for its diſcuſſion. Among the lateſt modern ſurveys, thoſe of General Roy, in the Philoſophical Tranſactions, made with inſtruments of wonderful accuracy, will afford the greateſt inſtruction to ſuch as may wiſh to enter this path of inveſtigation. In the laſt paper of the continuation of this ſurvey, by Lieutenant Colonel Williams, Captain Mudge, and Mr. Dalby, inſerted in the Tranſactions for 1795, a fundamental baſe was meaſured on Hounſlow Heath, equal to 27404.2 Engliſh feet, and connected, by obſervation of the angles of ſeventeen triangles, with a baſe of verification meaſured on Saliſbury Plain. As the triangles preſented ſeveral ranges of connection, theſe were computed from the fundamental baſe, and gave the numbers 36574.8 and 36573.8 for the extreme reſults, expreſſing the baſe of verification in feet. The mean of theſe, namely, 36574.3, proved to be about one inch ſhort of the actual admeaſurement of the baſe of verification. It ſeems probable that there may have been ſome fortunate compenſation of errors in this wonderful co-incidence; but the reſult appears to prove, that the method of terreſtrial admeaſurement will give an original meaſure to be depended on for more than four places of figures, and leſs than five.

When ſurfaces and ſolids are to be derived from linear meaſures, if there be any error in the original line, the error in the ſurface or area will be twice as great, and that in the ſolid three times as great, as in the line very nearly, as may be eaſily deduced from the common proceſs of involution. That is to ſay, in moſt caſes it will affect the ſame figure of the reſult. N.

TABLE OF THE NEW MEASURES OF THE (FRENCH) REPUBLIC,

Containing the Methodical Syſtem of Nomenclature, and their Proportion to the Ancient Meaſures.

Proportions of the Meaſures of each Species to its principal Meaſure, or Unity. Letters.	Figures.	Firſt Part of the Name which indicates the Proportion to the principal Meaſure, or Unity.	PRINCIPAL MEASURES OR UNITIES. Length.	Capacity.	Weight.	Agrarian.	For Fire-wood.
Ten thouſand	10000	Myria	Metre	Litre.	Gramme	Are	Stere
Thouſand	1000	Kilo					
Hundred	100	Hecto					
Ten	10	Deca					
One	1	———					
Tenth	0,1	Deci					
Hundredth	0,01	Centi					
Thouſandth	0,001	Milli					
Proportion of the principal Meaſures between themſelves and the Length of the Meridian			Ten millionth part of the diſtance from the Pole to the Equator.	A decimetre cube.	Weight of a centimetre cube of diſtilled water.	100 ſquare metres.	One cubic metre.
Value of the principal Meaſures, and the ancient French Meaſures			3 feet 11 lines and $\frac{1}{2}$ nearly.	A pint and $\frac{1}{20}$, or one litron and a quarter nearly.	18 grains & 841 thouſandth parts.	Meaſure of two ſquare perches des eaux et forêts.	One demi-voie nearly, or one quarter of a cord meſure des eaux et forêts.
Value in Engliſh Meaſures			39.383 inches.	61.083 cubic inches, which is more than the wine quart, and leſs than the ale quart.	22.966 grs.	119.68 ſqu. yards.	

EXAMPLES

Of the Names, compoſed, in order to expreſs different Unities of Meaſures.

Myriametre, length of ten millimetres, equal to about two leagues (or ſix Engliſh ſtatute miles one furlong and 159$\frac{3}{4}$ yards).

Kilogramme, weight of 1000 grammes, equivalent to a little more than two pounds, *poids de marc* (or 22966 grains Engliſh, which is 3lb. 4$\frac{1}{2}$oz. very nearly).

Hectare, Agrarian meaſure, equal to about two arpents des eaux et forêts, or three, meaſure of Paris (or 11968 ſquare yards Engliſh, which is rather leſs than 2$\frac{1}{2}$ acres).

Decalitre, Meaſure of capacity for corn, equal to about [illegible]-fifths of the Paris buſhel (or 610.83 cubic inches Engliſh, which is to the Engliſh peck of 537.6 inches, nearly as 8 to 7).

Decimetre, tenth part of a metre, equal to about three (French) inches and a half (or 3.9383 inches Engliſh).

Centigramme, hundredth part of a gramme, equal to about [illegible]6ths of a French grain (or 0.22966 grains Engliſh).

N B. Many compounds, ſuch as *Decaare*, *Kiloare*, and all thoſe which are formed with the ſtere, will not be uſed.

C O I N.

The monetary unity is called	*Franc.*
The Franc is divided into ten	*Decimes.*
And the *Decime* is divided into ten	*Centimes.*

The *Franc* does not differ from the ancient *livre tournois*; its value is that of a piece of ſilver of nine-tenths fine, weighing 5 grammes.

PROPORTION between the ANCIENT FRENCH MEASURES and the NEW.

The diſtance from the Pole to the Equator being { 5132430 toiſes. or 30794580 feet.

		Logarithms of the Proportions.			Logarithms of the Proportions.
Value of the mētre in Paris ells - -	0,841712	9,925164	Value of the Paris ell in metres - - -	1,188,05	0,074836
Value of the metre in feet - - -	3,07946	0,488474	Value of the foot in metres - - -	0,324732	0,511526
Value of the ſquare metre in ſquare feet	9,48306	0,976948	Value of the ſquare foot in ſquare metres	0,105451	9,023052
Value of the are in poles or perches of 18 feet ſquare - - -	2,92687	0,466403	Value of the pole or perch of eighteen feet ſquare in ares - -	0,341,662	9,533597
Value of the cubic metre in cubic feet - -	29,2027	1,465423	Value of the cube foot in cube metres -	0,0342434	8,534577
Value of the litre in Paris pints - - -	1,05130	0,021725	Value of the Paris pint in litres - -	0,951206	9,978275
Value of the decalitre in pariſh buſhels	0,788473	9,896787	Value of the Paris buſhel in decalitres - -	1,26827	0,103213
Value of the gramme in grains, *poids de marc*	18,841	1,275104	Value of the grain, *poids de marc*, in grammes	0,053076	8,724896
Value of the decagramme in ounces - -	0,327101	9,514681	Value of the ounce in decagrammes - -	3,05716	0,485319
Value of the kilogramme in pounds, *poids de marc* - - -	2,04438	0,310561	Value of the pound, *poids de marc*, in kilogrammes - -	0,489146	9,689439

II.

Analyſis of a Memoir of Citizen BONHOMME on the Nature and Treatment of Rachitis, or the Rickets.

[Concluded from page 177.]

THE following are the experiments by which Citizen Bonhomme appears to have ſucceeded in proving this truth. I ſhall tranſcribe his own words.

" I cauſed ſeveral young fowls of the ſame incubation to be fed in different manners. Some received the uſual food without any mixture; others received daily a certain quantity of calcareous phoſphate mixed in the ſame paſte as formed the ſupport of the others; and laſtly, one of them was fed with variations in the uſe of the mixture: the calcareous phoſphate was ſometimes given and ſometimes ſuſpended. When theſe fowls, after two months, had acquired their ordinary growth, I examined and carefully compared the ſtate of their bones. The progreſs of the oſſification in the epiphyſes was various according to the nature of the food the animal had received. The bones of the laſt fowl, which had received the phoſphate only from time to time, were rather more advanced than the bones of thoſe which had been fed without mixture. The bones of thoſe fowls which had been habitually fed with the mixture were evidently more ſolid, and their epiphyſes were much leſs perceptible. Simple inſpection was ſufficient to ſhew theſe differences when the bones were mixed together.

"I had fed ſeveral young fowls of the ſame incubation according to another plan. Some were fed on a ſimple paſte, without mixture; for others it was mixed with pulveriſed madder-root; and a third compoſition was made of this laſt paſte and calcareous phoſphate. This was alſo given habitually to other fowls. When after two months I examined the progreſs of oſſification in the bones of theſe different animals, I eaſily perceived the red traces of the madder in the oſſified parts of all thoſe which had uſed it; but I obſerved that the oſſification was not more advanced by the ſimple mixture of this root than by the ordinary food: on the contrary, the bones of thoſe fowls which had ſwallowed the phoſphate mixed with madder were much more ſolid than the others. The red colour ſerved admirably to diſtinguiſh the extremities of the long bones from their epiphyſes. After an exact compariſon, there could be no doubt of the efficacy of calcareous phoſphate in favour of the progreſs of oſſification. The virtue of the madder ſeemed confined to that of giving colour to the oſſified parts."

From theſe experiments, it was natural to make the trial of calcareous phoſphate in addition to the remedies made uſe of in the treatment of rachitic ſubjects. Here follows what the Author himſelf ſays, after having ſpoken of the exaggerated praiſes given by Haen to the uſe of oyſter-ſhells (oſtracodermata) in the treatment of rachitis.

"Without pretending," ſays he, "to a reſult ſo brilliant as that announced by Haen, I can affirm that the calcareous phoſphate has ſucceeded very well in the greateſt number of rachitic ſubjects to whom I have given it. I ſhall not multiply my accounts of ſucceſsful caſes in this place, but ſhall relate only two remarkable inſtances.

"The daughter of Mr. Ranchon, watch-maker, aged two years and a half, walked with a feeble and tottering pace, and the extremities of all her bones preſented epiphyſes very prominent. In this ſituation ſhe exhibited the appearance of imperfect rachitis, or the firſt period of this diſorder. Alkaline lotions, which I immediately adviſed, were attended with a good effect. Her ſleep became more firm; and as the firſt paſſages were in a good ſtate, I gave, without internal preparation, one ſcruple of a mixture of equal parts of phoſphate of lime and phoſphate of ſoda twice a-day. In the courſe of three weeks her legs were perfectly reſtored; and this amiable infant has ever ſince had the ſatisfaction to run with ſpirit and agility.

"A female infant, of the name of Boiard, aged four years, had experienced from her birth the moſt decided ſymptoms of rachitis. The protuberance of the epiphyſes and tumefaction of the abdomen firſt indicated the diſeaſe. The impoſſibility of ſupporting herſelf and walking at the uſual age confirmed theſe unfortunate ſymptoms. By degrees the glands of the neck and of the meſentery became ſwelled; the teeth were blackened, became carious, and were not replaced. This ſituation became ſtill more afflicting by criſes almoſt periodical at an interval of three or four weeks. At theſe afflicting periods, a fever of conſiderable ſtrength, cardialgia, and even convulſions, particularly in the night, were obſerved. The termination of each paroxyſm was announced or aſcertained by abundant ſtools, and the evacuation of urine ſtrongly charged with an earthy ſediment. The imprudent exhibition of a purge at the beginning of one of theſe criſes had nearly deprived the patient of her life. In this ſtate it was that I beheld her for the firſt time in the month of January 1791. The alkaline lotion was the only remedy the mother adopted in the firſt inſtance, and it produced a remarkable effect. After eight days the infant was ſo much better as to be able to ſupport herſelf. The remedy was then laid aſide, and eight days afterwards the child was incapable of ſtanding without ſupport. The uſe of the alkaline ſolution being renewed, was attended with the ſame ſucceſs, and its diſcontinuance was again followed by the complete return of all the ſymptoms. In the firſt days of March, the other remedies I had adviſed were exhibited. The conſtipation which had always exiſted became leſs, and the following criſis was effected without pain. And at length the convulſions, the pains, and the criſes diſappeared; but the impoſſibility of walking ſtill remained. At this time, namely on the ſecond of May, I gave the child the phoſphate of ſoda and calcareous phoſphate mixed together, in the doſe of half a dram twice a day. At the end of the month ſhe was able to ſtand upright, leaning againſt a chair, and the ſwellings began to diminiſh. She continued for a long time afterwards to take the mixture of the phoſphates. I likewiſe gave her occaſionally one grain of the extract of bile, prepared with ſpirit of wine; and at length in the month of July I had the pleaſure to ſee the patient run and play in the middle of the ſtreet with the other children of her own age, &c.

" J. B. Magné, aged two years and a half, appeared ſtrong and well formed. A general rachitis manifeſted itſelf rapidly in his conſtitution, without apparent cauſe. He ſoon became incapable of walking, and his relations learned from his playfellows that he had frequently evacuated white and thick urine. Moſt of the remedies and methods propoſed by different authors were made uſe of. At length diſcouraged at ſo many uſeleſs trials, the parents gave up every remedy, and the child remained incapable of walking. The bones of the legs were ſoftened and bent. At the age of four years and a half young Magné had the ſmall-pox; the diſorder was acute, but terminated happily without any particular attention. When the deſquamation was complete, the child was no longer capable of ſupporting himſelf. At this period he was brought hither, and I was conſulted. I obſerved that part of the bones were ſo much ſoftened as to occaſion variations and even violent pain in his walk. I adviſed a purgative, with alkaline lotions and the uſe of calcareous phoſphate, and a proper regimen. Eight days afterwards, the whole ſurface of the ſkin was covered with an infinity of ſmall bliſters, reſembling the itch. My preſcriptions were regularly continued, and in one month the ſolidity of the bones was entirely ſecured. For more than a year he has experienced no relapſe. In this caſe we may remark the inutility of the uſual remedies, the advantage of cutaneous eruptions, and the efficacy of the treatment I propoſe."

The incurvated ſpine, (though apparently confined to the vertebræ, which bones are not only affected with preternatural enlargement, but frequently alſo with caries) is this diſorder, I ſay, to be regarded as analogous to rachitis, or at leaſt to accidental rachitis? Both have been produced by the repercuſſion of cutaneous eruptions. Citizen Bonhomme relates obſervations in which the two affections are ſo combined that they appear to form one ſingle diſorder, varied only according to the parts affected. Is the uſe of calcareous phoſphate applicable to the treatment of the vertebral diſeaſe, to complete and accelerate the frequently partial proceſs of other remedies? The Author thinks that the following obſervation is concluſive in this reſpect:

"At the end of the month of February 1790," ſays this phyſician, "a rachitic child, aged eight years (J. Eſprit Guinde), was preſented to me in the General Hoſpital of this town. He had been for ſeveral weeks among the patients under the care of the ſurgeon. At the firſt moment a prominence of the firſt dorſal vertebræ was obſerved, with a conſiderable weakneſs of the legs. A large bliſtering plaiſter had been applied over the commencing gibboſity, and the bliſter was kept open by powder of cantharides for more than a month. The ſituation of the infant was not amended. I remarked in this patient all the characters of the diſeaſe deſcribed by Pott. I concluded that the bliſter had produced as great a diſcharge as was neceſſary, and that it was more eſſential to oppoſe the progreſs of the rachitic habit. I ordered the whole of the ſpine to be carefully waſhed three times a-day with an alkaline and aromatic lixivium. The phoſphate of ſoda was mixed with twice as much powder of burnt hartſhorn. Of this I gave to the child one ſcruple twice a-day in a ſpoonful of ſoup. After the firſt week he was able to walk with aſſiſtance; by degrees the ſolidity of his legs was re-eſtabliſhed, and the ſpine appeared to recover itſelf. And at length, on the 31ſt of December, when I quitted the ſervice of that hoſpital, the boy thought himſelf ſufficiently recovered to return to his friends.

"Eight or nine months afterwards, I found this unfortunate child in another hoſpital appropriated to the reception of ſcrophulous patients. From his anſwers to the queſtions I propoſed, I found that the treatment of the month of December had not been continued long enough to deſtroy the principle of the rachitis; that the diſorder had been as it were ſubdued during ſix months, but that coarſe food had developed it again. And laſtly, that the ſwelling of certain glands, and a diminution of the ſolidity of ſeveral ribs and of the humerus of the right ſide, were the effects of its renewed activity. I ordered purgatives to be repeated every eight days, the alkaline lotion, and the mixture of the phoſphate of lime and ſoda; in addition to which I preſcribed the uſe of the ſaponaceous and mercurial pills. The efficacy of theſe remedies was very ſenſible. The advances of the rachitic diſorder were ſtopped; the evacuations were kept up; *the diſtortion of the ribs and of the humerus diſappeared;* but the original gibboſity remained. This deformity, which could not be effaced, did not diminiſh the pleaſure I had to ſee this child perfectly cured after three months treatment."

In moſt of theſe obſervations mention is made of alkaline lotions and their beneficial effects. The following is the Author's own account:

"In ordinary caſes of rachitis, particularly at the commencement of the diſorder, it is of advantage to uſe a ſimple ſolution of pot-aſh to waſh the parts affected. This ſolution is made by diſſolving from half an ounce to an ounce of purified pot-aſh * in a pound of diſtilled or very pure ſpring water. When it is to be uſed, the ſkin muſt firſt be rubbed with a dry cloth, or a piece of fine flannel. After this precaution, the diſeaſed extremities are to be waſhed carefully with the warm ſolution, and at length wiped, ſo as to leave no trace of moiſture. This practice and waſhing muſt be repeated at leaſt twice a-day. I can affirm, from repeated trials, that it will ſoon be attended with ſucceſs.

---

* M. Hallé, in a note on this paſſage, obſerves, that pure pot-aſh, or the vegetable alkali, is a moſt powerful cauſtic, and cannot be uſed in theſe proportions. He found a ſolution of only one-eighth part of the ſalt here indicated was too ſtrong a lotion for the ſkin of an infant which was very irritable. Citizen Bonhomme, upon enquiry made, acquainted him that the pot-aſh he uſed was that of the ſhops, which is very far from being in a cauſtic ſtate. Probably it may be the ſame as the common ſalt of tartar of moſt of our ſhops in London, which is nothing but pot-aſh or pearl-aſh diſſolved in water, then filtered, and the water driven off by evaporation. It accordingly retains all its ſaline impurities. N.

"The ſolution of pot-aſh is not very coſtly; nevertheleſs, the habitual and long continued uſe of this remedy for a rachitic patient in the ſecond period, becomes expenſive to poor parents. The lixivium of wood-aſhes, which has been uſed for waſhing fine linen, in which aromatic plants may be infuſed, becomes of remarkable utility for the rachitic children of the poor. I have ſeen the moſt decided ſucceſs reſult from its uſe. Several children, who, after having walked alone, found a difficulty in ſupporting themſelves, were waſhed two or three times a-day with this lixivium, and in the ſpace of one or two weeks they walked with eaſe and agility, &c.

"I have ſeen various inſtances of children cured of their diſpoſition to rachitis merely by waſhing with alkaline liquids; but in moſt caſes I have thought it neceſſary to ſecure the firſt ſucceſs by other remedies. When the rachitis has already made ſome progreſs, it is evident that all the remedies muſt be united, and ſteadily continued. The alkaline lotion is a remedy the more preferable, as it is not at all diſguſting, and ſcarcely in any reſpect troubleſome to children; but the internal remedies poſſeſs ſuperior efficacy."

If the facts here announced ſhould be confirmed by further experience, might we not hope to obtain ſimilar advantage in ſuch other diſeaſes as attack the ſubſtance of the bones, and probably have more analogy to the rachitis than has been imagined? Such are the *ſpina ventoſa*, ſcrophulous tumours and caries, the difficulties in the formation of the bony proceſs, after fractures, ſlowneſs and irregularities of dentition, &c. Theſe queſtions, highly deſerving of inveſtigation, are produced by the Author of this memoir as conſequences of what he has related. He likewiſe communicates ſome uſeful notions reſpecting the caſes in which the other combinations of phoſphoric acid may be employed, ſuch as the phoſphates of iron and of mercury, concerning which experience has yet afforded him no information.

With the ſame candour and diffidence as is diſplayed by the Author of this memoir, we preſent his experiments as eſſays worthy of the attention of enlightened phyſicians. We have no doubt but the Author will direct his attention in future to extend his proofs and increaſe the degree of preciſion and evidence. And even if others ſhould go beyond him in this important enquiry, he will nevertheleſs enjoy the honour of having begun it.

Let us conclude this abſtract by a few reflections. It ſeems to us, that a multitude of ideas muſt preſent themſelves to him who ſhall meditate on the facts here diſplayed. Without dwelling on thoſe which are ſtill too hypothetical, the ſingle fact of the tranſition of calcareous phoſphate through the paſſages of circulation, and conſequently its ſolution in the fluids, and its application to the ſubſtance of the bones, deſerves the higheſt attention. The means which nature employs to render the calcareous phoſphate ſoluble are well entitled to the reſearches of chemiſts: without doubt its combination with ſoda may have ſome part in this effect; but it is not leſs important, in this place, as a remarkable phenomenon in the animal economy, which is eſſentially connected with nutrition, and the development of our organs. If we compare the experiments of Vauquelin on the ſeminal fluid, with thoſe of Fourcroy on the ſeroſity of milk, and more eſpecially a remarkable fact communicated to us by this laſt chemiſt, namely, that the nearer the milk of the human female approaches to the period of parturition, the more the ſeroſity is charged with calcareous phoſphate; and the more remote, on the contrary, the time is, from that moment, the leſs is the proportion of that ſubſtance, while the other nutritive parts of which it is compoſed are augmented in an inverſe ratio;—if we conſider that at the epocha of geſtation and parturition a diminution of ſolidity takes place in all the articulations of the mother, and a relaxation of the cartilages; that fractures of the bones at this time are moſt particularly ſlow in uniting by the formation of the calloſity; that it is at this very time of change in the oſſeous ſyſtem that the milk becomes charged with calcareous phoſphate, which it loſes in proportion as the infant and the mother become more removed from the time of the birth; ſo that the fluids which contribute moſt to the formation, the growth, and the nutrition of the fœtus, contain in themſelves the eſſential principle of ſolidity, the elements of oſſification; and laſtly, that it is not till this oſſification is very diſtinct, and the digeſtive organs of the infant are ſufficiently ſtrengthened to anſwer the purpoſes and the work of animalization, that the baſis diſappears from the milk of the mother;—we ſhall be compelled to acknowledge a particular direction of nature, by which the calcareous phoſphate becomes the matter of a peculiar ſecretion, eſſentially ordained to give firmneſs to our organs, and to conſolidate the firſt elements of man.

May theſe reflections impart to medical men a conviction how truly important the phyſical ſciences may become to their reſearches, though too often regarded as merely acceſſary to medical ſcience! May they ſee the advantage and neceſſity of ſtudying the progreſs of nature in theſe reſpects! For it is by theſe ſciences, and more eſpecially by the improvements of animal analyſis, which are rapidly advancing, that we may probably expect in a ſhort time to diminiſh the uncertainty of a large number of our methods of practice, and, to ſpeak freely, the too frequent ambiguity of our conjectures.

III.

Abſtract of a Memoir read at the Sitting of the French National Inſtitute on the 26th Pluvioſe, containing an Account of ſome Experiments concerning the Section which Cylinders of Camphor undergo at the Surface of Water; with Reflections on the Motions which accompany this Section. By J. B. VENTURI, Profeſſor of Natural Philoſophy at Modena, Member of the Inſtitute of Bologna, &c. *

ROMIEU formerly obſerved, that ſmall pieces of camphor have a progreſſive and rotatory motion upon water, and attributed this effect to electricity. Lichtenberg attributed it to the emanation of an ethereal ſpirit from the camphor itſelf. Volta produced the ſame motion of turning, by throwing upon water ſmall bodies ſoaked in ether, or particles of the acids of benzoin and amber. Brugnatelli found that the bark of aromatic plants moves on the ſurface of water like camphor. There was always a certain myſterious caprice in theſe motions, according to which they ſometimes could not be produced; and on other

* Annales de Chimie, XXI. 262.

occaſions,

the water, but cannot continue their motion becauſe the film which they form at the ſurface of the water is not diſſipated in the atmoſphere. From this laſt obſervation the Author deduces this conſequence, that volatility and the odorant property are not qualities neceſſary to produce theſe rotations. Volatility is merely neceſſary for their continuation.

At the end of his memoir, the Author ſpeaks of certain other motions obſerved in nature, which, by the mechaniſm of their cauſe, are in ſome reſpect analogous with the motions of oily bodies at the ſurface of water. In bodies brought near the fire, the humidity retires always to the extremities moſt remote from the fire itſelf; becauſe the vapour which is diſengaged from the part moſt heated repels the reſt in the oppoſite direction. So likewiſe drops of water, thrown on an ignited plate of metal, remain, and are agitated in the form of a ſphere; becauſe the vapour which is produced by the contact of the plate agitates them, and does not permit them to touch the metal. By the ſame principle, it is very eaſy to explain the motions of the tremella deſcribed by Adanſon and Corti, which had inclined ſome naturaliſts to rank this byſſus in an intermediate claſs between plants and vegetables. The tremella is a maſs of very minute fibres, which being ſuſpended in the water muſt be ſenſible to the ſlighteſt impreſſion. It is at preſent known, that plants, when expoſed to light, decompoſe the water in their veſſels, of which they ſeize the hydrogene, and extrude the oxigene in the form of gas, by an operation nearly the reverſe of animal reſpiration. If from one of the fibres of the tremella the gas iſſues on one ſide, the fibre itſelf muſt bend toward the oppoſite part. If, as in large trees, the gas iſſues from the tremella in the greateſt quantity from the ſide of the plant which is oppoſite the light, the tremella muſt be repelled towards the ſource of the light itſelf, to which it always tends when included in an opake veſſel, into which the light enters through a hole. It is not therefore neceſſary for the explanation of this fact, either that we ſhould ſuppoſe any degree of animality in the plant, or attraction at a diſtance between the oxigene and the light. In the ſame manner, the gas which is abundantly produced by the light of the day, becomes accumulated between the fibres of the tremella, and floats it to the ſurface of the water. The Author takes occaſion to expreſs his acknowledgment to Citizen Fourcroy, who afforded him the means of repeating the experiments in his laboratory, and who, by his excellent leſſons, informed him of the facts newly diſcovered ſince the communication between his country and France had been unfortunately interrupted.

ADDITIONS to the preceding MEMOIR. In a Letter from the AUTHOR to Citizen FOURCROY.

CITIZEN,

AS I underſtand that an extract of my memoir upon the ſection of camphor at the ſurface of water is intended to be printed, I requeſt you will add the following facts and reflections:

1. Dry camphor is very perceptibly volatilized under the weight of the atmoſphere, at the 50th degree of Reaumur (145 Fahrenheit). It melts at the 120th degree (302 Fahrenheit), and its volatilization is then extremely rapid. In the Torricellian vacuum it riſes even at the ordinary temperature of the atmoſphere. This vapour has very little elaſticity. It cryſtallizes along the ſides of the tube.

2. A column of camphor is cut aſunder much more ſpeedily at the ſurface of boiling

than of cold water. Camphor upon boiling water ſublimes in great abundance with the vapour of the water itſelf.

3. Camphor, when floating upon water, turns and is diſſipated by the contact of the oxigene, carbone, hydrogene, and azotic gaſes. Theſe two laſt afforded more rapid movements, and a more ſpeedy diſſipation. They likewiſe more readily diſſolve carbone, phoſphorus, and ſulphur.

4. When camphor is burned or heated on a float at the ſurface of water, if it touch the liquid, it gives a conſiderable motion to its ſupport; but if it do not touch the water, it remains motionleſs. The motion is not therefore produced by the ſimple emiſſion of volatilized particles from the camphor. An action likewiſe takes place on the part of the water.

5. This action appears to me to depend on the principle of the motions of bodies which float at the ſurface of water, and has been explained with remarkable perſpicuity by Monge. Of two ſmall pieces of paper, twiſted up and moiſtened, the one with the pure water, and the other with water well ſaturated with camphor, the firſt attracts and the ſecond repels camphor on the ſurface of water, which does not actually hold that ſubſtance in ſolution. Water conſequently has more attraction for ſolid camphor, than for the little portion which it has already diſſolved to ſaturation. It mounts along the ſolid piece, where it forms a curvilinear inclined ſurface. The ſmall portion which is diſſolved and ſaturated, deſcends along this inclined ſurface, and in its deſcent it repels backwards, by the laws of mechanics, both the ſurface itſelf, and the ſolid particle to which it adheres. This ſeparation of the diſſolved part accelerates the diſſipation of the ſolid piece, by affording a current of water conſtantly renewed. The atmoſphere abſorbs that part of the camphor which is already diſſolved and extended on the ſurface of the water; the evaporation being perhaps aſſiſted by a ſmall portion of the water itſelf.

6. If a drop of oil had no affinity with the ſurface of the water, it would lodge itſelf in a ſmall cavity; and, though more elevated than the ſurface itſelf, it would preſerve the globular form by its affinity of aggregation. But ſince it extends in a film over the water, the drop itſelf, or ſome of its principles, muſt neceſſarily be attracted in the ſame manner as fluids which riſe along the ſides of veſſels.

7. Air, ſtrongly impregnated with ether, or very hot exhalations of camphor, exerciſes on the ſmall floating particles at the ſurface of the water a repulſion ſimilar to that of oil, or of camphor diſſolved without heat in the water itſelf. The former are elaſtic fluids, and the latter ſimple liquids. They muſt not be confounded together.

I have perhaps dwelt too long upon a ſubject which ſeems rather to be matter of curioſity than immediate utility. But it is always a pleaſure to me to embrace an opportunity of expreſſing my eſteem and attachment to you.

J. B. VENTURI.

IV.

Analyſis of four Specimens of Steel; with Reflections on the new Methods employed in this Analyſis.

By Citizen VAUQUELIN *.

Preliminary NOTE *of the (French)* EDITOR.

THE ſteels which Citizen Vauquelin has analyſed were ſent to the Council of Mines numbered 864*a*, 864*b*, 977 and 1024. Theſe ſteels were produced at the forge of Remmelſdorff, ſituated in the canton of Groſs-Remmelſdorff, in the department of the Moſelle, at the diſtance of 5000 metres north from Bouzonville, and the left ſide of the Nied. The crude irons which are there refined come from the furnaces of Dilling and Betting, which, according to the report of Citizen Dietrich, obtain their iron ſtone from the communes of Greſaubach and Steinbach, as well as from the foreſt of Hommelſwald; and their calcareous flux (caſtine) from the woods of the commune of Merſching. The eſtabliſhment conſiſts of two fineries, one large hammer, two ſmaller, and a heating furnace. Citizen Soller, the proprietor, had eſtabliſhed before 1785 a manufactory of ſteel by cementation, which, after having been abandoned for ſeveral years, has again been put into activity. The cementing furnace contains, according to the account furniſhed by this citizen himſelf to the Council of Mines, about ſix thouſand weight of iron, and can be charged two or three times a month.

Citizen Dietrich, who viſited this eſtabliſhment in 1785 by order of Government, inſpected the proceſs, and the furnace of Citizen Soller, of which he gave the moſt advantageous account. By this report it is ſeen that Citizen Soller uſes coak for cementation †. We think it may be of advantage to tranſcribe in this place the report which was made on theſe ſame ſteels in 1786, by the Commiſſaries of the Academy of Sciences. It is thus by comparing the reſults of operations performed in manufactories under the inſpection of learned men, with thoſe afforded by the chemical analyſis, that we may hope to learn to what extent the nature and proportion of the conſtituent parts may influence the qualities of ſteel; and whether we may expect from chemical ſcience new and direct methods of aſcertaining what kinds are beſt appropriated to the different uſes of this ſubſtance in the arts.

Extract from the Regiſters of the Academy of Sciences, March 15, 1786.

ON the 15th of February laſt, Mr. Soller preſented ſome ſteel of cementation from his works at Remmelſdorff, in Lorraine, near Saar-Louis. The Academy has charged Mr. Vander Monde and myſelf to give an account of the ſame.

We muſt diſtinguiſh three kinds of ſteel, each of which poſſeſſes properties relative to the different uſes they are applied to.

1. Natural ſteel (acier de fonte) is obtained immediately from the crude iron. This ſteel is uſually unequal in its quality; ſubject to have cracks and ſcales; is leſs hard and brittle than the two other kinds; welds better, and is principally uſed for inſtruments of

---

* Journal des Mines, publié par le Conſeil des Mines par la République, N° XXV. 1.

† Houille. Doubtleſs as the fuel; not as the cementing compound. N.

huſbandry, common cutlery, and ſprings. As this kind of ſteel demands leſs expence in its fabrication, it is of courſe the cheapeſt. The greateſt quantity is imported (into France) from Germany.

2. The ſecond kind is ſteel of cementation. It preſents a more equal grain in its fracture, and takes a better poliſh than the former. It is alſo harder, more brittle, and requires more particular attention in the forging. This kind being more perfect, is of great uſe in all the circumſtances which require the qualities here mentioned.

3. Laſtly, the third kind is caſt-ſteel, obtained by fuſion of one of the other of the foregoing kinds. It is characteriſed by its uniformity of texture, and is deprived of thoſe ſcabrous places which are obſervable even in the ſteel of cementation. This third kind of ſteel is therefore ſuſceptible of the moſt beautiful poliſh, and is proper to form razors, lancets, ornamental work, wire-plates, laminating rollers, &c. The Engliſh have hitherto been almoſt totally in poſſeſſion of the manufactory of caſt-ſteel, which is the moſt valuable of any.

Four bars of cemented ſteel, manufactured by Mr. Soller, and choſen from among the ſteels of different qualities in his warehouſe at Paris, were ſubmitted to the following experiments:

With the firſt piece ſome hook-tools* for turning iron were made. They forged very well, the ſteel being very hard in the fire, and conſequently eaſy to be wrought. The hook-tools, on trial, proved very good. A graver for turning iron in the turn-bench was made out of the ſame piece. It was tried upon ſteel, and found to be as good as an Engliſh graver.

The ſecond piece was uſed to make a plane iron for metal. It ſucceeded perfectly well, and kept its edge in planing iron as long as thoſe which are made of Engliſh ſteel.

Out of the third piece, which was doubled into three, and welded upon a piece of iron, a carpenter's chiſel was made 26 lines in breadth. The ſteel welded perfectly, without requiring any particular care. The edge proved very good, and on compariſon with an Engliſh chiſel of the ſame dimenſions it was found to poſſeſs the ſame qualities.

A carpenter's chiſel, 14 lines broad, was made out of the fourth piece of ſteel. It welded extremely well, and proved to be as good as the foregoing inſtrument. The workmen eſtimate that a greater number of Engliſh chiſels would be found at the venders' below than above this quality.

Mr. Soller likewiſe ſent to the commiſſioners a ſample of iron, which was part of the tie of a faggot of ſteel, and manufactured out of a mixture of half its weight of old iron with the ſame quantity of pig-iron. This iron, from which the ſteel preſented to the Academy appears to have been made, is of the firſt quality. It cannot be broken without tearing. It extends under the hammer with the greateſt facility, whether heated or cold.

---

* The hook-tool is uſed by the turners of ſtrong metallic work; but from its ſteadineſs, and other good qualities, it well deſerves to be applied in many delicate operations with ſteel and braſs, by a numerous ſet of workmen in this capital who are entirely unacquainted with it. Its uſual form is that of a hook or claw, leſs than half an inch long, proceeding nearly at right angles from the ſide of a ſtraight ſtem, which laſt terminates below in a point. The cutting edge is at the extremity of the claw, and is variouſly figured, according to the nature of the work. By bearing the angular point on the Reſt of the lathe, the claw may be applied to the work with great power and ſteadineſs, while the perpendicular handle bears againſt the ſhoulder of the workman. The ſteel commonly uſed in London for this inſtrument is called ſheer-ſteel, from its mark. The edge ſtands very well againſt ſoft ſteel, when the tool is a little harder than a hard ſaw, or juſt capable of yielding to a good Lancaſhire file. If it be harder, there is a greater probability of its breaking. This kind of work is performed by a ſlow motion, with the conſtant application of water to the piece to prevent heating. It is obvious from theſe facts that tenacity is the requiſite moſt wanted here. N.

The ſamples of ſteel from Mr. Soller exhibited in theſe trials the qualities of the ordinary ſteel of cementation which we receive from England under the name of acier punte *. Like that ſteel, they may be twiſted while hot without cracking, but they cannot be compared with the fine Engliſh ſteels.

Mr. Soller has likewiſe communicated to us the details of his proceſs, and the deſign of his furnaces. We have every reaſon to believe that he may fabricate ſteel of an excellent quality. But to prevent the objections which might be made on the ſuppoſition that theſe pieces had been ſelected, he has drawn out the whole contents of a caſe of ſteel made by a new cementation. This ſteel will be ſent to us, with every atteſtation in form which can be deſired. We ſhall ſubmit it in like manner to different trials, and render an account to the Academy. In the mean time, we are of opinion, that the enquiries and the undertaking of Mr. Soller are among thoſe which the Academy will never fail to applaud.

Done at Paris, at the Academy, March 15, 1786. (Signed) VANDER MONDE AND BERTHOLLET.

I certify that the preſent extract agrees with its original, and the judgment of the Academy.

Paris, March 18, 1786. (Signed) CONDORCET.

SECTION I.

INTRODUCTION †.

THE analyſis of ſteels is one of the leaſt advanced and moſt difficult parts of chemiſtry, more eſpecially when the accurate proportions of conſtituent parts are required to be determined.

The agents commonly made uſe of to diſunite theſe principles do themſelves ſuffer part of their elements to eſcape, which re-act on thoſe of the ſteel, and put them into a ſituation which renders it difficult to form any eſtimate.

Thus, for example, we find, that when ſteel is diſſolved in the ſulphuric acid, diluted with water, the hydrogene gas which is developed, diſſolves, and carries with it a portion of carbone, the quantity of which varies from a great number of circumſtances.

The inſufficiency of the methods hitherto propoſed for acquiring this knowledge is well ſhewn from the reſults they have afforded to thoſe who invented them, and who were no leſs ſkilful in experiments than in the art of reaſoning.

It is well known that we are indebted to Bergman for the firſt analytical proceſſes with iron and ſteel; and that little addition has been made to them ſince. But theſe proceſſes are inaccurate, as we ſhall ſhew in the courſe of the preſent memoir, and as it alſo appears from the different reſults he obtained from the analyſis of various kinds of ſteel, the uſual properties of which did not indicate ſo great a difference.

---

* Is this ſpur-ſteel ? N.

† This word, and ſeveral other ſentences or ſubordinate titles, appear in the margin of the original as ſide-notes. I have inſerted them in the text. N.

It is admitted, ſince the valuable experiments of Bergman, and particularly thoſe of Berthollet and Monge, that ſteel differs from pure iron only from the preſence of a certain quantity of carbone, which is intimately combined; a proportion which perhaps may admit of ſome degree of latitude; but which, nevertheleſs, if above or below a certain quantity, affords either an over-cemented, brittle, and too fuſible kind of ſteel, or elſe ſteel which too nearly approaches to iron in its qualities.

If ſteel were merely and conſtantly a combination of carbone and iron, in determined quantities, it would be eaſy to aſcertain this point once for all; but it almoſt always happens, that ſteel contains ſilex, phoſphorus, and ſometimes mangneſe, of which the influence upon the qualities of the ſteel is not known, even on the ſuppoſition that ſteel is neceſſarily no more than a combination of iron and carbone.

If we admit that theſe different ſubſtances are not indiſpenſable to the conſtitution of ſteel, it muſt nevertheleſs be allowed that they will affect its properties by greater or leſs modifications, dependent on their own quantities reſpectively.

If, therefore, as it ſeems out of doubt, the various qualities of iron and ſteel ariſe from the ſeveral principles of which they are compoſed, and their reſpective proportions, it is equally intereſting to philoſophy and the arts to determine, by chemical experiments, what influence each of theſe principles may exert in the combination, and to find by ſimple, ready, and cheap experiments, in what uſes theſe metallic ſubſtances may be moſt advantageouſly employed. But in order to arrive at this deſirable termination, a more preciſe knowledge is required of the uſual properties of the known kinds of iron and ſteel, compared with the chemical nature of thoſe matters. There is not, however, any other method than that of examining the two means at the ſame time, by which we may ſucceed hereafter in eſtabliſhing and fixing by chemical eſſays the qualities of the kinds of iron and ſteel already uſed, and even thoſe which may hereafter be fabricated.

It may be eaſily conceived that this undertaking would demand a great number of experiments. For it not only requires a knowledge of the number and nature of the elements neceſſary for the formation of ſteel of the beſt quality, but likewiſe the proportions of theſe elements, which may be infinitely varied, and to determine beſides the modifications which may be produced by various quantities of ſubſtances not eſſential to the conſtitution of this metal.

In the mean time, till circumſtances ſhall permit the execution of this uſeful plan, I proceed to offer the reſults of the analyſis of four kinds of ſteel, the difficulties I have found in the progreſs of this work, the means I have uſed to overcome them, and the methods I have ſubſtituted, inſtead of ancient proceſſes, to aſcertain and meaſure the eſſential and accidental principles of the ſteel.

SECTION II.

STEEL, NO. 864—SMALL PIECE.

Experiment I. 576 grains, or 30,57 grammes, of this ſteel, reduced into filings, and diſſolved in ſulphuric acid, diluted with five parts of water, afforded 1,98 grains of black reſidue.

Experiment II. 100 grains, or 5,3 grammes, of the ſame ſteel diſſolved in the requiſite quantity of ſulphuric acid, produced 108 inches, or 2169 millimetres cube of hydrogene gas.

New Proceſs to ſeparate the Phoſphate of Iron from Steel.

Experiment III. The exceſs of acid contained in the ſolution No. 1. having been ſaturated with carbonate of pot-aſh, there were depoſited 19 grains, or about one gramme, of a white taſteleſs powder, completely ſoluble in muriatic acid.

This matter, boiled in a ſolution of cauſtic ſoda, aſſumed a deep red colour, and greatly diminiſhed in volume.

The liquor having been filtered, and mixed with concentrated muriatic acid, gave no ſign of effervеſcence, and formed, before and after its mixture with the muriatic acid, a white precipitate, on the addition of lime-water.

This calcareous depoſition, being waſhed and dried, was ſoluble in acids, and without producing any effervеſcence; whence we may conclude that it is phoſphate of lime, and conſequently that the ſteel No. 864, the ſmall piece, contains phoſphorus. Experiments ſtill more poſitive afford an inconteſtable proof of the truth of what is here advanced on the nature of this ſubſtance.

The Inconvenience of uſing the Sulphuric Acid to determine the Quantity of Carbone contained in Steel.

CHEMICAL experiments having demonſtrated that hydrogene, when it develops itſelf from the interior part of bodies which contain carbone in a very divided ſtate, diſſolves a certain quantity, which depends on the more or leſs elevated temperature, and the more or leſs rapid diſengagement of the gas, I had reaſon to preſume that the reſults offered by the experiment numbered 1, could not give an accurate expreſſion of the quantity of carbone contained in the ſteel. I therefore ſought another method, in which the iron diſſolved without the diſengagement of hydrogene gas ſhould afford the real or abſolute quantity of carbone which renders it ſteel.

New Method of accurately determining the Quantity of Carbone contained in Steel.

AS the ſulphureous acid poſſeſſes the double advantage of diſſolving iron without producing gas, and does not act upon the carbure of iron, it preſented this deſirable requiſite.

Experiment IV. I put into a bottle 288 grains, or 15,28 grammes, of ſteel, in fine filings, with about two pounds, or about 978,24 grammes, of diſtilled water, and I paſſed into this bottle ſulphureous acid gas, diſengaged from the ſulphuric acid by mercury *. When the ſulphureous acid had ceaſed to act on the ſteel, I carefully decanted the liquor with a ſiphon. I waſhed the black precipitate ſeveral times with diſtilled water. After deſiccation by the gentle heat of the ſtove, it weighed ſeven grains, or about 0,37 grammes.

We ſee that this quantity of carbure of iron is much more conſiderable than that of the firſt experiment. For 576 gave only 1,98, whereas, in the preſent experiment, I obtained 7 from 288. But on examining this matter, I found that it contained ſulphur in a ſtate of mixture. For by expoſing it on a heated metallic plate, it took fire, like ſulphur alone.

---

* We ſhall not in this deſcribe the phenomena which appear during this operation. They have been expoſed by Citizen Berthollet, and we have developed and extended them to other metals in a particular memoir on the metallic ſulphates. V.

To extract this combuſtible ſubſtance from the carbure of iron, I heated the mixture ſlightly with a ſolution of cauſtic pot-aſh; and having left the matter to ſettle, I decanted the fluid. The powder, then, after waſhing and drying, weighed no more than four grains, and gave no further ſigns of ſulphur by combuſtion *. This experiment evidently proves, that in the experiment No. 1, where ſulphuric acid was uſed, the greateſt part of the carbone was diſſolved, and carried off by the hydrogene gas, ſince 576 grains afforded only 1,98 grains of this matter; though the ſame quantity of ſteel with the ſulphureous acid afforded eight grains. This ſteel contains therefore 0,014 of its weight of carbure of iron.

Analyſis of the Carbure of Iron.

Experiment V. To aſcertain the quantity of carbone contained in the four grains of carbure of iron obtained in the experiment No. 4, they were ſubmitted to the action of fire in a ſmall porcelain ſaucer, under a muffle. As ſoon as the temperature was ſufficiently raiſed, it took fire, and left 1,9 grains of a grey yellowiſh maſs, which treated with the boiling muriatic acid became white, and was reduced to 0,44 grains.

The muriatic acid acquired in this operation a lemon colour. It afforded with very pure pruſſiate of pot-aſh a blue precipitate, and with ammoniac reddiſh flocks, which conſiſted of oxide of iron. The 0,44 grains of white matter inſoluble in the muriatic acid, preſented on examination all the characters of ſilex.

From theſe experiments it follows, that the ſteel No. 864, ſmall piece, contains phoſphorus, carbone, and ſilex, of which the proportion with the maſs of iron is eſtabliſhed in thouſandth parts in a table at the end of this memoir.

Eſſay to diſcover the Preſence of Mangan[e]ſe in the Steel No. 864, Small Piece.

Experiment VI. Bergman having announced the preſence of mangan[e]ſe in all the iron and ſteel he examined, it became a neceſſary part of my plan to examine whether this metal exiſted in the ſteel which formed its object. Accordingly I took a known quantity of the ſolution of 576 grains, (*Experiment No.* 1,) from which the phoſphate of iron had been ſeparated. I precipitated it by well ſaturated pruſſiate of pot-aſh, to which I had added a ſmall portion of acid, in order that there might be no precipitation of iron in the ſtate of oxide.

The waſhed precipitate was then boiled with muriatic acid a little diluted. The maſs of the matter was perceptibly diminiſhed, and the liquor aſſumed a purpliſh red colour; which phenomenon led me to preſume that this ſteel contained manganeſe. During the ſolution or rather the ebullition of the liquor, a very perceptible ſmell of pruſſic acid was emitted. The muriatic fluid, mixed with a ſolution of carbonate of potaſh, afforded a white precipitate, which became yellow by the contact of the air. This precipitate, dried by the ſtove, and expoſed to the action of heat, under a muffle, in the cupelling furnace, acquired a black colour. It was again diſſolved in the muriatic acid, and, upon the addition of pruſſiate of pot-aſh, afforded pruſſiate of iron as beautiful as at firſt. The ſecond precipitate, treated as before, and ſtrongly heated under a muffle, became at firſt black, but afterwards acquired a brown-red colour, which began to deſtroy the opinion I had conceived reſpecting the exiſtence of manganeſe.

---

* It appears that pot-aſh acts not only on the ſulphur, but that it diſſolves alſo a portion of iron. When an acid is poured into the fluid, there is found at firſt a black precipitate, which is ſoon followed by another white depoſition. This precipitate being collected and examined afforded ſulphur and perceptible traces of iron. The four grains of black precipitate obtained in this experiment are therefore carbone in a pure ſtate, or nearly ſo. V.

Nevertheleſs, to clear up this doubt, I applied the ſulphureous acid to the calcined precipitate, in the hope of diſſolving the manganeſe, if it ſhould exiſt, without touching the iron. But I was convinced that a ſmall portion of iron only had been taken up; for the ſulphureous acid afforded a blue precipitate by the pruſſiate of pot-aſh, and a deep green by cauſtic pot-aſh. This proves at the ſame time that the iron does not contain manganeſe, and that this method is inſufficient to demonſtrate its preſence; more eſpecially to ſeparate it from the iron. For, on the one hand, the muriatic acid decompoſes, or at leaſt diſſolves, the pruſſiate of iron; and, on the other, the ſulphureous acid likewiſe diſſolves the oxide of iron.

This proceſs being founded on the aſſertion of chemiſts, that the pruſſiate of iron is inſoluble and not decompoſed in acids; and that the pruſſiate of manganeſe, on the contrary, is eaſily diſſolved by the ſame agents;—if the laſt aſſertion be true, the preſent experiments prove that the firſt is falſe. It is nevertheleſs probable, that the weaker acids, ſuch as the acetous and ſulphureous, greatly diluted, may not have ſo evident an action on the pruſſiate of iron; and that they may be preferable to ſeparate it from the pruſſiate of manganeſe. But experiment has not yet decided in this reſpect, though it is certain that it is by no means a matter of indifference what acid is employed in this operation.

The Method of Bergman to ſeparate Manganeſe from Iron.

THE bad ſucceſs of this proceſs engaged me to follow another method propoſed by Bergman, in his Diſſertation on the White Ores of Iron, and other parts of his works, to ſeparate iron from manganeſe.

I took one part of the oxide of iron which had been precipitated from the ſolution of the ſame ſteel by the cauſtic alkali, and afterwards waſhed, dried, and calcined in the cupelling furnace. Upon this I repeatedly boiled the nitric acid to dryneſs, and calcining it in each operation. Laſtly, I treated it with the ſame acid in which a ſmall quantity of ſugar had been diſſolved. During this operation, a great quantity of nitrous gas was diſengaged, and the fluid aſſumed a brown colour. After evaporation to dryneſs, and waſhing of the reſidue, part was diſſolved in water. The ſolution afforded a beautiful blue precipitate by the pruſſiate of pot-aſh, green by cauſtic pot-aſh, and white by the carbonate of pot-aſh.

The ſulphuric acid, poured on the ferruginous ſolution, did not diſengage nitrous vapours, but a white fume, the ſmell of which conſiderably reſembled that of the acetous acid.

It is therefore proved, that the method propoſed by Bergman, and followed and repeated by chemiſts in general, is very faulty, becauſe it favours the ſolution of iron, and the acid does not, when applied to a mixture of this metallic gas and the oxide of manganeſe, act excluſively on the latter, unleſs by accident ſuch a quantity of ſugar ſhould be employed as might be capable of forming ſo much acid only as could diſſolve the oxide of manganeſe alone. But how is it poſſible to aſcertain this quantity, when the proportion of the two metals is unknown?

A New Method for ſeparating Manganeſe and Iron, and determining their Proportions.

THE uncertainty of the methods propoſed by chemiſts to aſcertain and determine the quantities of manganeſe contained in different kinds of iron and ſteel, induced me to ſeek another which combines ſimplicity and accuracy. I think I have obtained, in this reſpect, all the ſucceſs which can be deſired in a matter of ſuch importance.

For this purpoſe, a known quantity of iron crude or malleable, or of ſteel, ſuſpected to contain manganeſe, is to be diſſolved in ſulphuric acid, diluted with four or five parts of water. The ſolution is to be precipitated with the cauſtic alkali; and the precipitate, after waſhing, and drying in the air, is to be calcined under the muffle of the aſſayer's furnace. When the calcination is as much advanced as it can be by this method, the metallic oxide is to be weighed, and diſſolved in muriatic acid; and after having evaporated the exceſs of acid, a ſolution of carbonate of pot-aſh, well ſaturated with carbonic acid, is to be poured into the fluid diluted with water, until no more precipitate is afforded. The filtered liquor is then to be boiled; and if it contains the oxide of manganeſe, this metal will fall down in the form of a white powder, which is a true carbonate of manganeſe.

This proceſs is founded on the conſideration that the carbonate of pot-aſh, well ſaturated with carbonic acid, does not precipitate the ſalts of manganeſe, becauſe the quantity of carbonic acid contained in the pot-aſh neceſſary to the ſaturation of the acid, united to the oxide of manganeſe, is ſufficiently great to hold the latter ſubſtance in ſolution; whereas the ferruginous ſalts, when their baſis is highly oxided, are entirely precipitated by the ſame re-agents.

On this ſubject I have made many experiments, ſynthetical and analytical, upon very ſmall quantities of iron and of manganeſe, which have left me no doubt with regard to the accuracy of the method. But the manganeſe exiſts in iron in the metallic ſtate; and in the preſent experiment it is obtained in combination with oxigene and carbonic acid. It is neceſſary, therefore, to divide the maſs, after drying it in the air, by 2,5. Thus, if 100 parts of carbonate of manganeſe be obtained, the metallic part will be 100 divided by 2,5, that is to ſay, 40.

I have not yet had occaſion to apply this method to a very great number of ſpecimens of ſteel or iron; thoſe which I have hitherto examined have exhibited no traces of manganeſe. Whence I am ſtrongly inclined to think that Bergman, who found this ſubſtance in every kind of iron and ſteel, muſt frequently have miſtaken iron for manganeſe, particularly in the inſtance where he affirms that he obtained 30 per cent.

[*To be continued.*]

V.

Deſcription of an Artificial Rock Cryſtal produced in the Humid Way. By M. TROMMSDORFF, *Profeſſor of Chemiſtry at the Univerſity of Erfurt* *.

IT is well known that ſilex, either pure, or mixed with heterogeneous ſubſtances, and cryſtallized in beautiful tranſparent columns, conſtitutes rock cryſtal. The curious enquirer, who for the firſt time beholds a fine group of theſe cryſtals, will naturally be diſpoſed to aſk, What are the means employed by nature to produce this form? Does this power operate by the humid or by the dry way?

---

* Journal der Pharmacie fur Arzte Apotheker und Chemiſten, 2 Band. 2 St. pag. 76.

The ſecrets of nature are concealed behind a thick veil, which is ſeldom to be penetrated. Too often it happens, that when we think ourſelves in poſſeſſion of the object of our enquiry, we find ourſelves deceived by the varying powers around us, which exhibit new forms. The preſumptuous ſpirit of man becomes impatient; cool experimental inveſtigation is laid aſide; the powers of imagination are recurred to, and a region of chimerical images preſents itſelf. Vanity and indolence both conſpire to render the philoſopher a creator of hypothetical worlds. Under theſe circumſtances it is not ſurpriſing that a variety of diſcordant theories have been invented concerning the production of rock cryſtal; that imaginary reaſoning has been offered and received, and that no chemiſt has yet ſucceeded in the artificial production of this ſubſtance.

The extent of our deductions by analogy is, that ſilex, before its cryſtallization, muſt have been diſſolved in ſome liquid; but the queſtion is to determine which? In fire this is not credible, for ſilex is apyrous. In acids?—Except the acid of fluor there is no other which acts on this earth, and this acid is a ſolvent which would not have again been ſeparated.

Bergman has indeed aſſured us, that he found hard cryſtals having the form of rock cryſtal, in the fluate of ſilex; but as he did not carefully examine them, we may ſtill queſtion whether they were true rock cryſtal.

Achard likewiſe attempted to imitate nature, and endeavoured, as is well known, to compoſe even the precious ſtones, by means of carbonic acid; but it is alſo known that his notions were not confirmed, and that this celebrated man either deceived himſelf, or was deceived by others; for his excellent character abſolutely forbids the ſuſpicion of voluntary deception.

It is known that the alkalis are capable of diſſolving ſilex; it is alſo known that the acids only are capable of deſtroying this combination. The ſilex in that caſe, when the ſolution is weak, falls down. The precipitate is very fine and tranſparent. When the ſolution is more ſaturated, it aſſumes the form of a jelly, or appears like gelatinous condenſed flakes, but never in cryſtals.

Profeſſor Siegling the elder, my intimate friend, had prepared the liquor of flints after the ordinary proceſs, which he ſet aſide in a glaſs veſſel covered with paper. This fluid remained undiſturbed for eight years, when by accident his attention was directed to it, and he remarked a beautiful cryſtallization. On this occaſion he had the goodneſs to ſend me the veſſel.

It was a jar of greeniſh glaſs, capable of holding twelve ounces. The bottom was covered with a number of ſmall cryſtals, over which repoſed about two ounces of a very tranſparent fluid. This liquid was covered with a cryſtalline cruſt, ſo ſtrong that the veſſel could be inverted without any loſs of the fluid.

The jar was then broken, and the contents more particularly examined.

The cryſtals at the bottom were compoſed of ſulphate of pot-aſh, and cauſtic pot-aſh, in a cryſtallized ſtate. The fluid exhibited the appearance of a cauſtic lixivium; but which had attracted a ſmall quantity of carbonic acid, and ſtill contained a little ſilex. Hence it efferveſced, and depoſited ſilex when treated with acids. The cruſt was viſibly compoſed of two kinds of cryſtals. When boiled with water, a great part of theſe were diſſolved, and afforded, after the evaporation, carbonate of pot-aſh. The remaining cryſtals were tetrahedral pyramids in groups. Acids had no action upon them. *They were perfectly tranſparent, and ſo hard as to give fire with ſteel.* They diſſolved in four parts of deliqueſcent pot-aſh, and afforded a maſs of ſoluble glaſs, from which ſilex could be precipitated. The fluoric acid attacked them, and diſſolved a conſiderable part. They were therefore perfect rock cryſtals, produced by art. Part of the cruſt, which was leſs tranſparent, fell down during the boiling. It was ſiliceous earth, ſo hard and firm that it acted as a grinding powder on glaſs.

Though I am convinced that this obſervation affords us no light with regard to the proceſs of nature in the production of rock cryſtal, it has not given me leſs ſatisfaction, from the proof that it is poſſible at leaſt for us to imitate this natural production, though perhaps by different means.

I explain the origin of rock cryſtal in this glaſs nearly in the following manner: The liquor of flints was very weak, and ſuper-ſaturated with alkali. This diſengaged alkali inſenſibly attracted the carbonic acid from the atmoſphere, and formed in this manner a cruſt upon the liquor, which was at reſt, and was not therefore ſo much expoſed to the action of the carbonic acid. The alkali could not attract this laſt principle but with difficulty, and conſequently the ſilex was ſeparated very ſlowly in the form of a fine powder. Now as the moſt perfect repoſe obtained in this operation, the fine particles of the ſilex were not diſturbed in their attraction, which they exerted without impediment, and formed cryſtals. As ſoon as a double cruſt was formed of carbonate of pot-aſh and rock cryſtal, the carbonic acid could no longer penetrate to the liquid; whence followed a cryſtallization of part of the diſengaged alkali.

An admirer of hypotheſes might apply this obſervation to the formation of natural rock cryſtal. It would be ſufficient for this purpoſe to ſuppoſe that a ſoluble maſs of ſilex and ſoda was formed by any ſubterraneous revolution; that, by the preſence of moiſture, this maſs was converted into the liquor of flints, from which afterwards, by a ſlow attraction of carbonic acid, the rock cryſtal was depoſited.

But it will reaſonably be aſked, What has become of the ſoda in this operation? Perhaps it may have been neutraliſed in the courſe of time by other acids, or elſe (for it is eaſy to find ſupport for an hypotheſis) it may be changed into magneſia; and in this caſe the experiments of Oſburg and ſome other authors would preſent themſelves to be quoted. For my part, I prefer an avowal of my ignorance, and am diſpoſed to ſay with a certain poet, that "no mortal can penetrate the depths of nature, and few even the ſurface." I am nevertheleſs deſirous that this obſervation may engage other chemiſts to repeat the experiment, and examine more particularly into the facts. Circumſtances, perhaps, in part to me unknown, may have a marked influence on the ſucceſs of the experiment.

VI.

Geological Obſervations on North Wales *, *By Mr.* Arthur Aikin.

I. THERE are no proper volcanic productions to be met with in North Wales. By proper volcanic ſubſtances I mean aſhes, pumice, lava, and ſcoriæ, or ſemi-vitrified ſtones, ſuch as are the peculiar products of Etna and other acknowledged volcanos. A variety of porous ſtones may be found on Cader Idris, and Snowdon; and theſe have been miſtaken for cellular lava; they conſiſt, however, merely of decompoſed granite, porphyry, or toad-ſtone. Fragments of this laſt, indeed, I have found in the plain of Salop, ſo porous, and penetrated with carbonate of iron, as greatly to reſemble a ſlag.

II. The indefatigable Sauſſure, whoſe accurate reſearches into the poſition and nature of the Alps, and the other ſurrounding mountains, have deſervedly ranked him among the moſt illuſtrious and perſevering mineralogiſts, ſays, in the firſt volume of his Voyages dans les Alpes, "It is a general obſervation, with few exceptions, that, in the greater chains of mountains, the exterior ridges are of lime; the next contain ſlates; to theſe ſucceed the primitive ſtratified rocks, and then the granites." The relative poſition of the Welſh mountains tends to confirm a remark made among the Swiſs Alps. For if from the central ridge of the Snowdon chain (in which term I comprehend the whole mountainous extent of Caernarvonſhire from north to ſouth) we proceed to the Menai, it will be found that the primitive rocks in maſs, ſuch as the granites and porphyries, occupy the interior and higher peaks; to the ſide of theſe are applied the banks of primitive ſtratified rocks; then come the ſlates, which terminate in the lime-ſtone, which forms the bank of the Menai. The ſame gradation of ſtrata will appear, if, inſtead of the weſtern, we examine the eaſtern ſide of Snowdon; the variation is not indeed ſo ſudden, but perhaps on that very account is more intereſting, as the ſpecies and varieties of rocks are more numerous, and in larger maſſes. From the peak of Snowdon to Llanrwſt through Capel Cerig are found granite and porphyry in maſs, micaceous ſchiſtus, and other primitive ſtratified rocks; ſerpentine in large blocks and of extraordinary beauty, and hornblen ſlate mixed with veins and rocks of quartz: from the vale of Llanrwſt to Llangollen extend the ſlates, which are there circumſcribed by the lime-ſtone range already mentioned. The general diſpoſition of the mountains of North Wales may be deſcribed in a very few words. There are two ridges of primitive mountains extending nearly due north and ſouth, of which one is the Snowdon chain, and the other the Cader Idris chain (comprehending, beſides this mountain, the Arrans and other lofty peaks that overlook the ſouthern extremity of Bala-Pool). Owing to the near approach of the primitive and ſecondary mountains to the coaſt of Merioneth, the lime does not commence till near the port of Crickaeth; hence proceeding northwards in an interrupted line

---

[*] From "A Journal of a Tour through North Wales and Part of Shropſhire; with Obſervations in Mineralogy and other Parts of Natural Hiſtory." By Arthur Aikin. Small octavo, 231 pages. Printed for Johnſon, London, 1797.

This Journal is of much value for the intereſting ſcientific obſervations it contains, as well as for authentic accounts of the manufactories and mine-works in the diſtrict through which the tour was made. Its plan in ſome reſpect reſembles that of the celebrated De Sauſſure; like whom our author has given ſome animated ſketches of the grand romantic ſcenery of a mountain country. N.

along the ſhore, it arrives at Caernarvon. From this place it proceeds along the Menai, forming the eaſtern bank, as far as Bangor Ferry. Hence to Orme's-head it is cut off by the northern extremity of the Snowdon chain, which terminates in the bay of Conway, by the cliffs of Penmaen-mawr and Penmaen-bach. The lime recommencing in the lofty promontory of Orme's-head, continues the boundary of the coaſt as far as the mouth of the Dee; it then takes a weſterly direction, curving to the ſouth, as it paſſes by Holywell and the upper end of the vale of Clwyd to the Eglwyſeg rocks in Llangollen vale; then, paſſing due ſouth, it appears on the oppoſite ſide of the vale; is broken near Oſweſtry by the Ferwyn mountains, appears again at Llanymynech, and is at length ſtopped in its courſe by a line of primitive mountains ſtretching northwards out of Radnorſhire. The ſlates occupy the whole intermediate ſpace between the ridge of lime and the primitive mountains.

The primitive, ſecondary, and derivative mountains may in general be diſtinguiſhed by peculiarities in their form, as well as by their relative poſition; the primitive rocks are craggy, ſteep, and tending more or leſs to a peak, or ſlender-pointed ſummit; the loftieſt mountains are generally about the middle of the chain, which both commences and terminates in abrupt precipices: theſe, together with the inſulated peaks that are continually interrupting the outline of the chain, form a very ſtriking diſtinctive character. The plates to Mr. Pennant's Snowdonia will convey a clearer idea of this than the moſt laboured deſcription. Indeed it is but juſtice to obſerve, with reſpect to theſe engravings, that they are perfectly accurate repreſentations of the ſcenes which they profeſs to deſcribe.

The ſlates are diſtinguiſhable from the primitive mountains by their inferior height, by the evenneſs and almoſt ſquareneſs of the individual hills, and by the eaſy flowing though varied outline of the chains, ſuch as that of the Ferwyn mountains already deſcribed, Chap. III.

The lime and ſand-ſtone hills are conſiderably lower even than the ſlates; riſing in general very gradually at one extremity, and terminating abruptly at the other. The banks of ſand rock are however broader and rounder than the lime: where the lime is the hardeſt, its form is the moſt perfect; but as it becomes ſlaty, ſoft, and mixed with clay, it approaches nearly to the form of the ſlate-hills, as is remarkably the caſe in the ſouthern part of Wenlock-edge. The ſand-ſtone, too, where it contains but little iron and clay, being almoſt wholly compoſed of ſand and lime, reſembles moſt the limeſtone hills. This may be obſerved by comparing the difference of form between the red ſand rocks of Neſcliff, and the white freeſtone of Grinſhill.

III. I have already mentioned the beds of rounded pebbles that are to be found on the higheſt parts of the ſlate mountains. Their preſent ſituation could never have been that in which they were formed, for they conſiſt almoſt univerſally of porphyry, quartz, ſerpentine and other ſtony ſubſtances which lie in large maſſes, compoſing the primitive mountains: their rounded ſhape too, like that of the pebbles on the ſea ſhore, ſeems to intimate that they have been carried by the force of water to the places which they now occupy. Another circumſtance which appears to point out the quarter whence they originally proceeded is, that, in proportion to their vicinity to the primitive mountains, is their ſize: a circumſtance that might naturally be expected, ſince the further they were carried, the more would they be rounded and comminuted. Still, however, there is a difficulty attending this hypotheſis; namely, by what means could theſe rounded pebbles have been forced acroſs the many deep valleys that interſect the mountains in all directions, without firſt filling up the valleys themſelves? And, if this was the caſe, by what means were the valleys ſo entirely cleared of them afterwards, as they appear now for the moſt part to be? The difficulty, however, I think, is more apparent than real; for it ſeems highly probable, that at the time when theſe ſlate mountains were formed under the water, there were no valleys, but the whole maſs was one uniform bank, the valleys being afterwards formed by the rivers as the water ſubſided: by this not improbable ſuppoſition, as it appears to me, the objection is wholly removed. On deſcending from the ſlate rocks to the limeſtone and the derivative hills, the marks of ſubmerſion are more numerous and unequivocal; ſand, pebbles, ſhells, and other marine exuviæ, being found in conſiderable abundance. Following the example of moſt mineralogiſts, I might indeed have mentioned the exiſtence of lime as of itſelf a ſufficient proof of ſubmerſion: it appears to me, however, that there is by no means evidence ſufficient to ſupport the large aſſertion, that all the lime which forms ſo conſiderable a part of the ſurface of the earth, has been actually produced by the proceſs of animalization. The only fact that I recollect, which has any reference to this queſtion, would lead one to draw a directly contrary inference; it being well known that the eggs of hens are without ſhells, when care has been taken in the feeding of the bird to hinder its having acceſs to mortar or any other ſubſtance that contains a large quantity of calcareous matter. Theſe tokens of the preſence of water on the tops of mountains that are now 2000 feet above the level of the ſea, naturally lead me to enquire into the cauſe of this phenomenon, in explanation of which two hypotheſes have been ſtarted; one, that the continents were forcibly elevated to their preſent height above the ſea by ſucceſſive exploſions, as ſome of the Lipari iſlands are known to have been formed; the other, that the ſea has gradually, or ſuddenly, ſubſided to its preſent level, but that no alteration has taken place in the poſition of the mountains. The extreme ſcarcity of acknowledged volcanic productions ſeems to render the firſt ſuppoſition highly improbable; and it may alſo be objected to it, that an immenſe power, to which we ſee at preſent ſcarcely any thing analogous, is called into exiſtence to accompliſh that which the operation of common allowed cauſes would effect juſt as well. The grand difficulty is to account for the diſappearing of ſo great a quantity of water, nor indeed have I ever ſeen this explained in a tolerably ſatisfactory manner: it is eaſy to imagine vaſt chaſms in the earth into which the waters have retired; but of this there is no proof whatever: it is alſo contrary to the gradual decreaſe of the ſea, which, from the preſent appearance of the earth, and from hiſtorical records, appears probable.

IV. The Welſh primitive mountains in maſs contain no metals; copper, however, is found in ſeveral of the hornſtone ſtratified mountains, of which the Parys mine, and thoſe at Llanberris and Pont-aberglaſlyn are examples. In theſe mines the ore is for the moſt part yellow ſulphuret of copper: the green and blue malachites, or carbonates of copper, are found in limeſtone, as at Orme's-head and Llanymynech-hill; nor have I heard any inſtance of theſe two laſt mines furniſhing copper in any ſtate but that of carbonate. Carbonated copper is alſo found in the calcareous cement of ſand rocks, as has been already mentioned to be the caſe at Pym-hill and Hawkſtone in the plain of Shrewſbury. Lead and calamine, I believe, are not to be found in North Wales, in any of the primitive ſtratified rocks. Theſe metals are moſt frequently found in ſlate, with a matrix however of calcareous ſpar, as in the vale of Conway, at Llangynnog, and the Snailbeach mines; they are frequently alſo found in limeſtone as at Llanymynech and Holywell. Reſpecting the formation of the above-mentioned metals, it is not eaſy to give a tolerably probable opinion; it appears, however, that carbonate of copper is of conſiderably later formation than the ſulphuret, the former probably originating from the decompoſition of the latter, and deriving its acid from the carbonate of lime, in which it is found. It is not likely that the lead found in limeſtone was originally formed elſewhere, becauſe lead, even in ſlate rocks, lies in a matrix of calcareous ſpar; and eſpecially becauſe it does not form thin ſtrata between the ſtrata of lime, as is the caſe with copper, but it traverſes, in a ſtream, the ſeveral ſtrata without any alteration in the line of its direction; to which may be added, that ſulphuret of lead is the general ſtate in which the metal is found, both in the ſlate rocks and limeſtone, the carbonate being equally rare in both ſituations.

There is no coal found in North Wales between the primitive mountains and the ſlates: a very ſmall quantity is procured between the ſlates and limeſtone; but by far the moſt extenſive beds are between the limeſtone and the ſand rocks, as about Wrexham or Coalbrook Dale, or between theſe laſt and the alluvial hills, as round Wolverhampton.

V. From the above-mentioned circumſtances it would appear, that at ſome former period the ſea covered the whole of North Wales, of the preſent plain of Salop, and of Cheſhire, except a line of iſlands conſiſting of the Snowdon chain, another to the ſouth conſiſting of the preſent Cader Idris chain, and a few detached rocks, ſeveral leagues to the eaſt, which now form the tops of the Wreakin, Caer Caradoc, and Stiperſtones. Under this primitive ſea, and prior to the exiſtence of animals or vegetables, the vaſt banks of ſlate appear to have been formed. My reaſons for thinking that animals and vegetables had as yet no exiſtence are, becauſe there was no ſoil upon theſe hard inſulated rocks for the growth of plants upon which the animals might feed, and becauſe we meet with no impreſſions or remains of organized bodies in the primitive mountains or banks of ſlate. By ſlow degrees the water ſubſided; being poſſibly in part abſorbed by the earth, in part fixed in a ſolid form in the ſlates, and in part decompoſed, forming oxygene and hydrogene: the former of which conſtituted the oxygenous baſe of the lower atmoſphere; the latter, by its ſuperior levity, riſing above the atmoſpherical air, according to the opinion of ſome philoſophers, forms a vaſt ſtratum many miles above the ſurface of the earth; whence originate meteors, the aurora borealis, and other ſimilar appearances.

The water having for the moſt part retired from the beds of ſlate, the greater part of Wales, and the ſecondary hills of the Engliſh counties weſt of the Severn, would form one or more conſiderable iſlands, ſeparated from the ſmall part of England then above water by a wide channel occupying the flat part of Cheſhire and Shropſhire, and the preſent vale of the Severn from Coalbrook Dale to the Briſtol Channel. At this period I imagine the ſecondary limeſtone hills to have been formed. The deſiccation of the water ſtill continuing, the tops of the limeſtone ridges themſelves would begin to appear above the ſurface; and then the plain of Salop, the flat part of Cheſhire, and the ſouthern extremity of Lancaſhire, would form one vaſt bay; into which the Severn, Dee, and Merſey emptied themſelves, flowing into the ſea by an united ſtream, filling the preſent mouths of the two latter rivers, and the intermediate ſpace, the hundred of Wirral. Into this bay or eſtuary a large quantity of ſand would be conſtantly poured in by the violent weſtern winds, and the currents of the three rivers not being able entirely to clear it away, banks of ſand would be by degrees formed, conſtituting the preſent ſand and free-ſtone rocks extending from Neſcliff eaſtward by Pym-hill and Grinſhill to the hills of argillaceous ſchiſtus round the Wreakin. This accumulation of ſand would prevent the free egreſs of the waters of the Severn into the main bay, which by degrees, or more probably at once, urged by a ſtrong weſt wind, and ſwelled unuſually by rains or ſnow, broke through the lime-ſtone rock at Coalbrook Dale, and ruſhed into the Channel, which it has ever ſince flowed in. The banks of ſand, that almoſt entirely ſhut out the Severn from the bay of Cheſhire, prevented the Dee from deviating from its original courſe, and the further decreaſe of the water added conſtantly to the difficulty. A number of particulars might be mentioned in confirmation of the foregoing hypotheſis, derived from the appearance of the hills, the ſoil, and other circumſtances, were it my intention to enter minutely into the ſubject; but this could not be done without the aid of charts and engravings, in a manner capable of intereſting the attention of any, except thoſe who have viſited and carefully obſerved the tract in queſtion.

The ſea, however, has not been uniformly receding; for ſome time paſt it appears to have been advancing upon the Welſh coaſt: a brief enumeration of the proofs of this will conclude the ſubject.

The coaſt of Cardigan from Aberyſtwith northward, if it does not furniſh any direct proof of the advance of the ſea, yet ſhews at leaſt that the water is not retreating, from the circumſtance of there being no beach at high tide, and the many caverns and receſſes in the ſlate rocks on the coaſt that are every day filled by the ſea. The ſouthern part of Merionethſhire exhibits certain proofs of the progreſſive ſtate of the ſea, in the vaſt banks of peat already mentioned, which extend along the ſhore to Tovyn, and ſtretch to an unknown diſtance into the water. From near Harlech a long range of ſand and gravel, including Traeth-mawr and Traeth-bychan, runs twenty-two miles into the ſea, being called at preſent Sarn Badrig, or the Ship-breaking Cauſeway; the whole of which tract, formerly called Cantrer Gwaelod, or the Lowland Hundred, was about the year 500 overwhelmed by an inundation, occaſioned by the careleſſneſs of thoſe who kept the flood-gates, as is mentioned in an extant poem of Talieſſin. Northward of the town of Abergeley in Denbighſhire, a vaſt extent of inhabited country is ſaid to have been deſtroyed by the ſea; in proof of which an epitaph without date or name in Abergeley church-yard is cited, ſignifying that the perſon to whoſe memory the monument was erected lived three miles to the north. A more deciſive evidence is furniſhed by Mr. Pennant in his Snowdonia. "I have obſerved," ſays he, "at low water, far from the clayey banks, a long tract of hard loam, filled with the bodies of oak trees, tolerably entire, but ſo ſoft as to be cut with a knife as eaſily as wax." Finally, I have obſerved on the Lancaſhire coaſt, a few miles north of Liverpool, the beach overſpread with trunks and branches of oak-trees; the whole ſhore to a conſiderable diſtance inland, being a peat moſs, now for the moſt part covered with ſand; the extent of the moſs along the ſhore is very evident by the almoſt blood-colour of the beach, occaſioned by the boggy iron ore with which the water that oozes out of the peat is highly impregnated. From theſe facts it may, I think, be fairly inferred, that moſt of the preſent ſands which border the coaſt of North Wales and Lancaſhire, were formerly foreſts or cultivated land; and that the ſea is at preſent, and for theſe twelve or thirteen centuries paſt has been, gaining upon the ſhore.

VII.

An Account of the Fata Morgana; or the Optical Appearance of Figures, in the Sea and the Air, in the Faro of Meſſina. With an Engraving.

> As when a ſhepherd of the Hebrid' Iſles
> Placed far amid the melancholy main,
> (Whether it be lone fancy him beguiles,
> Or that aërial beings ſometimes deign
> To ſtand, embodied, to our ſenſes plain)
> Sees on the naked hill, or valley low,
> The whilſt in ocean Phœbus dips his wain,
> A vaſt aſſembly moving to and fro:
> Then all at once in air diſſolves the wondrous ſhow.
>
> THOMSON.

VARIOUS philoſophical writers and travellers, and among them our Engliſh travellers Brydone and Swinburne, make mention of a very ſtriking phenomenon which occaſionally appears in the Straits of Meſſina, and is known by the name of Fata Morgana, or, as ſome render it, the Caſtles of the Fairy Morgana. The accounts differ from each other, as well with reſpect to the appearances, as the concomitant circumſtances which are ſuppoſed to be neceſſary for producing them. How far the effects themſelves may be ſubject to variation, or to what extent the imagination of the narrators, who ſpeak of the exhibition as calculated to produce aſtoniſhment, may be ſubject to irregularity, would admit of diſcuſſion; but the general certainty of the events is matter of univerſal notoriety, and admits of no doubt. I have not had the good fortune to meet with any of the authors who treat on this ſubject expreſsly from their own knowledge and obſervation, till lately that the Diſſertation of Minaſi[*] was lent me by the Right Honourable Sir Joſeph Banks, Bart. &c. In this treatiſe the facts are related with much ſimplicity and preciſion, and the philoſophical reaſoning of the author is kept diſtinct from the narrative. I have therefore choſen to collect the preſent account from this author. The engraving, plate X. is copied from the ſame work.

His firſt chapter contains a deſcription of the phenomenon. "When the riſing ſun ſhines from that point whence its incident ray forms an angle of about forty-five degrees on the ſea of Reggio, and the bright ſurface of the water in the bay is not diſturbed either by the wind or the current, the ſpectator being placed on an eminence of the city, with his back to the ſun and his face to the ſea;—on a ſudden there appear in the water, as in a catoptric theatre, various multiplied objects, that is to ſay, numberleſs ſeries of pilaſters, arches, caſtles well delineated, regular columns, lofty towers, ſuperb palaces, with balconies and windows, extended alleys of trees, delightful plains with herds and flocks, armies of men on foot and horſeback, and many other ſtrange images, in their natural colours and proper actions, paſſing rapidly in ſucceſſion along the ſurface of the ſea during the whole of the ſhort period of time while the above-mentioned cauſes remain.

---

[*] Diſſertazione prima ſopra un Fenomeno volgarmente detto Fata Morgana: O ſia apparizione di varie, ſucceſſive, bizzarre immagini che per lungo tempo ha ſedotti i popoli, e dato a penſare ai dotti. A ſua Eminenza il Signor Cardinale de Zelada. Del P. Antonio Minaſi Domenicano. In Roma 1773.

“ But if, in addition to the circumſtances before deſcribed, the atmoſphere be highly impregnated with vapour, and denſe exhalations not previouſly diſperſed by the action of the wind or waves, or rarefied by the ſun, it then happens that in this vapour, as in a curtain extended along the channel to the height of about thirty palms, and nearly down to the ſea, the obſerver will behold the ſcene of the ſame objects not only reflected from the ſurface of the ſea, but likewiſe in the air, though not ſo diſtinct or well defined as the former objects from the ſea.

“ Laſtly, if the air be ſlightly hazey and opake, and at the ſame time dewy and adapted to form the iris, then the above-mentioned objects will appear only at the ſurface of the ſea, as in the firſt caſe, but all vividly coloured or fringed with red, green, blue, and other priſmatic colours.”

The author therefore diſtinguiſhes three ſorts of Fata Morgana: that is to ſay, the firſt at the ſurface of the ſea, which he calls the Marine Morgana; the ſecond in the air, called the Aërial Morgana; and the third only at the ſurface of the ſea, which he calls the Morgana fringed with priſmatic colours.

In a note in this chapter P. Minaſi enquires into the etymology of Morgana. After various remarks, he thinks the opinion of thoſe who derive this word, which is ſo foreign to the Roman idiom, from μωρος triſtis and γανόω lætitiâ afficio, is not far from the truth, conſidering the great exultation and joy this appearance produces in all ranks of people, who on its firſt commencement run haſtily to the ſea, exclaiming Morgana, Morgana! He remarks that he has himſelf ſeen this appearance three times, and that he would rather behold it again than the moſt ſuperb theatrical exhibition in the world.

In the ſecond chapter the author deſcribes the city of Reggio, and the neighbouring coaſt of Calabria; by which he ſhews that all the objects which are exhibited in the Fata Morgana are derived from objects on ſhore.

In his third chapter, conſiſting of phyſical and aſtronomical obſervations, he affirms that the ſea in the Straits of Meſſina has the appearance of a large inclined ſpeculum; that in the alternate current or tide which flows and returns in the Straits for ſix hours each way, and is conſtantly attended by an oppoſite current along ſhore to the medium diſtance of about a mile and a half, there are many eddies and irregularities at the time of its change of direction; and that the Morgana uſually appears at this period. Whence he enters into conſiderations of the relative ſituations of the ſun and moon, which are neceſſary to afford high water at the proper time after ſun-riſe, as before deſcribed. It is high water, that is to ſay, the northern current ceaſes, at full and change, at nine o’clock. There is probably a ſmall riſe and fall, though the annotation to a large chart before me affirms that there is none.

In the fourth chapter and ſubſequent part of the work, the author collects the opinion and relations of various writers on this ſubject, namely, Angelucci, Kircher, Scotus, and others; and he afterwards proceeds to account for the effects, by the ſuppoſed inclination of the ſurface of the ſea, and its ſubdiviſion into different plains by the contrary eddies. The aërial effects are referred to conſiderations of ſaline and other effluvia ſuſpended in the air, which I forbear to abridge, becauſe it ſeems difficult to make any clear or productive ſtatement either from the narrative or the reaſoning.

What I ſeem to collect upon the whole from the ſeveral relations, and the curious print which I have copied, is: 1. That by the ſituation of the Faro of Meſſina, the current from the ſouth, at the expiration of which this phenomenon is moſt likely to appear, is ſo far impeded by the figure of the land, that a conſiderable portion of the water returns along ſhore. 2. That it is probable the ſame coaſts may have a tendency to modify the lower portion of the air in a ſimilar manner, during the ſouthern breeze; or, in other words, that a ſort of baſon is formed by the land, in which the lower air is more diſpoſed to become motionleſs and calm than elſewhere. 3. That the Morgana Marina preſents inverted images below the real objects, which are multiplied laterally as well as vertically; and that there are repetitions of the ſame multiplied objects at more conſiderable vertical intervals. This I gather from the appearance of the dome and other objects in the plate. 4. That the Aërial Morgana is not inverted, but, as I am diſpoſed to conjecture, is more elevated than the original objects. 5. That the fringes of priſmatic colours are produced in falling vapours, ſimilar to many appearances which have been deſcribed by authors, but not accurately explained by the general principles of refraction through ſpheres of water. The ſhip is referred to by the author as an object ſurrounded by theſe fringes: whence it appears that the colours apply to the direct rays from objects, as well as to thoſe of the Marine Morgana. 6. Various other objects in the drawing, as well as in the deſcription, afford matter for queſtion and conjecture, but none perhaps which it may be proper to enlarge upon, until the theory be better known. 7. It ſeems at all events more probable that theſe appearances are produced by a calm ſea, and one or more ſtrata of ſuperincumbent air differing in refractive, and conſequently reflective power, than from any conſiderable change in the ſurface of the water, with the laws of which we are much better acquainted than with thoſe of the atmoſphere. 8. By attentive reflection upon the facts and reaſonings in Mr. Huddart's paper *, we may form a theory to account for the erect and inverted images: the poliſhed ſurface of the ſea may perhaps account for the vertical repetition; but for the lateral multiplication we muſt have recourſe to reflecting or refracting planes in the vapour, which appear nearly as difficult to deduce or eſtabliſh, as thoſe which have been ſuppoſed on the water.

VIII.

A Memoir containing ſome Reſults ariſing from the Action of Cold on the Volatile Oils, and an Examination of the Concretions found in ſeveral of thoſe Oils. By Cit. MARGUERON, Member of the Societé des Pharmaciens at Paris.

[Concluded from page 186.]

An Examination of the Concretions found in ſeveral Volatile Oils.

THE volatile oil of fennel-ſeed, which had been long made, had depoſited a concretion of a lamellated form. The oil was of an amber-colour, and ſmell ſcarcely perceptible. It was fluid, and no longer capable of aſſuming the concrete ſtate at the temperature of four or five degrees below congelation. The concrete matter being ſeparated was yellowiſh, and had the taſte and ſmell of fennel. When expoſed to the air it became dry and friable between the fingers, like a reſin. When placed between two capſules of glaſs, and expoſed to the hot wood-aſhes, it melted and ſublimed in needles. On ſuſpending the operation there remained in the capſule a brown matter, which by cooling cryſtallized in a ſtriated maſs. The needles obtained by ſublimation were white and ſilky, with the taſte and ſmell of fennel. On expoſure to the flame of a candle they liquefied without taking fire, and by cooling they aſſumed a concrete tranſparent ſtate. They were inſoluble in the nitric acid; but their ſolution in alcohol reddened the tincture of tournſole; and when ſubmitted to evaporation it left a cryſtallized matter in ſilky needles.

---

* Philoſophical Journal, I. 145.

Volatile oil of wormwood, not rectified, and preſerved ſeveral years in a bottle with the diſtilled water of the ſame plant, had depoſited a yellowiſh matter; the oil had little fluidity; its colour was of a deep green, and its ſmell conſiderably pungent; the depoſited matter was yellowiſh, and emitted the ſmell of wormwood; when applied to the tongue it communicated the peculiar taſte of the plant; it adhered to the fingers, like turpentine; expoſure for ſome days to the air did not augment its conſiſtence; when it was afterwards preſented to the flame of a candle, it took fire, and was covered with ſmall needles. This matter cryſtallized by cooling, and became dry and brittle, like reſin.

The water on which the oil had been preſerved very ſpeedily reddened the tincture of tournſole.

The volatile oil of ſage, diſtilled long before, had depoſited a concretion which lined the bottom of the bottle in which it was preſerved. This concretion was white, and when examined by the magnifier appeared brilliant, cryſtallized, and laminated. Before I proceeded to examination I compreſſed it between ſeveral folds of the paper Joſeph, to deprive it of the oil which might be interpoſed between its parts. This concretion, expoſed to the air, became dry and friable; its ſolution in alcohol became white by the addition of water. Part of the ſolution, ſubjected to ſpontaneous evaporation, left in the capſule a whitiſh covering, and ſome ſmall needles. Expoſed to the flame of a candle it did not take fire, but liquefied, and became firm by cooling, and of a reſinous appearance. When placed between two capſules, and ſubjected to a gentle heat, it melted, but only a few particles ſublimed. When it was treated comparatively with the camphor of the ſhops by the nitric acid, and with the ſame degree of heat, the concretion was liquefied, aſſumed the conſiſtence of turpentine, and became much coloured: the camphor, on the contrary, entirely diſſolved in the nitric acid, and was recovered again with its properties by the addition of water. The nitric acid which I heated with the concretion, did not afford the ſame reſult when treated with a like quantity of water.

From theſe experiments we may conclude, that the concretions obſerved in the ſeveral volatile oils, approach rather to the nature of reſins with a ſuperabundance of acid, which form a kind of ſalt ſimilar to the flowers of benzoin* rather than camphor, to which they have frequently been compared. For, if we compare the effects of the nitric acid on theſe concretions, the foregoing obſervations ſhew that its action upon them is very different from its action upon camphor.

---

* In 1792, Citizens Deyeux and Vauquelin informed us, that the concretions depoſited by cinnamon-water had the properties of the acid of benzoin. M.

That the concretions were not camphor, is no doubt aſcertained by the different action of nitrous acid; but how far they may agree with the acid of benzoin, ſeems to require further proof. It is generally agreed, from the experiments of Lichtenſtein and others, that the acid of benzoin is ſoluble in the vitriolic and nitrous acid, and ſeparable by the addition of water, without alteration of its properties, and that it is not readily altered by digeſtion or abſtraction of the latter acid. N.

IX.

On the Cold Winds which iſſue out of the Earth. By Profeſſor DE SAUSSURE *and others; with Obſervations.*

IN the fifth volume of De Sauſſure's Voyages dans les Alpes, page 342, lately publiſhed, the author, after having aſcertained by experiment that the water at the bottom of the Alpine Lakes is much colder than temperate, namely, about 4 degrees of Reaumur, while that of the ſurface indicated about 15°, whereas the temperature of the water at 886 feet and at 1800 feet depth in the Gulph of Genoa, near the land, proved to be above temperate, namely, 10,6 degrees, while at the ſurface the heat was 16½ degrees, proceeds to make remarks on a phenomenon probably of the ſame nature, that there are many ſubterraneous cavities out of which winds iſſue, which are colder than the mean temperature of the earth. The former effects are conſidered by this author as very difficult to be accounted for; and indeed they appear to depend on the principle of the almoſt perfect non-conducting power of fluids with regard to heat, which has ſcarcely, if at all, been treated by any author but Count Rumford in his ſeventh eſſay, publiſhed a few days ago. The latter he thinks may more eaſily be explained. I ſhall here relate the facts by a tranſlation nearly cloſe, and afterwards give the ſubſtance of his reaſonings, with ſuch remarks as may preſent themſelves.

He begins his account with the caves of Mont Teſtaceo near Rome, which were the firſt that fixed the attention of an accurate obſerver. The Abbé Nollet obſerved them in his travels into Italy*, and found their temperature was 9½ degrees on the 9th of September 1749, in the afternoon, while the thermometer in the open air ſtood at 18 degrees; and he remarks with reaſon, that their coolneſs is ſo much the more aſtoniſhing, as they are not deep; that the entrance has ſcarcely any deſcent, and that the ſun ſhines for the greateſt part of the day upon the door of the entrance.

M. De Sauſſure found them cooler than the Abbé Nollet did, though he viſited them in hotter weather; the reaſon of which, as he remarks, may be deduced from the explanation of the phenomenon. On the firſt of July 1773, the external air in the ſhade was at 20½ degrees; that of one of the caves was at 8; of another at 5⅔; and of a third at 5¼. Theſe caves are made in the ſide of the mountain, and occupy nearly its whole circumference. Thoſe he entered were on the weſt ſide. The walls at the bottom have perforations through which the cold air enters.

The air itſelf comes through the interſtices left between the pieces of broken urns, amphoræ, and other veſſels of pottery of which this ſmall mountain appears to be entirely compoſed. He went to its ſummit, which is not above two or three hundred feet in height, and every where obſerved theſe fragments, which he does not doubt were formerly collected in this place by ſome public order. At preſent the police maintains them; for theſe caves are ſo uſeful and valuable, and there is ſo much apprehenſion leſt their quality ſhould be altered, that every excavation, and even the cultivation of the ground on this ſmall mountain, is prohibited. And it is truly a very ſingular phenomenon, that in the midſt of this diſtrict, of which the air is ſo hot and ſtifling in ſummer, there ſhould be found a ſmall iſolated hill, from the baſe of which currents of air of extraordinary coolneſs ſhould iſſue on all ſides.

---

* Acad. des Sciences, 1749, page 486.

It is not leſs ſingular, that under a ſtill more ſouthern climate, and in an iſland like that of Iſchia, which is entirely volcanic, and every where abounding with hot ſprings, a cold ſubterraneous wind ſhould be found like that here deſcribed. Sir William Hamilton aſſured our author, that there is a ſimilar grotto at Ottiano, at the foot of Veſuvius. Theſe grottos have even an appropriate name; they are called Ventaroles. That of Iſchia is called Ventarola della Funera. It is beneath a ſmall chapel dedicated to St. Anthony, which is itſelf beneath the Caſa Monella. On the 9th of March 1773, the thermometer in the open air in the ſhade out of the grotto ſtood at 14 degrees, and that which M. De Sauſſure placed at the bottom of the grotto ſtood at 6 degrees; and he was aſſured that in the ſummer, during the great heats, he would have found it much lower.

The cold caves of St. Marino are at the foot of a hill of grit-ſtone, on which the capital of this ſmall republic is built. On the 9th of July 1773, about three in the afternoon, the thermometer in the open air ſtood at 13 degrees; and in the caves at 6. The floor of theſe caves lies between 320 and 330 toiſes above the level of the ſea.

The caves of Ceſi are ſituated in the town of the ſame name, which is ſix miles north of Terni, in the Eccleſiaſtical State. That inſpected by M. De Sauſſure was in the houſe of Don Giuſeppe Ceſi. The cold of this cave, like the preceding caves, does not proceed from its depth, but from a cold air which iſſues from the crevices of a rock againſt which it is built. This air iſſued out with ſo much force at that time, that it almoſt extinguiſhed the flambeaux which enlightened them; and the proprietor aſſured our author, that the wind would have been much ſtronger, if the weather had not been cold, as it was for the ſeaſon. In the winter, on the contrary, the wind ruſhes violently in, and the more ſo the colder the weather. This is expreſſed in the Latin verſes which the maſter of the houſe ſhewed him:

Abditus hic ludit vario diſcrimine ventus,
  Et faciles miros exhibet aura jocos:
Nam ſi bruma riget, quæcumque objeceris haurit;
  Evomit æſtivo cum calet igne dies, &c.

The maſter of this houſe derives great advantage from the coolneſs of this cave, not only by preſerving wines, fruits, and proviſions of every kind, but likewiſe by conducting this cool air by pipes into the apartments. Cocks, at the extremity of theſe tubes, emit the cold air in ſuch quantities as may be deſired. This refinement is carried ſo far as to convey the air under certain ſtands, the foot of which is pierced; ſo that bottles placed on theſe ſtands are continually cooled by the wind which iſſues forth. On the day when M. De Sauſſure meaſured the temperature of this ſubterraneous wind at the entrance of the ſmall cavern whence it iſſued, he found it at 5¾ degrees, while the external air was at 14½ degrees. This was on the 4th of July 1773, in the afternoon; whence, in fact, it appears that the day was very cold for that climate and ſeaſon.

The Cantines, as they are called in Italian Switzerland, or the cold caves of Chiavenna, are likewiſe ſituated againſt a rock to the ſouth-eaſt of the town. The cold air enters into the caves through the crevices of this rock, of which the compoſition is an indurated ſteatites, intermixed in various places with aſbeſtos and flexible amianthus. On the 5th of Auguſt 1777, at noon, the thermometer ſtood in theſe caves at 6 degrees, while in the open air it was at 17.

On this occaſion our author remarks, that the ſtones which compoſe the mountains whence the cold winds iſſue, are very different in their nature; which, as he obſerves, affords an anſwer to the queſtion of the Abbé Nollet, relative to the caves of Mont Teſtaceo, "Is pottery of a nature to be more difficultly heated than other materials; or do the influences of the atmoſphere cauſe refrigeration, which would not take place elſewhere?" It is certain, adds M. De Sauſſure, that this phenomenon does not depend on the nature of the pottery; for the cold winds of Ceſi iſſue from a calcareous mountain, thoſe of St. Marino from grit-ſtone, thoſe of Chiavenna from a ſteatites, &c.

The caves in which M. De Sauſſure found the air coldeſt, and which he moſt carefully obſerved, were thoſe of Caprino, near the lake of Lugan, and oppoſite that agreeable ſmall town of Italian Switzerland. Theſe caves are ſituated at the foot of a calcareous mountain, whoſe very rapid ſlope terminates near the lake.

Before your entrance, you are deſired to remark the cold wind which iſſues from the key-hole of the door, which is ſenſible even at ſeven or eight inches diſtance. On your entrance, the ſenſation of cold is very ſtriking, ſo as even to produce an apprehenſion of inconvenience; and on coming out, you ſeem to enter an oven. In the firſt viſit to theſe caves (June 29, 1771), the author found the thermometer at the bottom of the cave to be 2⅓ degrees; while in the ſhade in the open air it was at 21. The ſecond time that he viſited them, namely, on the 1ſt of Auguſt 1777, the thermometer deſcended no lower than 4½ degrees; while in the air it ſtood at 18.

What he thinks remarkable in theſe caves is, that they are not deep, nor hollowed into the earth. Their floor is on a level with the ground; the external wall and the roof are entirely expoſed to the open air. There is only the wall at the bottom, and part of the lateral walls, which are within the foot of the mountain. This foot is every where covered with angular fragments of the ſame mountain, and it is from among theſe fragments that the cool air iſſues. By a fortunate chance, he ſaw one of theſe caves conſtructed. The maſon who overlooked the work, affirmed that he was in poſſeſſion of the art of finding proper ſituations; and that it was neceſſary to ſeek for thoſe places whence the wind iſſued out, and to bore apertures correſponding to thoſe places. It is by theſe ventiges that the caves are cooled, as may be eaſily perceived by applying the hand; and againſt theſe it is that the thermometer muſt be placed to find the loweſt temperature.

It is aſſerted that the country is indebted to the ſheep for this diſcovery. A ſhepherd obſerved, that during the great heats his ſheep all repaired to certain places in preference, and applied their noſes to the earth. He endeavoured to aſcertain the reaſon of this preference, by applying his hand to the ground. The cool air, which, even without any conſtruction, is perceptible, induced him to build a cave.

In the cave which M. De Sauſſure ſaw built, there was only the interior wall raiſed, ſo that its external face was, at that time, abſolutely in the open air; nevertheleſs his thermometer, placed at the entrance of theſe ventiges, ſtood at 4 degrees. He plunged his thermometer to the depth of 8 inches in the ground of this open cave; it then ſtood at 7 degrees; and at 8, when merely laid on the ground; but on the floor of a cloſe cave it ſtood at 5. The thermometer, as already remarked, ſtood at 18 degrees in the open air.

This cold air has no ſenſible quality different from pure atmoſpheric air cooled to the ſame degree. It has neither ſmell nor taſte; but, as the author obſerves, it would certainly be curious to analyſe it.

The conſtructor of theſe caves, who appeared to be an intelligent man, expreſſed his belief to M. De Sauſſure, that the cold air comes from the inſide of the mountain, and iſſues from clefts concealed beneath the rubbiſh: but that nevertheleſs there was no knowledge exiſting of any cavern or natural repoſitory of ice in this mountain, in which the ſnows might accumulate during the winter. The mountain is not high enough to preſerve viſible ſnow during the ſummer. It is neceſſary however, according to M. De Sauſſure, that the cauſe of this phenomenon ſhould be very extended; for he was certainly informed that theſe caves exiſt as far as Capo di Lago, at the diſtance of eight miles from Caprino, and even at Mendriſio, which is a league farther off. There are ſome alſo on the oppoſite ſhore of the lake. It is affirmed that there are ſeveral on the banks of the lake of Come; which M. De Sauſſure more readily believes, becauſe he found the water of the intermitting ſpring at the Villa of Pliny, ſituated on the banks of the lake, to be as cold as $7\frac{1}{2}$ degrees.

The laſt caves deſcribed by De Sauſſure, from whence cool ſtreams of air are emitted, are thoſe of Hergiſweil. Theſe were the only ones he ſaw on that ſide of the Alps. At Winckel, a village at the diſtance of one league from Lucerne, he embarked on the lake; and in leſs than half an hour he was oppoſite Hergiſweil, a village belonging to the canton of Underwald, ſituated at the bottom of a ſmall bay, and ſurrounded with meadows and vineyards, in a poſition highly rural and romantic. At the diſtance of ten minutes from the village, at the foot of the mountain, are the cold caves, which are nothing but huts of wood except the bottom wall. This wall, like thoſe at Lugan, is applied to the accumulated fragments at the foot of the mountain. The ſtones of the wall are not bound with mortar; and it is through the interſtices between them that the cold wind enters, which iſſues from the fragments of the mountain.

On the 31ſt of July 1783, at noon, the thermometer ſtood at 18,3° in the ſhade, and 3,3° at the bottom of the cave. The maſter of the houſe aſſured him that milk could be kept in the cave for three weeks without change, fleſh meat a month, and ſtrawberries from one year to the other. Near this hut there was another of the ſame kind, in which ſnow is kept for ſale at Lucerne in the ſummer; but there was no ſnow in the cave where he obſerved the temperature. Cloſe to the hut, under the ſame roof, a fire was kept for domeſtic purpoſes, without any apprehenſion of its affecting the temperature of the cave. In winter it freezes in theſe huts rather later than in the open air, but afterwards, as they affirm, more ſtrongly; no doubt, ſays M. De Sauſſure, by reaſon of the returning current of air into theſe ſubterraneous cavities.

The mountain which overlooks theſe caves is calcareous, its ſteep ſide lying to the north, againſt which the huts are placed. Its name is Renq, and its foot advances into the lake of Lucerne, where it forms a promontory. It is one of the baſes of Mount Pilate, of which it forms part. M. Peyffer informed M. De Sauſſure, that the lake is very deep near the rock.

It appears that the cold wind iſſues at many places; for at the foot of the mountain in the neighbourhood, if the earth be any where removed which covers the rubbiſh, the cold wind which iſſues forth may be felt againſt the hand[*].

The theory by which M. De Sauſſure explains the cold of theſe caves is as follows:

It muſt be ſuppoſed that the air which cools theſe caves was included in ſubterraneous cavities, not ſufficiently deep to be inacceſſible to the heat of ſummer, and the cold of winter, but yet enough ſo to admit of a variation of a few degrees from theſe changes. It muſt alſo be ſuppoſed, that after this air has been ſomewhat condenſed by the winter's cold, and the ſummer's heat begins to cauſe it to iſſue forth by dilatation, it is again cooled by evaporation during its paſſage through humid clefts, or the interſtices of wet ſtones.

He thinks that the exiſtence of ſuch reſervoirs, acceſſible to the cold of winter and the heat of ſummer, is not hypothetical, but an immediate conſequence of the facts univerſally atteſted by the poſſeſſors of theſe caves, namely, that the air iſſues out in the ſummer with ſo much the more force as the weather is hotter, and returns again by the ſame apertures in proportion to the intenſity of the cold.

Though the cooling effect of evaporation admits of no doubt, he thought it proper to make an experiment reſembling what he conceives to happen in this phenomenon. He took a tube of glaſs of one inch in diameter, which he filled with fragments of wetted ſtone. Through this tube he forced the blaſt of a large pair of bellows: the air iſſued out of the bellows at the temperature of 18 degrees, and its paſſage through the tube cauſed it to deſcend to 15. He had the ſame reſult when he uſed a chemical receiver with two necks, half filled with ſmall flints wetted; but when he directed the wind of the ſame bellows againſt the ball of a thermometer ſurrounded with a wet cloth, the refrigeration was four degrees. A ſtill greater refrigeration, namely, of nine degrees, was produced by incloſing the ball of the thermometer in a wet ſponge, and whirling it round in the air. But, as this candid obſerver juſtly remarks, the air in this laſt caſe is continually renewed; ſo that the circumſtances are not accurately parallel to thoſe in which the air is ſuppoſed to become continually more and more loaded with moiſture.

---

[*] To theſe inſtances may be added the caves of Roquefort, of which a deſcription is given by Chaptal in the Annales de Chimie, IV. 31, 45. From the deſcription of the caves themſelves, which is leſs preciſe than thoſe of M. De Sauſſure, it appears that the air iſſues from among the fragments of a calcareous mountain. Theſe caves are uſed in the manufacture of a peculiar and highly eſteemed cheeſe. M. Marcorelle in the month of October found the thermometer of Reaumur deſcend in theſe caves to 5½ degrees; while it ſtood at 13 degrees in the open air; and Chaptal on the 21ſt Auguſt 1787, with a good thermometer, which ſtood at 23 in the ſhade in the open air, found the temperature of a rapid current in one of the caves to be 4 degrees. He was informed that the thermometer had been ſeen in that expoſition as low as 2 degrees above zero. The hotter the external air, the cooler the caves are found to be, becauſe the current is then ſtronger.

Mr. Chaptal ſuppoſes, in his explanation, which is very conciſe, that the external air enters the earth, and is cooled by evaporation; but he does not point out any cauſe why the current is produced, and varies in its velocity in proportion to the external heat. N.

He thinks, therefore, that evaporation would not ſuffice to explain the refrigeration of ſeven or eight degrees below temperate, ſuch as is obſerved in the caves of Lugan, but that it would ſuffice to explain a cold of five or ſix degrees below this term, ſuch as that at Ceſi, Iſchia, and Mont Teſtaceo. In fact, he ſuppoſes a great ſubterraneous reſervoir of air to exiſt ſufficiently near the ſurface of the earth, that the cold of the winter may cauſe it to deſcend three degrees below temperate, or the tenth degree, and that the heat of ſummer may cauſe it to riſe as much above that term.

When the cold ſhall have penetrated this reſervoir to its maximum, the temperature will be ſeven degrees.

Afterwards, when the heat of ſpring begins to dilate it, its temperature will riſe, ſuppoſe to eight. It will begin to iſſue out; and the evaporation, diminiſhing its heat three degrees, will reduce it to five; and this will be its term of the greateſt cold. In proportion as the heats of ſummer ſhall penetrate the reſervoir, the heat of the air which iſſues out will increaſe alſo: it will not however riſe above temperate, becauſe the greateſt heat of the reſervoir will be 13, and evaporation will reduce it to 10.

The compariſon of his experiments with each other, as well as with thoſe of the Abbé Nollet, proves, in fact, that the heat of theſe cool winds increaſes as the ſeaſon advances. For the Abbé Nollet found the caves of Mont Teſtaceo at 9,5 on the 9th of September, whereas M. De Sauſſure obſerved them at 5,3 on the 1ſt of July. In the ſame manner he found the caves of Lugan at 4½ on the 1ſt of Auguſt, and at 2⅟ a month earlier, namely, on the 29th of June.

When the air has obtained its higheſt degree of heat, it muſt remain for a certain time in a ſtate of ſtagnation; after which the reſervoir begins to cool, and abſorb the external air. In this ſituation the coolneſs of the autumn, and the froſts of winter, are ſufficient to preſerve the cold ſtate of the caves.

The author remarks, that from the original ſuppoſition of the medium temperature being at 10 degrees, and the cold produced by evaporation 3 degrees, it is impoſſible to explain a degree of cold which in ſummer is lower than five degrees. For if the reſervoir be ſuppoſed nearer the ſurface, ſuch for example as that the cold of the winter would cauſe it to deſcend to 5, it is true that the air with the additional cooling by evaporation would iſſue out in the ſpring at the temperature of two degrees. But it will likewiſe follow, that the reſervoir would riſe in its temperature proportionably higher by the heat of ſummer.

If, therefore, it be required to explain a greater degree of coolneſs than 4 or 5 degrees, ſuch as that of Lugan and Hergiſweil, and it be not ſuppoſed that evaporation in theſe circumſtances can produce more refrigeration than three degrees, it will be neceſſary to ſuppoſe the mean temperature of the reſervoir to be lower than 10 degrees; a ſuppoſition, as the author obſerves, by no means forced, at leaſt for the vicinity of the Alps; where alone obſervations have been made of caves poſſeſſing ſo low a temperature.

M. De Sauſſure anticipates the objection againſt his theory, that if the air in theſe caverns be already ſaturated with humidity, it cannot produce evaporation, nor conſequently cold. But he remarks, that caverns are not all humid; that the caverns here ſuppoſed muſt be of vaſt extent, in order that the dilatation cauſed by a few degrees ſhall afford conſiderable currents of air through the whole ſummer; and conſequently, that a great quantity of cold dry air muſt enter in with the winter, which will be very deſiccative when expanded by heat, and will dry the ſides of theſe caverns. Whence he thinks that the air may be conſidered as ſufficiently dry to produce an evaporation which may cool it three degrees.

After having given ſo intereſting and accurate a detail of facts on this curious ſubject, from the work of a philoſopher to whom the world is ſo greatly indebted, I have thought it a point of juſtice to relate his theory in a manner ſcarcely leſs copious, and that the more particularly, as the notions which occur to me on the ſubject have led me to differ from him in opinion.

In the firſt place I muſt remark, that we have no actual information of the exiſtence of ſuch vaſt caverns, eſpecially ſo near the ſurface of the earth. 2. By the author's own experiments it appears, that at very inconſiderable depths, compared with the thickneſs required for the roofs of ſuch caverns, the influence of the ſeaſons is ſcarcely perceptible. He found the greateſt difference at the depth of 29½ feet, in compact ground, during three years, was only from 8,95 degrees to 7,75 degrees, or 1,2 degrees in the whole. 3. If the roof were ever ſo thin, it appears from Count Rumford's experiments *, that heat, which in air, and very probably in all fluids whatſoever, is ſcarcely tranſmitted but by the aſcent of the heated parts or deſcent of the parts cooled, would not paſs downwards, becauſe the rarefied parts could not deſcend; conſequently the expanſion, even in this moſt improbable ſituation, could not take place. 4. From the experiments of Duvernois, related in the Encyclopédie Méthodique, article Air, page 686, which are alſo to be found in the firſt volume of the Annales de Chimie, it appears that common air dilates rather more than $\frac{1}{12}$th part of its bulk by the firſt twenty degrees of Reaumur, and therefore ſcarcely more than $\frac{1}{80}$th part by the difference of temperature ſuppoſed by M. De Sauſſure. This cave muſt therefore contain near the ſurface of the earth, to be within the reach of the ſeaſons, eighty times as much air as flows out of all the apertures during the whole ſummer. For want of data, that is to ſay, the apertures and velocities of emiſſion, it is impoſſible to inſtitute a calculation; but it ſeems utterly improbable that ſuch a volume of air ſhould have been reſerved in an appropriate veſſel. At all events, it is not in the ſmalleſt degree likely, that Mont Teſtaceo contains, or communicates with, ſuch a reſervoir. 5. Laſtly, it ſeems a gratuitous ſuppoſition, that the reſervoir ſhould be always dry, and the paſſage through which the air iſſues always wet.

After this undiſguiſed examination of the conſiderations of M. De Sauſſure, I ſhall myſelf preſent a few notions reſpecting the cauſe of this phenomenon; with the perfect wiſh to ſee them refuted, if exiſting facts, future diſcoveries, or undetected errors ſhould render it neceſſary.

The whole, then, appears to me to depend upon the ſimple circumſtance of the cavity between a conſiderable maſs of ſtones or other ſubſtances not being capable of ſpeedily changing its temperature, by any other means than the contact of the air, or other fluids, which may paſs through it. Let us ſuppoſe, for example, the cathedral of St. Paul's in London, the dome of which is near 400 feet in height, to be filled with fragments of broken pottery. This large maſs might be ſuppoſed at firſt to poſſeſs the temperature of the air at the time when it was accumulated. From the imperfect conducting power of pottery and moſt earthy ſubſtances, the effect of the ſun's rays, or of the actual heat of the ſurrounding air, would pene-

---

* Experimental Eſſays, VII.

trate to a very little depth. From ſeveral known facts, it does not ſeem probable that the variations from theſe cauſes would extend to the depth of three feet. Suppoſe now the external air to be cooled ten degrees; the whole body of air contained in the interſtices of the pottery, being lighter than that without, would aſcend through the upper openings of the maſs, in conſequence of the preſſure from the denſer air into the lower apertures. This proceſs would continue until the contents of the edifice had acquired the common temperature; that is to ſay (without attending to the capacities of pottery and of air for heat, and the relative bulks of the interſtices to the ſolid parts, but ſuppoſing theſe to be equal), it would follow that a quantity of air near one thouſand times the bulk of the edifice muſt paſs through the interſtices before the common temperature would be reſtored. As the winter advanced, the current would continue to flow in at the doors, or near the baſe of the heap, and out from the ſuperior parts. This would continue ſo long as the temperature of the external air continued to be colder than the internal parts of the heap; and the velocity would be greater the greater the difference between theſe temperatures. But when the winter had reduced the whole maſs nearly to the freezing point, or perhaps below it, the return of ſpring, rarefying the outward air, would ſuffer the internal cold denſe air to flow out below. The warm air would neceſſarily preſs in above, become cooled, and flow out again beneath in that ſtate of diminiſhed temperature. And in this caſe alſo the velocity of the current would be greater the hotter the external air.

It is eaſy to ſuppoſe a prodigious variety of caſes. If the maſs be very large, the extreme variations will be more conſiderable, and the effect more permanent and ſteady. If the fragments be large, the current will be ſwifter, but the refrigeration leſs. And if among theſe fragments there be a conſtant cauſe of humidity, it may be inferred that this will render the deſcending current colder by evaporation than it would otherwiſe have been.

I was led to mention the cathedral of St. Paul's, from having very frequently in hot weather met a ſtrong cool blaſt iſſuing out of the doors and vaults of that edifice, produced, as I ſuppoſe, by the refrigeration of the air againſt the ponderous maſſes of maſonry within its area. As the probable cauſe did not occur to me till the preſent inveſtigation offered itſelf, I have not examined all the circumſtances of this laſt fact. Currents of this nature are however very common. The well-known experiment of holding a candle firſt at the top and then at the bottom of a door, is of this kind. If the air within the room, or the walls, be warmer than the external air, the flame will be blown outwards at the top, and inwards at the bottom of the door; but the contrary will happen if the walls of the room or its contents be colder. In this caſe, the lower current reſembles the cooling blaſt of the caves. There is no doubt, if two holes were bored in the door of a cellar in ſummer, the one near the top and the other near the bottom, that the upper hole would draw in the air, and the other emit a cold ſtream, until the walls had acquired the heat of the external air.

X.

The Combuſtion of Phoſphorus in the Vacuum of the Air Pump. By DR. MARTINUS VAN MARUM[*].

I. HAVING propoſed in December 1794 to exhibit in one of the lectures on the Teylerian foundation the combuſtion of phoſphorus in oxygene, and its combination with that ſubſtance in a glaſs veſſel, after the manner of M. Lavoiſier; but not being able to avail myſelf of the burning-glaſs to ſet fire to the phoſphorus, I ſuppoſed I might ſucceed by electric ſparks. I attempted therefore to give fire to the phoſphorus by electrical ſparks or ſmall diſcharges. Neither of theſe being attended with ſucceſs, I attached to a ſmall piece of phoſphorus a little cotton ſcarcely weighing three quarters of a grain, which I ſprinkled with a little finely powdered reſin. I afterwards placed this cotton, which roſe about one-fourth of an inch above the phoſphorus, in ſuch a poſition between the extremities of two conducting wires within the glaſs receiver, that the electric ſpark might paſs through it.

---

[*] Annales de Chimie, XXI. 158.

In this manner, on the 4th of December 1794, I attempted for the firſt time the inflammation and combuſtion of phoſphorus; without any other deſign, however, in this firſt experiment than to aſcertain, before the lecture, that there ſhould be no fault in the apparatus or method of making the experiment; and the time I could give on that day to this experiment being already expired, I did not rareſy the air in the receiver, more than till the mercury in the barometer gauge was about one inch lower than in the ſtandard barometer beſide it. I then paſſed the oxygene gas from the gaſometer into the receiver: I ſet fire to the phoſphorus after the receiver was thus filled, and the experiment was made without any difficulty.

II. As I thought I could now repeat the experiment in my lecture without any riſk of failure, I made the attempt a few days afterwards. I placed another ſmall piece of phoſphorus in a ſmall crucible of platina, which hung in the centre of a glaſs receiver thirteen inches in diameter, having firſt ſurrounded the upper part of the ſmall ſtick of phoſphorus with a little cotton powdered with reſin. I then proceeded to rareſy the air as much as poſſible; but an unexpected appearance prevented me. The candles had been removed, for the better obſervation of the light at the ſurface of the phoſphorus. We ſaw the light increaſe very perceptibly in magnitude and ſtrength, when the height of the mercury in the barometric gauge ſtill differed one inch from that in the barometer. This light increaſed in proportion as the air was more rarefied. I was far from ſuppoſing that the phoſphorus would take fire, for which reaſon I continued to work the pump; but, contrary to all expectation, we ſaw the phoſphorus take fire when the mercury was about half an inch lower than that in the barometer.

[*To be continued.*]

MATHEMATICAL CORRESPONDENCE.

QUESTION V. *Anſwered by J. F...... R.*

THE bulk or volume of a chemical compound being very ſeldom, and perhaps never, equal to the ſum of thoſe of its component parts before their union, it follows that the relative quantities of its ingredients cannot be inferred from its ſpecific gravity by the mere rule of alligation; but recourſe muſt be had to the reſults of actual experiment. Perhaps M. Baumé's experiments on the ſpecific gravities of different ſolutions of common ſalt in water, preparatory to the conſtruction of his peſe-liqueur (an account of which will be found in this work, p. 38.), with the appreciation of the ſcale, deduced from the obſervation of M. De Morveau, will afford us the beſt data for the ſolution of the preſent queſtion. The ſpecific gravity of the ſolution given being 1.1213, we ſhall find, from the table in p. 39, that it contained about 0.1581 of ſalt, or 444 grains in the whole; being near 29 grains leſs than what would reſult from mere computation.

⁂ The above was the anſwer ſent previous to the appearance of the Editor's note, p. 192. F. was ſufficiently aware that the abovementioned table was not to be relied on to indicate the ſpecific gravities of all poſſible ſolutions of common ſalt in water; which indeed he conceived to be ſo obvious as to preclude the neceſſity of more reſtrictive terms. It is however certainly true, that, if Baumé's conſtruction of the inſtrument, and De Morveau's obſervation, were both correct, the tabular ſpecific gravity anſwering to 15° would be accurately that of the ſolution of 0.15 of ſalt uſed by the former; and that the errors of the table, for all inferior degrees of ſaturation, would be thoſe only ariſing from the hypotheſis of equal graduation on which it is calculated; which, as the Editor obſerves, p. 192, are probably not very conſiderable. According, therefore, to this mode of reaſoning, the table ought to be perfectly correct about that degree, and ſerve well enough for giving nearly the ſpecific gravities of all aqueous ſolutions of common ſalt, in which its proportion does not exceed 0.17 or 0.18. The great difference between this table and that of Dr. Watſon (p. 192.), which gives upwards of 0.18 of ſalt, inſtead of 0.15, for the ſpecific gravity of 1.114, ſeems only to be attributable to the reaſons aſſigned p. 38. Perhaps M. Baumé had either expoſed the ſalt to a conſiderable heat before he diſſolved it, or ſuffered the ſolution to evaporate before he uſed it for the inſtrument which fell into the hands of M. De Morveau; or poſſibly even its ſcale had been transferred from another whoſe ſtem bore a ſmaller proportion to its bulb. The latter table is probably the moſt to be depended on in all caſes.

NEW MATHEMATICAL QUESTION.

QUESTION IX. *By TRIGONOMETRICUS.*

THE angles of elevation of a terreſtrial object ſituated above the horizon, taken at three given ſtations in a horizontal plane, being given; it is required to determine from thence its perpendicular height.

NEW PUBLICATIONS.

Count Rumford's Experimental Eſſays, Political, Economical, and Philoſophical. Eſſay VII. Of the Manner in which Heat is propagated in Fluids. Of a remarkable Law which has been found to obtain in the Condenſation of Water with Cold, when it is near the Temperature at which it freezes; and of the wonderful Effects which are produced by the Operation of that Law in the Economy of Nature. Together with Conjectures reſpecting the final Cauſe of the Saltneſs of the Sea. Octavo, 108 pages, with two plates. Price 2s. ſtitched. Cadell and Davies, 1797.

THIS eſſay, which contains important diſcoveries and applications of the doctrine of heat, will demand particular and more ample notice in the preſent work than can be here given. The author had before proved that the communication of heat through air is almoſt totally effected by the circulation of its parts. He has ſince aſcertained, that water, and thence probably all other fluids, conduct that quality or matter by a ſimilar proceſs; and from the general facts obſerved by him, he concludes that they are non-conductors of heat. The experiments contained in this eſſay are very ſtriking and ingenious, and the author has availed himſelf of them in forming a ſet of deductions of great uſe in the explanation of the economy of heat on the ſurface of the planet we inhabit.

Traité de la Fonte des Mines, &c.; or, A Treatiſe on the Fuſion of Ores by Pitcoal, and the Conſtruction and Uſe of Furnaces proper for the Fuſion and Refining of Metals and Minerals by means of Pitcoal. Together with the Method of rendering this Coal fit for the ſame Uſes as Charcoal of Wood. By Mr. De Genſſane, of the Royal Academy of Sciences at Montpellier, &c. Sold at Paris at the Veterinary Library of M. R. Huzard, Rue de L'Eperon-Saint-Andre-des-Arcs, No. 11. 2 volumes in quarto; the firſt containing 400 pages, with 34 plates, and the ſecond 534 pages, with 42 plates. Price 20 francs for the two volumes in boards, and 12 francs for the ſecond ſeparately.

The firſt volume of this work appeared in 1770, and the ſecond, which was retarded at firſt to verify certain facts, and afterwards for other reaſons, was printed in 1776. Other circumſtances, unneceſſary to be detailed, have prevented the proprietor from offering the work regularly to ſale until lately.——Journal des Mines.

Anfangsgrunde der Chemie zum Grundriſſe Academiſcher Vorleſungen nach dem neuen Syſteme, &c.; or, Elements of Chemiſtry, to ſerve as the Plan of Academical Leſſons according to the new Theory. By M. G. Fr. Hildebrandt, Profeſſor of Medicine and Chemiſtry at the Univerſity of Erlang. Printed at Erlang 1794, 3 volumes, large octavo.

The celebrated L. B. Guyton, in the Annales de Chimie, XXI. 333, ſpeaks very highly of this work, for its order and perſpicuity. The author begins by exhibiting the general principles of mixture, ſolution, affinity, precipitation, &c. together with the operations of chemiſtry. In the next place he treats in ſeparate ſections on caloric, light, oxygene, azote, atmoſpheric air, hydrogene water, the earths, alkalis, acids, and compound ſalts. Theſe ſubjects occupy his firſt volume.—The ſecond volume treats of metals, their alloys, and combinations.—And in the third the chemiſtry of organized ſubſtances is explained.

Notizie, &c. An Account of the Siliceous Incruſtations of the Thermal Baths of Italy; and certain remarkable Products found under the Lava which buried Part of the Town of Torre del Greco in 1794. By M. Tompſon. Octavo. Naples, 1795.

The author has obſerved ſiliceous ſtalactites in the mountain of Santa Fiora in Tuſcany, in the Euganean mountains, in the iſle of Iſchia, and in the crater of the Solfatara or Pozzuolo. In moſt of theſe places there are likewiſe ſoda, humid and hot vapour, and ſulphur, either in ſubſtance or in the ſtate of ſulphuric acid. The ſolution of the ſilex may therefore be attributed to the combined action of theſe three chemical agents, particularly the ſoda. A kind of liquor ſilicum is formed. The author has diſcovered that the arundo donax of the family of the bamboos contains ſea-ſalt, and conſequently ſoda; and it is known that the knots of the bamboo contain ſiliceous concretions. The celebrated Black aſcertained the preſence of mineral alkali in the Geyſer ſpring in Iceland; round which ſiliceous incruſtations are formed.—Edinburgh Tranſactions, Vol. 2 and 3.

By digging into the houſes of the town of Torre del Greco, which were buried under the lava in 1794, among other things were found: 1. Glaſs changed into Reaumur's porcelain. 2. Iron mineralized, augmented in volume to thrice its former bulk; cryſtallized within in grains or plates, ſome of them three lines in length, with the argentine brilliancy; other portions in red roſettes of ſpecular iron ore, more or leſs participating of the nature of ſulphate of iron. 3. Silver coin fuſed in the ſame degree of heat which copper money reſiſted. 4. A candleſtick of braſs (chandelier de laiton) entirely metamorphoſed, the outſide cryſtallized into blende, intermixed with octahedrons of copper, more or leſs red: the fracture of the candleſtick towards the centre preſents very fine cubes of red copper. 5. Lead converted in ſome inſtances to litharge, and in others to minium, which is ſolid, compact, and of the moſt beautiful red colour.—Pictet, Bibliothéque Britannique.

An Inaugural Diſſertation on the Chemical and Medical Hiſtory of Septon, or Azote.

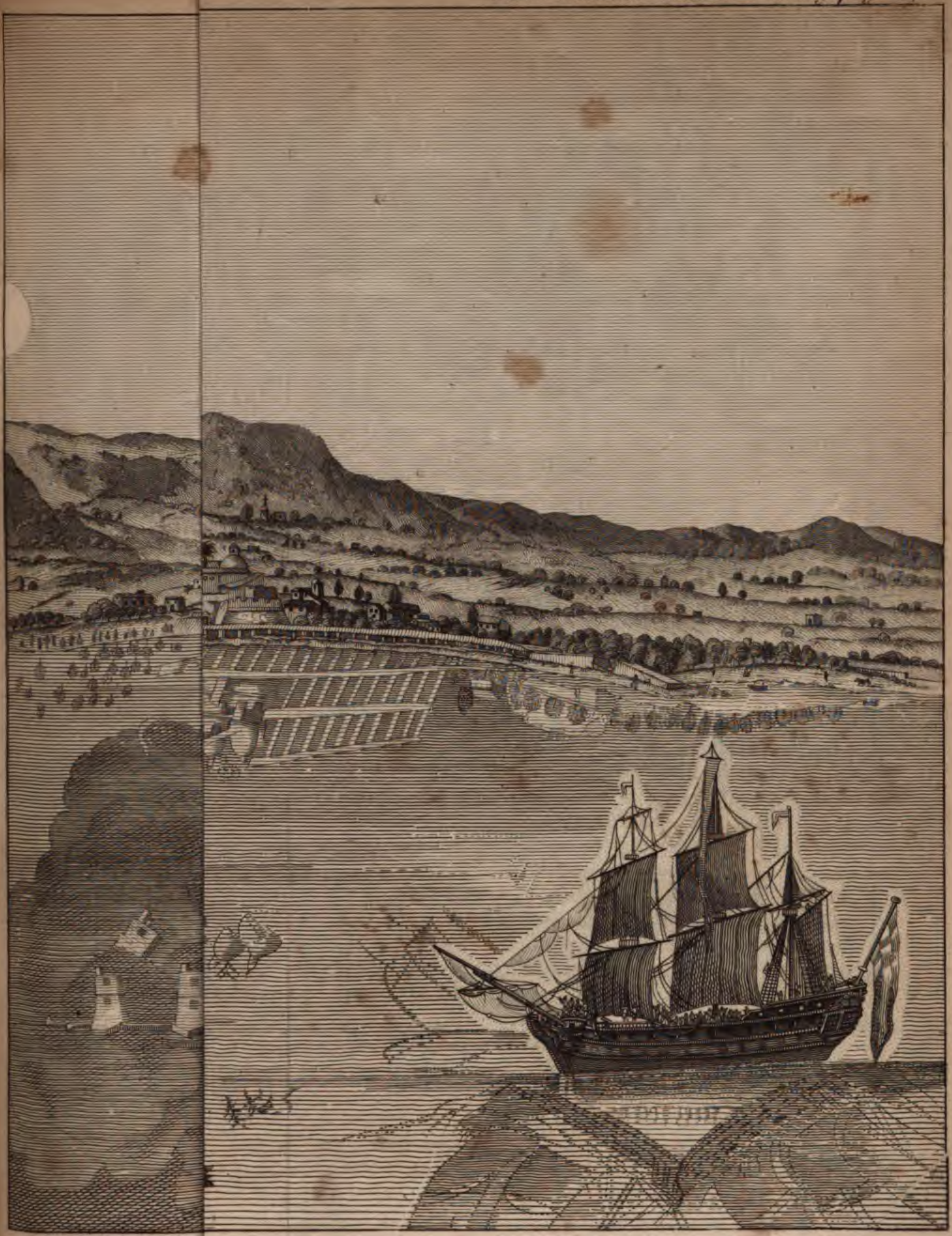
By Winthorp Saltonſtall. Octavo. New York, 1796.

I find this work announced in the 22d volume of the Annals of Chemiſtry, No. 64, for laſt April, with the following obſervations:

It is known that azotic gas forms 73 parts out of 100 in the atmoſphere. Cavendiſh made the important diſcovery, that 68 parts of oxygene, with 32 of azote, produced nitrous gas. Berthollet has proved that 6 parts of azote and 1 of hydrogene form ammoniac. Deimann and Van Trooſtwyk have lately proved that 37 parts of oxygene, chemically united to 63 of azote, form that gaſeous oxide called dephlogiſticated nitrous air by Prieſtley, which has the ſingular property of ſupporting combuſtion, though it deſtroys the life of animals which reſpire it. So that we know of four degrees of oxidation or combuſtion of azote: 1. The dephlogiſticated nitrous air of Prieſtley, called by the modern chemiſts oxide of azote. 2. Nitrous gas. 3. Nitrous acid gas. 4. Nitric acid.

This author thinks that the putrefaction of animal ſubſtances has an epocha in which the azote, at the inſtant of its diſengagement, coming into contact with oxygene, may combine with it without requiring a very elevated temperature. The oxide of azote thus formed may become a very active poiſon; and the cancer, with the whole family of corroding ulcers, has probably no other origin. The exhalations of azotic ſubſtances which putrefy in marſhes, in priſons, and in damp hot countries, form, according to the hypotheſis of the author, a certain chemical combination of azote with oxygene, which becomes the cauſe of contagion, and of various endemic and epidemic diſorders. This deſtructive oxide may introduce itſelf into the animal ſyſtem by the lungs, and produce putrid fevers and aſthmatic ſymptoms; it may penetrate with the aliments into the ſtomach, and become the leaven of epidemic bilious diſorders. Does not the yellow fever of America depend on the ſame morbific gas, ſince we know that it is azote which gives the more or leſs intenſe orange colour to wool, ſilk or ſkin, when it is abundantly contained in thoſe ſubſtances, and combined with a ſmall portion of oxygene? Laſtly, The oxide of azote may act on the abſorbent veſſels of the animal, and produce peſtilential bubos and ſores.

Here, ſay the editors, are conjectures not entirely without ſome appearance of truth; but which require many experiments and obſervations before they can be admitted in the extenſive degree propoſed by this author. For example, it is ſo far from being demonſtrated, as the author thinks, that the orange colour of wool, &c. is owing to azote, that it rather appears to ariſe from an exceſs of carbone. The ſame may be remarked of the oxide of ſepton of this author, or dephlogiſticated nitrous acid. This gas appears to be nitrous vapour mixed with oxygene gas, which laſt does not unite with the ſurplus of nitrous gas in the acid.



ed Fata Morgana. See p. 225.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER 1797.

ARTICLE I.

Experiments and Obſervations made with the View of aſcertaining the Nature of the Gas produced by paſſing Electric Diſcharges through Water; with a Deſcription of the Apparatus for theſe Experiments. By GEORGE PEARSON, M. D. F. R. S.[*]

IN the Journal de Phyſique for the month of November 1789, were publiſhed the very curious and intereſting experiments of Meſſrs. Paets Van Trooſtwyk and Deiman, with the aſſiſtance of Mr. Cuthbertſon, on the apparent decompoſition of water by electric diſcharges.

The apparatus employed was a tube 12 inches in length, and its bore was 1-8th of an inch in diameter, Engliſh meaſure; which was hermetically ſealed at one end, and, while it was ſealing, 1½ inch of gold or platina wire was introduced within the tube, and fixed into the cloſed end, by melting the glaſs around the extremity of the wire. Another wire of platina, or of gold, with platina wire at its extremity, immerſed in quickſilver, was introduced at the open end of the tube, which extended to within 5-8ths of an inch of the upper wire, which, as was juſt ſaid, was fixed into the ſealed extremity[†].

The tube was filled with diſtilled water, which had been freed from air by means of

---

[*] Communicated by the Author. This paper was read before the Royal Society, and an abſtract of the ſame publiſhed in the Tranſactions for 1797. N.

[†] In another part of Mr. Van Trooſtwyk's memoir it is ſtated that the diſtance was 1¼ inch from the end of the upper wire to the top of the lower wire; and that the diſtance between the inſulated ball and prime conductor was at firſt 3-4ths of an inch, but that afterwards it was increaſed to one inch. Although the wire faſtened into the top of the tube was ſaid to be 1¼ inch in length, it is obſerved, that when a column of 3-8ths of an inch of air was collected, it was almoſt at the extremity of the upper wire. From theſe and other inaccuracies, it will be made appear, that no one, from the account publiſhed, has been able to repeat the experiment.

Cuthbertſon's laſt improved air pump, of the greateſt rarefying power. As the open end of the tube was immerſed in a cup of quickſilver, a little common air was let up into the convex part of the curved end of the tube, with the view of preventing fracture from the electrical diſcharges.

The wire which paſſed through the ſealed extremity was ſet in contact with a braſs inſulated ball; and this inſulated ball was placed at a little diſtance from the prime conductor of the electrical machine. The wire of the lower or open extremity, immerſed in quickſilver, communicated by a wire or chain with the exterior coated ſurface of a Leyden jar, which contained about a ſquare foot of coating; and the ball of the jar was in contact with the prime conductor.

The electrical machine conſiſted of two plates of 31 inches in diameter, and ſimilar to that of Teyler. It poſſeſſed the power of cauſing the jar to diſcharge itſelf 25 times in 15 revolutions. When the braſs ball and that of the prime conductor were in contact, no air or gaz was diſengaged from the water by the electrical diſcharges; but on gradually increaſing their diſtance from one another, the poſition was found in which gaz was diſengaged, and which aſcended immediately to the top of the tube. By continuing the diſcharges, gaz continued to be diſengaged, and aſcend, till it reached nearly to the lower extremity of the upper wire; and then a diſcharge occaſioned the whole of the gaz to diſappear, a ſmall portion excepted, and its place was conſequently ſupplied by water.

The reſiduary portion of gaz being let out after each experiment, and the diſcharges being continued in the ſame water, this reſiduary gaz was left in ſmaller and ſmaller quantity; ſo that after four experiments, probably made on the ſame day, it did not amount to more than 1-80th of the bulk of gaz which had been produced. If it had been poſſible to paſs electric ſparks through this very ſmall quantity of gaz a ſecond time, or oftener, it was ſuppoſed it would have been diminiſhed ſtill more. But when the tube had been left for a night only filled with water, the reſiduary gaz was in greater quantity than after the laſt experiment the preceding day *.

It was concluded that the gaz produced by the electrical diſcharges was oxygen and hydrogen gaz, from decompounded water:

1. Becauſe no other gaz hitherto known inſtantly diſappears on paſſing through it an electric ſpark.

2. The gaz obtained muſt have been the oxygen and hydrogen of decompounded water, becauſe they were in exactly thoſe proportions in which by combination they reproduce water; the trifling reſidue being conſidered to be merely a portion of air which had been diſſolved in the water.

3. Liquids which are not compounded of hydrogen and oxygen, as ſulphuric and nitric acids, afforded gaz by the electric diſcharges, but which did not diſappear on paſſing through it an electric ſpark; but which did diſappear on adding to it nitrous gaz over water. Mr. Schurer alſo aſſerts, on the authority of Mr. Van Trooſtwyk, that even liquid

---

* In at leaſt fifty experiments I have never ſeen the reſidue of gaz leſs than 1-40th of the gaz produced, although the water had been freed from air by the moſt effectual means. But Mr. Schurer (Annales de Chimie, tom. v. p. 276.) teſtifies that he ſaw Mr. Van Trooſtwyk make the experiment; and that after it was repeated many times, on the ſame parcel of water, *there was no reſidue at all.* I have very good grounds for believing, that this is one of the number of inaccuracies in the account publiſhed of this ſubject.

muriatic acid, which contains a very large proportion of water, affords hydrogen gaz only, the oxygen being abſorbed by the muriatic acid, and becoming oxy-muriatic acid.

From much experience I can ſafely affirm, that it is ſcarcely poſſible for the ſtudent, or even the proficient, to inſtitute the above experiment with ſucceſs from the explanation publiſhed. Hence, during the ſix years which have elapſed ſince its publication, no confirmation has been publiſhed except the experiment repeated by Mr. Cuthbertſon, for my ſatisfaction, as related in my work on the chemical nomenclature; but I have heard of many perſons, and ſome of whom were experienced electricians and chemiſts, who made the attempt.

Since Mr. Cuthbertſon came to reſide in London, I have learned from him the circumſtances requiſite to the ſucceſs of the experiment; and I have received from him alſo very great aſſiſtance in continuing a proceſs with the objects I had in view, the tediouſneſs and even difficulties of which can only be conceived by thoſe who have been engaged in the ſame purſuit.

I am very ſenſible that it would be unneceſſary for me to explain the importance of a proceſs which may at laſt afford demonſtration of the compoſition of water, by the fulleſt and unequivocal evidence of its analyſis and ſyntheſis; a demonſtration which no other ſingle proceſs but the preſent promiſes to afford.

I propoſe therefore in this paper:

1. To give ſuch a deſcription of the experiment of rendering water into gaz by electric diſcharges, as ſhall enable any perſon who is verſed in pneumatic chemiſtry, and acquainted with the theory and practice of electricity, to repeat it with ſucceſs. By this deſcription, alſo, I apprehend I ſhall make known more generally the very elegant, and frequently moſt ſatisfactory, mode of decompounding and compounding bodies, by means of the fire of the electric diſcharge.

2. It is propoſed to relate the additional evidence which I have already obtained from this proceſs, concerning the compoſition of water. For although it ſeems moſt probable that water is really decompounded in Mr. Van Trooſtwyk's experiment, it muſt be confeſſed that it does not make appear a ſingle unequivocal and deciſive property of hydrogen and oxygen in the gaz produced. The diſappearance of this gaz by combuſtion, or in ſome other way, inſtantly on paſſing through it an electric ſpark, it is true, is a property known only to belong to the mixture of oxygen and hydrogen gaz; but it is well aſcertained, that things of totally different ſpecies may agree in one or more properties. And there is at leaſt a poſſibility, that electric diſcharges may produce various other kinds of gazes in water, beſide hydrogen and oxygen from decompounded water; and which may have the property of inſtantly diſappearing on paſſing through them an electric ſpark.

3. I ſhall attempt to reſolve the phenomena of the proceſs into a general law of the action of fire, or of the joint action of caloric and light.

SECTION I.

Of the Manner of conducting the Proceſs.

ELECTRIC diſcharges may be employed in two different manners to decompound water. One of theſe is by what has been termed *the interrupted exploſion*; which was the method,

thod, although not ſo explained, of Mr. Van Trooſtwyk. And the other method is by means of *the uninterrupted or complete exploſion;* for which there are two different kinds of apparatus. Theſe were invented by Mr. Cuthbertſon in the courſe of my inveſtigation of this ſubject.

To ſucceed by the method of the *interrupted exploſion*, the following are the neceſſary parts of the apparatus to be uſed, and the circumſtances to be attended to:

1. *The electrical machine muſt poſſeſs ſufficient power.* I do not think any cylindrical machine can be made to anſwer in this proceſs if a large quantity of gaz be required; becauſe they cannot be made to act with due regularity, conſtancy, and force. Inequality of the ſurface of the cylinder is unavoidable, which cauſes undulation. A cylindrical machine never continues in full force above five or ſix minutes, without freſh amalgam. Hence, from the repeated amalgamization, the diſcharges will be ſo variable that the tubes muſt be frequently broken.

I uſed plate machines of a peculiar conſtruction by Cuthbertſon. Theſe machines do not require freſh amalgamization oftener than once in eight hours, and they poſſeſs ſuperior powers of acting, in point of regularity, force, and duration. A plate of 24 inches in diameter, with a jar containing 150 ſquare inches of coating, afforded an adequate diſcharge every ſecond or third revolution, for ſeveral hours; and for a ſtill longer time every third or fourth revolution, with one application of amalgam. A 31 inch plate machine afforded a due diſcharge at firſt every revolution, and afterwards every ſecond revolution for many hours, with one application of amalgam. The moſt uſeful and expeditious machine was that with two plates, each 24 inches in diameter, and ſimilar to that of Teyler. It produced 25 diſcharges every 15 revolutions for an hour or two; and for four or five hours longer a diſcharge was produced by leſs than two revolutions, with one amalgamization.

2. *The Leyden jar muſt have a ſufficient quantity of coated ſurface;* without which the diſcharge will not be ſufficiently powerful to produce the gaz required. The proper quantity, as found by experience, was about 150 or 160 ſquare inches, with an uſual proportional prime conductor.

3. *The diſtance between the inſulated ball, and the prime conductor, muſt always be leſs than the diſtance between the extremities of the wires.* Not the leaſt notice of this circumſtance has been taken; yet without attention to it the experiment can never ſucceed, or only for a very ſhort time. Accordingly, as the diſtance between the extremities of the wires within the tube anſwered beſt when it was 5-8ths or 7-8ths of an inch, the diſtance between the inſulated ball and prime conductor was ſeldom more, but frequently leſs, than 5-8ths or 6-8ths of an inch. The eye muſt be kept upon the ſparks within the tube, and by practice a perſon may become a judge of their force by their vividneſs; which will direct him to bring the receiving ball nearer to the prime conductor, when there appears danger of the tube being broken; and on the contrary, to remove them to a greater diſtance from one another when the ſparks do not produce gaz duly from the water. When the diſcharge is of the moſt productive force, both ends of the wire within the tube will be illuminated by a ſpark; but, when it is weaker, one end only of the wire will be illuminated; and when this is the caſe, there is no riſque of the tube being fractured, but gaz will riſe from the end of one wire only inſtead of two.

4. *The extremities of the upper and under wire within the tube muſt be at a certain diſtance*

from one another. If they be too near one another, the points of them will not be illuminated; and provided the inſulated ball be as near to the prime conductor as the two wires are to one another, the tube will be broken, becauſe there will be a complete exploſion. But if the wires be at too great a diſtance from one another, the electric fluid of the diſcharge will be ſo diffuſed through the water that no gaz will be produced. If the Leyden jar contain, as above ſtated, 150 or 160 ſquare inches of coated ſurface; and the ball of the prime conductor and of the inſulated ball be about three inches in diameter, the diſtance between the wires which generally anſwered beſt, was about 5-8ths or 7-8ths of an inch, as above ſaid. The narrower the bore of the tube, the greater may be the diſtance between the two wires; accordingly, the diſtance may be one inch with a tube 1-14th of an inch wide.

5. *The upper wire fixed into the cloſed extremity of the tubes muſt be of a proper length and thickneſs.* If this wire be too long, either the diſcharge will not be carried through to the end of the lower wire in ſufficient quantity to produce gaz; or, if it be in ſufficient quantity to produce gaz, the tube will be fractured. The ſmaller the diameter of the tube, the longer may be the upper wire, for a reaſon to be given under the next head. I generally found that the diſcharge requiſite to produce gaz fractured the tube, if the upper wire was more than 6-8ths or 7-8ths of an inch in length, within a tube of more than 1-8th of an inch in diameter. But with very narrow tubes, ſuch as thoſe of 1-16th of an inch in diameter, I frequently ſucceeded when the upper wire was 1¼th of an inch in length. It is obvious, that the ſhorter the upper wire the more readily will gaz be produced: the proceſs, however, will be rendered ſtill more tedious in thoſe caſes in which a quantity of gaz is to be collected in a reſervoir for examination; on account of the time conſumed in transferring ſuch ſmall parcels of gaz by each experiment.

The diameter of the upper wire cannot, perhaps, be too ſmall; for the greater its ſuperficies, the more electric fluid will be parted with to the ſurrounding water. Hence platina wire of the fineſt ſort, as that of 1-240th of an inch in diameter, may be uſed with ſuperior advantages. This ſort of wire alſo cannot be melted while it is ſoldering to the glaſs, which can hardly be prevented with fine wire of other metals. However, I found that copper, braſs, or gilt wire, of about 1-80th or 1-100th of an inch in diameter, could be ſoldered to the glaſs, and anſwered perfectly. I did not find that any of the metal wires were affected by the diſcharge; but iron or ſteel ones are not proper, on account of their being ſo ſoon oxidified by the water, and conſequently extricating from it hydrogen gaz. I did not find any advantage from uſing ſeveral ſmall wires twiſted together, but ſeparated at the end within the tube; for gaz was extricated generally, at the point of one of them only, namely, the undermoſt. I think care ſhould be taken to fix the upper wire ſo that it ſhall be in the middle of the tube; as in that caſe the tube will be leſs liable to be broken. As to the under wire, the diameter of it ſeems to be of little importance; for, if it be a thick one, as much gaz will be extricated as if it were a ſmall one; becauſe the electric diſcharge will take the firſt point of the ſurface of the wire where the gaz is produced.

6. *The tubes muſt be of a proper length and diameter.* If they be ſhorter than ſeven inches, the diſcharge will be liable to paſs over the outſide; and if they be longer than twelve inches, they will be of an inconvenient length. I found the moſt convenient length to be from nine to ten inches, excluſive of the curved part. The curved part was found very uſeful in preventing air aſcending, which was accidentally let into the tube, by which the

product of gaz from the experiment would have been contaminated. Such curved extremities were however leſs convenient than ſtraight ones, on account of the greater difficulty of transferring from the former than from the latter. See Fig. 1 and 2, Plate XI.

The diameter of the tubes ſhould not be more than 1-8th or leſs than 1-12th of an inch. At leaſt in my experiments theſe tubes anſwered beſt. If they were wider, the diſcharge requiſite to produce gaz broke the tube. If the tubes be narrower than juſt mentioned, the experimenter will find it difficult and tedious to transfer the gaz, through the curved part, into a reſervoir, in thoſe caſes in which a large quantity is wanted for examination. Where however the object is merely to ſhew the production of gaz by means of the electrical diſcharge paſſed through water, between two wires; and the inſtant diſappearance of gaz ſo produced by paſſing through it an electric ſpark, the narroweſt tubes are moſt eligible; as with them the experiment can be made in a ſhorter time. But there was one inconvenience experienced from very narrow tubes, namely, the bubbles of gaz were very apt to hang near the end of the upper wire inſtead of aſcending; and they were apt to form large bubbles, with water between each; in which ſituation the diſcharge frequently fractured the tube, or made gaz to diſappear by combuſtion in the courſe of the experiment. To poſſeſs at once the advantages of the narrow and wide tubes; about three to ſix inches of narrow tube were joined by fuſion to a bottom part of wider tube (ſee Fig. 3); into the curve of which tube the gaz produced was let up from time to time; ſo that this part frequently contained the products of ten or more experiments before the gaz was transferred into the reſervoir. The vaſt number of tubes which were broken in this experiment induced me to try various different kinds of them. I experienced no advantages from annealing tubes; but their being ſeemingly rendered more brittle, and harder, by this treatment, was an effect which I leaſt of all expected. Tubes with thin ſides anſwered juſt as well as with thick ones. Bohemian green glaſs tubes were found excellent for this experiment, as they were much leſs apt to be cracked by the diſcharges than any kind of Engliſh glaſs. To ſave time and trouble in ſo frequently letting out gaz produced in very narrow tubes, a ſmall bulb was made at the ſealed end (ſee Fig. 4). In this caſe, however, as the upper wire muſt be ſhorter than other narrow tubes, it was more difficult to regulate the exploſion.

Although common atmoſpherical air is an electric, and water is a conductor of electricity, it appears that the diſcharge paſſes with more reſiſtance from wire to wire through water in the above experiment, than through common air under otherwiſe the ſame circumſtances. The reaſon of which is this: air being an elaſtic and very rare fluid, it more readily gives way to the electric diſcharge than water; and it can therefore paſs through a longer and thicker column of air between two wires, without breaking the glaſs tubes, than it can through water. For although water is a conductor, yet in a very ſmall quantity it is a very indifferent one; ſo that its denſity and defect of elaſticity more than compenſate for its conducting power. Hence alſo, and on account of the conducting power of water, the reaſon of the upper wire in this experiment being ſhorter in proportion as the tube is wider; and on the ſame account will be ſeen the reaſon of the advantages of a ſmall upper wire over thicker ones.

It will be neceſſary to add, that the tubes with curved extremities can only be filled by ſetting them in water under a receiver, and exhauſting the air from the receiver, tubes, and water; then, by letting in the air again, the water will be forced up into the tubes. Sometimes, however, I have filled the tubes by ſetting them in Papin's digeſtor.

Theſe are the directions for making the experiment; but the rationale of it cannot be underſtood unleſs the nature of the *interrupted exploſion* be explained; becauſe I believe books on electricity do not contain the neceſſary information. It muſt be conſidered, in the above experiment, that if in place of water the tubes be filled with air, the whole of the charge of the Leyden jar will paſs, at each exploſion, from the upper to the under wire, and no interruption in the diſcharge will happen; but if they are filled with water, then an *interrupted diſcharge* may be cauſed; by which is meant that a part of the charge only paſſes at each exploſion through the water, from wire to wire, and with much diminiſhed velocity. The reſiduary electricity in the Leyden jar is nearly one half; as may be accurately demonſtrated from the difference in point of denſity, elaſticity, and conducting power of the medium of water and air as already obſerved. It muſt be added, that although water in large quantity is a good conductor, and air is not; yet water being here in very ſmall quantity, it proves a bad conductor, as is the caſe with the very beſt conductors. A cubic foot of water is only juſt capable of receiving, or letting paſs through it, a full diſcharge from a jar of one foot of coated ſurface; and the quantity of water employed in this experiment not being $\frac{1}{1000000}$th part of a cubic foot, it is a very imperfect conductor; ſo that an interrupted diſcharge only can paſs through the tube, without diſperſing the whole of the water. But if the diſcharge be not ſeemingly as ſtrong as the tube can bear without breaking, the gaz is not produced from it; and on this point hinges this extremely delicate proceſs.

The ſituation of the different parts of the above deſcribed apparatus is ſhewn by Fig. 5.

To ſucceed by the method of the *complete* or *uninterrupted exploſion*, the following apparatus muſt be uſed, and rules obſerved:

1. A tube (Fig. 6.) is employed, about four or five inches in length, and 1-5th or 1-6th of an inch in diameter. One end is mounted with a braſs cap (Fig. 7.), and the other end is ſealed at the lamp, with a wire about 1-40th of an inch in thickneſs fixed into it, as above deſcribed; which extends into the braſs cap, ſo as to be almoſt in contact when the exploſion is made. If the wire touches the braſs cap there will be no exploſion. The tube being filled with, and ſet in, a cup of water, the diſcharge may be made into it as in the above deſcribed proceſs; but here the inſulated ball muſt be placed at a greater diſtance from the prime conductor, and a Leyden jar, with only 50 ſquare inches of coating, will anſwer the purpoſe. In this way of making the experiment, gaz is produced by each diſcharge in the braſs tube, and in much greater quantity, and with much leſs frequent accidents, and leſs trouble, than in the former method with the interrupted diſcharge. But the gaz obtained with this apparatus always contains a larger proportion of atmoſpherical air, on account of the quantity of water, and more immediate and extenſive communication of it with the atmoſphere. By repeated diſcharges there is an impreſſion made in the braſs tube, in the part where the diſcharge paſſes through it, and at laſt a ſmall hole is made in that part. On this account the ſame mounted tube cannot ſerve for producing a large quantity of gaz.

The other ſort of apparatus invented by Mr. Cuthbertſon is repreſented by Fig. 8. At firſt it conſiſted of a glaſs tube half an inch wide and about five inches in length, mounted at one end with a braſs funnel, and inverted in a braſs diſh; but afterwards the tube was blown funnel-wiſe at the end, as ſhewn by Fig. 9. The other end muſt have a wire about 1-40th of an inch thick, ſealed into it at the lamp; which wire extends to nearly the bottom of the braſs diſh in which the tube ſtands.

The exact diſtance between the end of the wire and braſs diſh muſt be found by trials; that which generally anſwered in my experiments was about 1-20th of an inch. If it be properly arranged, gaz will be produced at each diſcharge.

The Leyden jar uſed with this apparatus muſt contain about 150 ſquare inches of coating.

The diſtance between the inſulated ball and the prime conductor, at which the experiment ſucceeded, was commonly about half an inch.

If experiments be propoſed in which electrical diſcharges muſt be paſſed through water or other fluids, for even a much longer time than was conſumed in performing thoſe referred to or related in this paper, it may be an object to employ the wind, or perhaps the power of a horſe, to turn the electrical machines, the expence of labourers being conſiderable.

[*To be continued.*]

II.

Analyſis of four Specimens of Steel; with Reflections on the new Methods employed in this Analyſis.

By Citizen VAUQUELIN.

[Continued from page 217.]

SECTION III.

STEEL NO. 864—LARGE PIECE.

EXPERIMENT I. 1152 grains, or 61,14 grammes, of this ſteel, broken into ſmall particles, were diſſolved in the ſulphuric acid diluted with five parts of water. There remained at the bottom of the ſolution 5,5 grains, or 0,292 grammes, of black powder of the carburet of iron. In another experiment, the ſame quantity of this ſteel afforded 6,4 of carburet of iron; a quantity, the medium of which is 5,95, and conſequently the reſidue forms a fraction of the diſſolved maſs equal to 0,0051.

Experiment II. 100 grains, or 5,3 grammes, of the ſame ſteel, diſſolved in the ſulphuric acid, afforded 121 inches, or 2420 cubic millimetres of hydrogene gas. The black reſidue weighed ſcarcely 0,75 grains.

Experiment III. The 5,5 grains of carburet of the firſt experiment being ſubjected to heat under a muffle, took fire, and left three grains of a grey yellowiſh maſs, which, when treated with boiling muriatic acid, aſſumed a white colour, while the acid became yellow. It was reduced to 1,66 grains, which amounts to 0,0014 of the maſs of ſteel. This ſubſtance poſſeſſed all the characters of ſilex.

The muriatic acid uſed in this boiling contained a perceptible quantity of iron, which appeared to be the cauſe of the yellow colour it poſſeſſed previous to the operation.

Experiment IV. The ſolution of 1152 grains of iron in the ſulphuric acid having been treated as in Experiment III. Section 2, afforded 58 grains of phoſphate of iron, which makes about 0,05 of the weight of the ſteel employed. The fluid likewiſe depoſited by ebullition 96 grains of a deep red matter, which did not perceptibly contain phoſphate of iron.

From this reſult it appears that the preſent ſteel contains nearly three times the quantity of phoſphorus exhibited by No. 864, the ſmall piece. For 576 afforded only 19 of phoſphate of iron, whereas 1152, or only twice the quantity, afforded 58 grains. This ſteel muſt conſequently be more brittle when cold, if, as there is no reaſon to doubt from the numerous experiments of Bergman and other chemiſts, that phoſphorus is the eſſential cauſe of the cold ſhort quality of iron and ſteel.

Experiment V. 288 grains of ſteel No. 864, the large piece, diſſolved in ſulphureous acid, as before mentioned, left 6,5 of carburet of iron, which, ſuppoſing it to contain as much ſulphur and iron as that of No. 864, the ſmall piece, would give 3,71 of carbone, or about the 0,010th part, or rather more than one-hundredth part of the ſteel employed.

Experiment VI. This ſteel was ſubjected to all the proofs calculated to ſhew the preſence of mangaueſe, but without any exhibition of that ſubſtance. It may therefore be concluded that this ſteel, as well as the other examined before, does not contain any perceptible quantity of manganeſe, at leaſt ſo far as the preſent methods of chemiſtry are capable of ſhewing it [*].

SECTION IV.

STEEL NO. 977.

Experiment I. 144 grains, or 7,631 grammes, of this ſteel, diſſolved in the ſulphuric acid, diluted with five parts of water, afforded 164,94 cubic inches of hydrogene gas, and left 0,941 grains, or about 0,05 grammes, of carburet of iron, or nearly 0,007 of its maſs.

Experiment II. 504 grains, or about 26,7 grammes, of the ſame ſteel, diſſolved in the ſulphuric acid, diluted with five parts of water, the ſolution being diluted with a large quantity of water, and its exceſs of acid ſaturated with carbonate of pot-aſh, depoſited, a few inſtants afterwards, a large quantity of a white matter, ſlightly inclining to grey, which acquired a ſtraw-colour by contact of the air. This matter was phoſphate of iron, and weighed 22 grains, or about 1,16 grammes.

Experiment III. The liquid whence the phoſphate of iron had been precipitated by the carbonate of pot-aſh was ſubmitted to ebullition, and again depoſited 22 grains, or 1,16 grammes, of matter, in which, however, there was ſcarcely any phoſphate of iron. So that 504 grains of the ſteel No. 977 contained 23 or 24 grains of phoſphate of iron; that is to ſay, 0,047 of its maſs.

Experiment IV. Theſe 24 grains of phoſphate of iron, treated by long ebullition with cauſtic ſoda, aſſumed a deep red colour, and after waſhing, and drying in the air, weighed only 15,33 grains. They loſt therefore 8,67 grains of phoſphoric acid, which afforded 17,54 grains of cryſtallized phoſphate of ſoda.

---

[*] The oxide of iron formed with this ſteel, ſtrongly oxided by the action of fire, and afterwards treated with the nitric acid and ſugar, afforded an oxide which threw down a very beautiful blue with the pruſſiate of pot-aſh. The ſame oxide fuſed with borax afforded a greeniſh globule; but when heated with the external flame at the extremity of a pair of forceps it acquired a ſlight purple colour, which diſappeared as the globule became cold.

Experiment V. This ſteel, diſſolved in the ſulphureous acid, afforded very nearly the ſame quantity of carbure of iron as No. 864, the ſmall piece.

SECTION V.

STEEL NO. 1024.

Experiment I. One hundred grains, or about 5,3 grammes, of filings of this ſteel afforded 108 cubic inches of hydrogene gas during their ſolution in the ſulphuric acid.

I have remarked that the hydrogene gas produced by this ſteel had an extremely fetid ſmell, infinitely ſtronger than that of the gaſes afforded by the other ſteels, which appears to ariſe from the greater quantity of phoſphorus it contains *.

Experiment II. 288 grains of filings of the ſame ſteel being diſſolved in the ſulphuric acid, diluted with five parts of water, the carburet of iron ſeparated, the ſolution diluted with water, and the exceſs of acid ſaturated with carbonate of pot-aſh, afforded a very conſiderable white depoſition of phoſphate of iron, which, when well waſhed and dried, remained white, and weighed 26 grains, or about 0,09 of the maſs of ſteel made uſe of. Theſe 26 grains of phoſphate of iron, treated with cauſtic ſoda, afforded 22 grains of phoſphate of ſoda, and the ferruginous reſidue weighed 17 grains.

Experiment III. 288 grains of ſteel, diſſolved in the ſulphureous acid, left a carbonaceous reſidue, which, treated with pot-aſh in the manner deſcribed Section II. and dried, weighed 3,5 grains. But on account of the minute loſſes which it is impoſſible to avoid in a numerous ſeries of manipulations, the quantity of this ſubſtance may be eſtimated at four grains; and that the more reaſonably, as the other kinds of ſteel gave nearly the ſame reſults. The quantity of carburet of iron in this ſteel was therefore ſomewhat leſs than 0,014 of the maſs of the ſteel.

Experiment IV. This ſteel, ſubjected to the proofs requiſite to diſcover the preſence of mangancſe, preſented no trace thereof.

SECTION VI.

Table of the Quantities of Hydrogene Gas afforded by each Kind of Steel.

By uniting the different quantities of hydrogene gas afforded by the docimaſtic quintal of each of the ſteels diſſolved in the ſulphuric acid, we have the following reſults for each 100 grains.

	Cubic inches.
Steel, No. 864, ſmall piece	108
No. 864, large piece	121
No. 977	114
No. 1024	108

---

* The ſmell of putrid garlick emitted by iron and ſteel during its ſolution in acids, appears to depend immediately on the preſence of phoſphorus in the metal, of which a portion is diſſolved by the hydrogene gas. For this odour is more ſtrong in proportion to the quantity of phoſphorus. This obſervation proves that the proportion of this ſubſtance to the iron which contains it cannot be accurately determined by the proceſs of ſolution in acids which emit hydrogene gas. The ſulphureous acid may anſwer this purpoſe.

Table

Table of the Quantities of Carburet of Iron, contaminated with Silex, afforded by each of the Steels, converted into Decimal Parts.

Steel, No. 864, ſmall piece	0,015
No. 864, large piece	0,013
No. 977	0,012
No. 1024	0,015

As it was aſcertained by ſeveral experiments made upon the carburets of iron extracted from the different kinds of ſteel, that this ſubſtance contains at a medium 0,526 carbone, and 0,474 ſiliceous iron, it follows:

1. That the 15 thouſandth parts of No. 864, ſmall piece, are compoſed of

Carbone	0,00789
Siliceous iron	0,00711

2. That the 0,013th parts of No. 864, large piece, are compoſed of

Carbone	0,00683
Siliceous iron	0,00616

3. That the 0,015th parts of No. 977 are formed of

Carbone	0,00789
Siliceous iron	0,00711

4. That the 0,012th parts of No. 1024, are compoſed of

Carbone	0,00631
Siliceous iron	0,00568

I do not think it neceſſary in this place to aſcertain the proportion between the iron and the ſilex which exiſts in the carburet of iron, becauſe this earthy ſubſtance being to all appearance accidentally in the ſteel, muſt vary according to a great number of different circumſtances.

The following, however, are the mean reſults obtained from ſeveral experiments on the carburet of iron:

1. Carbone	0,53
2. Iron	0,26
3. Silex	0,21
	1,00

Table of the Quantities of Phoſphate of Iron afforded by the four Kinds of Steel.

By comparing the quantities of phoſphate of iron obtained from the four kinds of ſteel here ſubmitted to examination, very remarkable differences appear; as follows:

No. 864, ſmall piece	0,0210
No. 864, large piece	0,0504
No. 977	0,0474
No. 1024	0,0900

Table of the Quantities of Phoſphorus contained in each of the Samples of Steel.

Analyſis having ſhewn me that phoſphate of iron is compoſed of 0,58 of oxide of iron, and 0,42 of phoſphoric acid, and this laſt being formed, according to Lavoiſier, of 0,39 phoſphorus, and 0,61 oxygene, it follows that the ſteel

No. 864, ſmall piece, contained phoſphorus	0,00345
No. 864, large piece	0,00827
No. 977	0,00791
No. 1024	0,01580

I cannot venture, however, to affirm that there really exiſts ſo great a difference between the quantities of phoſphorus contained in each of theſe pieces of ſteel as is here given by experiment. For ſo little is required to change the reſults in this reſpect, particularly when ſmall quantities are ſubjected to trial, that two or three thouſandth parts may ſoon be obtained either way. Nevertheleſs, I cannot ſuppoſe, that, with the ſame proceſſes and equal care, the difference between the maximum and minimum of theſe quantities will prove to be the conſequence of uncertainty in the methods of analyſis.

A Synoptic Table of the Proportions of Principles contained in the four Kinds of Steel before examined.

Steel, No. 864, ſmall piece	1. Carbone	0,00789
	2. Silex	0,00315
	3. Phoſphorus	0,00345
	4. Iron	0,98551
		1,00000
—— No. 864, large piece	1. Carbone	0,00683
	2. Silex	0,00273
	3. Phoſphorus	0,00827
	4. Iron	0,98217
		1,00000
—— No. 977	1. Carbone	0,00789
	2. Silex	0,00315
	3. Phoſphorus	0,00791
	4. Iron	0,98105
		1,00000
—— No. 1024	1. Carbone	0,00631
	2. Silex	0,00252
	3. Phoſphorus	0,01520
	4. Iron	0,97597
		1,00000

SECTION

SECTION VII.

Reflections on the Cauſes of the variable Quantities of Carburet of Iron afforded by Steel diſſolved in the Sulphuric Acid.

MANY circumſtances may influence the carburet of iron by the ſame ſteel; and it is not eaſy to obtain equal maſſes of this ſubſtance, in two different experiments, from ſteel of the ſame nature.

Bergman obtained from 0,002 to 0,008 of carburet of iron from different ſteels. Is it probable that there was really ſo great a difference in the proportions of a ſubſtance to which the ſteels owe their qualities, without there having been likewiſe a very ſenſible difference in their uſual properties?

Among the cauſes of uncertainty which may occaſion the reſults to vary, we may principally remark the following: 1. The greater or leſs concentration of the acid employed to diſſolve the iron. 2. Its mixture with water at or before the time of the experiment. 3. The more or leſs conſiderable diviſion of the metal. 4. The length of time during which the ſolution is effected. 5. The time during which the depoſition of carburet remains in contact with the liquor after the ſolution is effected.

1. It is evident that the more concentrated the ſulphuric acid, the more rapid the ſolution, and the greater the quantity of caloric developed in a given time.

Now experiment proves that the ſolution of carbone by hydrogene is in the direct proportion of the caloric which penetrates it, or at leaſt that it follows ſome progreſſive ratio of increaſe with the temperature. It follows, therefore, that the more elevated the temperature during the ſolution of ſteel in acids, the leſs of carbone will remain. It alſo follows that it is advantageous not to uſe an acid too concentrated.

2. As the too great concentration of the acid is inconvenient for the ſolution of ſteel from which the carburet is to be extracted, it is alſo equally inconvenient to dilute it too much. It is in fact known, that when acids are too much diluted with water, they exert a leſs affinity on other bodies; and this is the caſe with reſpect to very diluted ſulphuric acid, and ſteel in a high ſtate of diviſion. The ſolution then takes place very ſlowly; part of the ſteel decompoſes the water by its own attractive power, and paſſes to the ſtate of the black oxide, which cannot be diſſolved by diluted vitriolic acid. Hence, inſtead of carburet of iron, the reſidue conſiſts of a mixture of this ſubſtance with the black oxide of the metal.

3. The duration of the experiment is likewiſe of conſiderable inportance. It may ariſe from two cauſes. The firſt is the concentration of the acid. In this caſe, the ſulphate of iron which is formed abſorbs for its cryſtallization the ſmall quantity of water which remains, with which it falls down, and entirely ſtops the ſolution. The ſecond is the too conſiderable weakneſs of the acid, the particles of which being ſeparated too far from thoſe of the metal by the water which retains them, cannot in this caſe act but very ſlowly.

I am well convinced that whenever the carburet of iron remains in contact with the metallic ſolution, particularly when it contains diſengaged acid, it is ſubject to remarkable changes. Its black colour becomes yellow, or yellowiſh grey, and it diminiſhes in volume by the actual loſs of part of its maſs. From theſe obſervations it follows, that in order to obtain the greateſt poſſible quantity of carburet of iron from any kind of ſteel, the acid muſt be neither too much nor too little concentrated. I have obſerved that the beſt proportion between the acid and the water was one of the former to five of the latter, and that coarſe filings or turnings of ſteel were preferable to fine filings or pieces of conſiderable ſize.

Moſt of the inconveniences here mentioned do not exiſt with regard to the muriatic acid, at leaſt in ſo evident a manner; becauſe it is never ſo concentrated as the ſulphuric acid, has a ſtronger affinity for the oxide of iron, and forms with this metal a very ſoluble ſalt. But it cannot be employed to determine, in one and the ſame operation, the proportions of carburet of iron and of phoſphorus; becauſe it decompoſes the phoſphate of iron, and renders it neceſſary to uſe a cauſtic alkali to precipitate it by double affinity, which may be attended with ſome inconvenience.

The method we have propoſed to determine the quantity of carbone contained in ſteel, is therefore preferable to this in every reſpect.

SECTION VIII.

Reflections on the Means propoſed by Bergman to diſcover the Preſence of Phoſphorus in Iron and Steel.

BERGMAN, in his Diſſertation on the Analyſis of Iron and Steel, does not ſpeak of the preſence of phoſphorus or ſiderite, which he found only in cold ſhort iron.

It is nevertheleſs very unlikely, that, among the very numerous ſamples of iron and ſteel which he examined, there ſhould have been none which contained phoſphorus, ſince the four ſteels which conſtitute the ſubject of this memoir all preſented quantities very conſiderable, though they appeared of ſufficiently good quality for the uſes in the arts. I ſuſpect, therefore, that this chemiſt's not having found it was owing to a want of attention to the probability of its exiſtence, or elſe, that the means he made uſe of were not adapted to detect its preſence.

The following is the method he propoſes for this purpoſe, in his Diſſertation on the cauſe of the brittleneſs of cold ſhort iron:

He takes a bottle A, of the capacity of about 12 cubic inches, into which he puts 16 loths of crude iron, which affords the cold ſhort iron. Upon the iron he pours ſix cubic inches of diſtilled water, and half an inch of concentrated ſulphuric acid. As ſoon as the efferveſcence is ended, he filters the liquor, and receives it in another bottle B, of the ſame contents as the firſt. He waſhes the reſidue in the bottle A, until the filtration fills the bottle B. This being done, it is obſerved that the fluid in B, which at firſt was clear, becomes turbid and white. The powder does not ſettle until after ſeveral hours. He pours on the iron which remained in the bottle A a ſecond quantity of water and acid equal to the firſt; when the efferveſcence is over, he pours the liquid into a third bottle C. He repeats this manœuvre a third and fourth time, &c. and collects the liquors in the veſſels marked D, E, &c. The phenomena were the ſame in each of theſe operations; but the fifth ſolution in F remained clear for ſeveral weeks, at the end of which it formed a ſlight cloud. The ſixth ſolution depoſited nothing in the ſame ſpace of time, though much iron remained undiſſolved.

I repeated this proceſs on the ſteels treated in this memoir, and did not diſcover the leaſt trace of phoſphate of iron, though they would have afforded conſiderable quantities by the method I have before propoſed. This juſtifies the aſſertion that Bergman did not find this ſubſtance in the different irons he analyſed, becauſe his method was inſufficient.

I prove its inſufficiency as follows:—Iron and phoſphorus united together, and brought into contact with acidulated water, both tend to unite with oxygene by virtue of a prediſpoſing attraction between the acid which is added, and that which is formed, towards the oxide of iron, whence the reſult is ſulphate and phoſphate of iron.

This laſt is inſoluble in water; but it has an affinity for acids, by virtue of which it combines, and becomes ſoluble in water. This affinity of the phoſphate of iron for acids is ſufficient to impede and even ſuſpend the ſolution of new quantities of iron; ſo that it is not without difficulty, and after a length of time, that the phoſphate of iron can be completely ſeparated from the ſulphuric acid by means of metallic iron.

It happens therefore in every caſe when iron or ſteel which contains phoſphorus is diſſolved in an acid, that the acid divides itſelf into two parts, one of which unites with the oxide of pure iron, and the other with the phoſphate of iron as it is formed; and that when the acid is ſaturated by the oxide of iron and the phoſphate of iron, the ſolution is almoſt totally ſtopped, though the liquor reddens blue vegetable colours. It may be hence inferred, that the ſulphuric acid, united with the phoſphate of iron, no longer exerts an equal power on the oxide of iron; and that the metal does not decompoſe the water, nor unite to the oxygene of this ſubſtance but by its own peculiar force, which occaſions a retardation or even a complete ſuſpenſion of the ſolution. Now, if in ſuch a ſolution the phoſphate of iron ſhould be ſmall in quantity, no ſign of this metallic ſalt will appear, even by the addition of a great quantity of water: and this is what happens in the proceſs of Bergman; whereas, by adding an alkaline carbonate to this ſolution until it ceaſes to effervеſce, the alkali unites with the ſulphuric acid; and the phoſphate of iron, whatever may be its quantity, falls down in the form of a white powder.

SECTION IX.

Reflections on the Inſufficiency of the Methods hitherto propoſed to diſcover and ſeparate Manganeſe from Iron.

BERGMAN is the firſt who found manganeſe in iron; and almoſt every kind of iron which he examined afforded quantities more or leſs conſiderable. The maximum of this quantity in ſteel, according to this chemiſt, amounts to 0,30, and the minimum to 0,005, a latitude extremely great, and which appears ſcarcely probable, as we ſhall proceed to ſhew.

In order to detect manganeſe in iron or ſteel, the profeſſor of Upſal propoſes two methods. The firſt, having for its object to aſcertain the preſence of this metal, and being ſpeedy, cheap, and eaſy to be performed, may be called explorative. It conſiſts in throwing into five parts of fuſed nitrate of pot-aſh, one part of filings of iron or ſteel, and to bring the maſs into ſtrong fuſion after the detonation has taken place. If the crucible when cold exhibits towards its ſuperior edge a vitreous circle of a greeniſh blue colour, it is, according to him, a ſure ſign of the exiſtence of manganeſe in the iron. The other method may be called determinative, becauſe it is not uſed until it has previouſly been aſcertained that the iron contains mangaueſe, and that nothing remains but to find its quantity. It is performed by diſſolving in the nitric acid a known weight of iron or of ſteel, containing manganeſe, and, after having evaporated the ſolution, by ſtrongly calcining the ferruginous reſidue, which is treated with more diluted nitric acid, into which a certain quantity of ſugar has been put. By this means (ſays Bergman) the manganeſe will be diſſolved, and the oxide of iron will remain untouched.

Theſe proceſſes, carefully repeated, did not afford me the ſame reſults. I muſt even ſay that they are falſe, and capable of leading to erroneous concluſions. In fact, with regard to the firſt, it is known that nitrate of pot-aſh, decompoſed in a crucible of pure earth or fine ſilver, leaves the pot-aſh in the form of a maſs of a greeniſh or blueiſh colour. It is in vain to object, that this colour is owing to the manganeſe contained in the pot-aſh of commerce; for this alkali, when purified by alcohol, preſents the ſame phenomena. And even ſuppoſing it to be a certain ſign of the preſence of manganeſe, it would alſo prove a ſource of error, becauſe the nitrate of pot-aſh, and pot-aſh alone, exhibit this colour by fuſion.

Pure iron itſelf might produce deception in this reſpect; for, when it has been oxided by the nitrate of pot-aſh, it combines by the aſſiſtance of the alkali with the ſiliceous matter of the crucible, and forms a glaſs which has often a greeniſh colour. Common crucibles alſo are not without inconvenience; for they may contain the oxide of manganeſe, which is well known to be often mixed with earthy matters.

The ſecond method of Bergman is attended with ſtill more danger of error than the firſt, by fixing the opinion of the exiſtence of manganeſe, and producing a deciſion with regard to its ſuppoſed quantity.

Bergman, who was perſuaded that the nitric acid mixed with ſugar does not diſſolve (the oxide of) iron, muſt frequently have attributed appearances to manganeſe which really aroſe from iron only; for we have proved, that the vegetable acid which is formed in this operation diſſolves a conſiderable quantity.

III.

Extract of a Letter from Mr. HUMBOLDT to Mr. BLUMENBACH, containing new Experiments on the Irritation cauſed by the Metals with Reſpect to their different Impreſſions on the Organs of Animals [*].

MR. HUMBOLDT is one of the philoſophers who has made the moſt numerous obſervations on the phenomenon diſcovered by Galvani concerning the irritability produced by the contact of different metals with the parts of animals in which the principle of life is apparently extinguiſhed. As long ago as the year 1795 he obſerved that the animal irritability was augmented by the oxygenated muriatic acid. Not having diſcontinued his attention to this object, the peruſal of the phyſiological writings of Reil, and his correſpondence with Scarpa and Volta, afforded him indications for new enquiries, of which he has occaſionally had the courage to make himſelf the ſubject.

---

[*] This Letter forms part of Gren's (German) Journal of Natural Philoſophy for the month of October laſt. The extract was read to the Inſtitute of France, by Citizen Guyton.

“In a converſation,” ſays he, “with M. Scarpa, at Pavia, on the effects which Galvaniſm produced upon myſelf, nothing ſurpriſed him more than the appearance of a lymphatic and ſerous humour on my back. ‘What can be the nature,’ ſaid he, ‘of this ſtimulant, which in a few inſtants changes the nature of the veſſels to ſuch a degree as to cauſe them to prepare humours, which, the inſtant they touch the epidermis, excite inflammation, and mark their courſe by a redneſs which laſts for whole hours?’” M. Humboldt promiſed to repeat the experiment; and the account he gives of the facts conſtitutes one of the moſt intereſting articles in his letter.

For this purpoſe he cauſed two bliſtering plaſters to be applied on the deltoid muſcle of both ſhoulders. When the left bliſter was opened, a liquor flowed out which left no other appearance on the ſkin than a ſlight varniſh, which diſappeared by waſhing. The wound was afterwards left to dry up: this precaution was neceſſary, in order that the acrid humour which the Galvanic irritation would produce might not be attributed to the idioſyncriſis of the veſſels. This painful operation was ſcarcely commenced on the wound, by the application of zinc and ſilver, before the ſerous humour was diſcharged in abundance: its colour became viſibly dark in a few ſeconds, and left on the parts of the ſkin where it paſſed traces of a brown inflamed red. This humour having deſcended towards the pit of the ſtomach, and ſtopped there, cauſed a redneſs of more than an inch in ſurface. The humour, when traced along the epidermis, left ſtains, which after having been waſhed appeared of a blueiſh red. The inflamed places having been imprudently waſhed with cold water, increaſed ſo much in colour and extent, that Mr. Humboldt, as well as his phyſician Dr. Schalleru, who aſſiſted at theſe experiments, entertained ſome apprehenſion for the conſequences.

Mr. Humboldt has not undertaken to determine the nature of the fluid which produces ſuch aſtoniſhing effects; but he applies himſelf to circumſcribe the phenomena in the real circumſtances which produce them. He judiciouſly varies the preparations, and carefully notes all the reſults; being perſuaded that the cauſe of Galvaniſm cannot be explored with ſucceſs, but by obſerving the proportions in which the chain of metals either irritates or has no effect: and to extend ſtill more this vaſt field of obſervation, he employs various means to raiſe or diminiſh the irritable capacity of the animal organs.

What is the ſenſation which the Galvanic irritation produces? Mr. Humboldt has diſcuſſed this queſtion. “No one (ſays he) can ſpeak more decidedly on this ſubject than myſelf, having made ſeveral experiments on my own perſon, the ſeat of which, in ſome inſtances, was the ſocket of a tooth which I had cauſed to be extracted; in others, certain wounds which I made in my hand; and in others, the excoriations produced by four bliſtering plaſters.” The following is his anſwer:

The Galvanic irritation is always painful, and the more ſo in proportion as the irritated part is more injured, and the time of irritation more prolonged. The firſt ſtrokes are felt but ſlightly; the five or ſix following are much more ſenſible, and even ſcarcely to be endured, until the irritated nerve becomes inſenſible from continued ſtimulus. The ſenſation does not at all reſemble that which is cauſed by the electric commotion and the electric bath; it is a peculiar kind of pain, which is neither ſharp, pungent, penetrating, nor by intermiſſions, like that which is cauſed by the electric fluid. We may diſtinguiſh a violent ſtroke, a regular preſſure, accompanied by an unintermitting glow, which is incomparably more active when the wound is covered with a plate of ſilver, and irritated by a rod of zinc, than when the plate of zinc is placed on the wound, and the ſilver pincers are uſed to eſtabliſh the communication.

When the communication is made by the contact of the epidermis, it produces no effect; it appears to inſulate like glaſs, when interpoſed between the wound and the metal: but if the ſkin be removed, by making two wounds at eight inches diſtance, and a plate of zinc be placed on one of them, and on the other a leg of a frog prepared, this laſt is ſeen to contract itſelf when it communicates with the zinc by the ſilver wire; which proves that the Galvanic fluid then paſſes beneath the epidermis.

This fluid produced in ſome circumſtances a very ſenſible acid taſte. The two wounds of Mr. Humboldt having been covered, one with ſilver, the other with zinc, an iron wire of ſeveral feet in length, attached to the zinc, was conveyed between his upper lip and the ſpongy ſubſtance of the teeth, and thence to the tongue of another perſon. When the iron wire was made to touch the ſilver, a ſtrong contraction of the ſcapular muſcle took place, and at the ſame inſtant the perſon whoſe tongue formed part of the chain of communication perceived the ſenſation of acidity. There are alſo caſes in which the fluid acts on the organs of taſte without producing any ſenſible effect on the organs of motion: ſuch is that where the epidermis ſerves as the conductor from zinc to the frog; for there is not then any contraction, but merely an acid taſte on the tongue.

The author, having learned from Mr. Volta that he employed the ſolution of pot-aſh *(oleum tartari per deliquium)* in order to augment the conducting power, availed himſelf ſucceſsfully of this means to raiſe the capacity of the animal organs. He moiſtened one of his wounds with this liquor, which produced little pain; but the Galvanic irritation was more violent, and accompanied with more heat; ſparks appeared and diſappeared before his eyes; the tongue moiſtened with the ſame diſtinctly perceived the acid ſenſation, although the communication was eſtabliſhed only between zinc and zinc. The thigh of the frog, moiſtened with the alkaline ſolution and laid upon a plate of glaſs, without touching either metal or carbonic matter, fell of itſelf into violent convulſions, the antagoniſt muſcles of the legs and toes being inceſſantly agitated. Irritability has been re-eſtabliſhed by this application in the animal parts, where it had been extinguiſhed by warm ſolutions of the oxide of arſenic. Laſtly, the irritation (which does not commonly take place when the nerve and the muſcle are armed with the ſame metal, the different metals being between the coatings) becomes manifeſt after this preparation; which ſeems to indicate that the alkali not only irritates the nerve, but likewiſe adds to its irritability.

The author applied this method to amphibious animals, which he rouſed from their winter's ſleep, and in which he perceived a peculiar ſymptom of irritability.

Theſe obſervations led him to diſtinguiſh two ſtates of the animal organ. The firſt, of irritability naturally or artificially raiſed or excited; the ſecond, irritability in a leſs degree. Theſe two ſtates, which he calls *poſitive* and *negative*, are merely, as he remarks, different degrees, and not phenomena abſolutely diſtinct from each other.

In individuals naturally ſenſible, the effects produced by alkaline ſolutions, by the oxygenated muriatic acid, by the ſolution of oxide of arſenic, are very rarely of the ſame intenſity.

In the caſe of increaſed irritability, muſcular motions are obſerved without metal or carbonic matter. They may be obtained with metals, though without communication between the nerve and the muſcle; that is to ſay, without the regular connection or chain. They may be alſo obtained by forming the chain of ſimilar metals.

Let the crural nerve of an animal naturally tenacious of life be placed upon glaſs. Let a ſmall piece of freſh muſcular fleſh be fixed on a ſtick of ſealing-wax, and then brought into contact with the crural muſcle. The reſult will be a violent convulſion at the inſtant when the chain of communication is completed. The ſame thing happens if, inſtead of the ſmall piece of muſcular fleſh, a detached piece of the crural nerve be fixed on the ſtick of ſealing-wax. The connection is therefore formed of two things, nerve and muſcular fibre. How in this ſimple caſe can the fluid which paſſes from the nerve into the muſcle cauſe it to be contracted? Mr. Humboldt thinks that it becomes ſtimulant, merely becauſe it returns from the nerve into the nerve by a foreign animal matter; that is to ſay, not organically connected with the nerve.

The diſparity of the metals forming the chain has hitherto appeared as a neceſſary condition to produce Galvanic irritation. This hypotheſis, however, is overturned by the experiments of Mr. Humboldt. If it be true that, in the ſtate of leſs irritability, there is very rarely contraction with ſimilar metals (as Volta affirms, contrary to Aldini), this circumſtance becomes indifferent in the caſe of increaſed irritability. Mr. Humboldt put into a china cup ſome mercury exactly purified; he placed the whole near a warm ſtove, in order that the entire maſs might aſſume an equal temperature: the ſurface was clear, without the appearance of oxidation, humidity, or duſt. A thigh of a frog, prepared in ſuch a manner that a crural nerve and a bundle of muſcular fibres of the ſame length hung down ſeparately, was ſuſpended by two ſilken threads above the mercury. When the nerve alone touched the ſurface of the metal, no irritation was manifeſted; but as ſoon as the muſcular bundle and the nerve touched the mercury together, they fell into convulſions ſo briſk that the ſkin was extended as in an attack of tetanus.

We ought not to be ſurpriſed at the precaution here taken by Mr. Humboldt to heat the mercury. This is required in conſequence of the opinion which he announces, that the parity of the metals does not depend on the homogeneity of their chemical conſtituent parts, but of their heat, poliſh, hardneſs, and form.

Gold, placed between two armatures of zinc, produces irritation only when the gold is moiſtened by ſome volatile fluid, or by the moiſture of reſpiration.

Laſtly, Mr. Humboldt has attempted to include all the caſes in the following formulæ:

1. In the State of increaſed Irritability.

Poſitive caſes.
- Frog—muſcular fleſh.
- Frog—zinc—zinc.
- Frog—zinc—muſcular fleſh—ſilver.
- Frog—zinc—ſilver—zinc.
- Frog—muſcular fleſh—ſilver—zinc.
- Frog—zinc—muſcular fleſh—ſilver—muſcular fleſh—zinc.

2. In the State of diminiſhed Irritability.

Poſitive caſes.
- Frog—zinc—ſilver.
- Frog—zinc—muſcular fleſh—ſilver—zinc.
- Frog—zinc—muſcular fleſh—ſilver—muſcular fleſh—ſilver—zinc.

Negative caſes.
Frog—zinc—zinc.
Frog—zinc—muſcular fleſh—ſilver.
Frog—zinc—muſcular fleſh—ſilver—zinc.

Mr. Humboldt finiſhes this letter by ſome obſervations which he has collected in the courſe of his experiments on the *ſthenic* or *aſthenic* virtue of chemical agents; that is to ſay, their energy or their inefficacy to produce irritation. Alkalis appear to be to the ſenſible fibres what acids are to muſcular groups. The muriatic acid augments the iritability of the muſcle while it extinguiſhes that of the nerves, which does not re-appear even after the acid has been ſaturated with alkali.

By continuing to bathe the nerve with an alkaline ſolution, an entire atony is at length produced by exceſs of irritation; but if a few drops of muriatic acid be let fall on the part, the irritability is re-eſtabliſhed.

A thigh of a frog, irritated even to total relaxation by a warm ſolution of oxide of arſenic, has exhibited new convulſions, after having been immerſed for two minutes in a ſolution of pot-aſh.

The ſthenic virtue of the oxygenated muriatic acid is not leſs remarkable. Thighs of frogs naturally flaccid, and weakened ſtill more by the Galvanic proceſs for ſeven hours, which afforded no ſign of motion when ſilver ſerved as a conductor between zinc and the nerve, exhibited violent contractions when the nerve was moiſtened with oxygenated muriatic acid. The author refers to this ſubject the experiment which he publiſhed in 1793, in his *Flora Fribergenſis*, by which it is aſcertained that ordinary muriatic acid retards the germination of plants, but that oxygenated muriatic acid had cauſed a plant to germinate in ſeven hours, which required thirty-eight in pure water in order to arrive at the ſame development. This fact appears to him to indicate ſome relation between the vegetable and animal organization.

A judgment may be formed from this extract of the number of important facts contained in this letter, and of the intereſt they will excite, when they ſhall be collected, arranged, and amplified, in the large work which the author is preparing.

IV.

Uſeful Notices reſpecting various Objects[*]*.—Methods of cloſing wide-mouthed Veſſels—Preſervation of Gunpowder—Granulation of Shot—Precipitation of Magneſia.*

1. *Methods of cloſing wide-mouthed Veſſels.*

THE means of cloſing the apertures of large jars and other ſimilar veſſels, ſo as to render them air-tight, has long preſented, to perſons who are engaged in philoſophical purſuits, an object of ſome intereſt. Thoſe in particular who are in the habit of preſerving animal ſubſtances in ſpirit for anatomical or phyſiological cabinets, find that, notwithſtanding all the care which can be taken, the evaporation of the liquor in which they are immerſed is a ſubject which requires conſtant attention. Perhaps nothing with which we are acquainted is more convenient and proper for ſtopping ſmall circular apertures, whoſe interior ſurfaces are ſmooth enough to admit of their uſe, than common phial-corks: for a certain time at leaſt, where the contents of the veſſel are not corroſive, they fit more cloſely than moſt of the glaſs ſtopples which are uſed for this purpoſe. Where however the area of the aperture is conſiderable, the poroſity of cork and bladders, the neceſſity of uſing veſſels whoſe mouths are elliptical, and the unavoidable irregularities in the form of the ground glaſs covers, which are beſides of conſiderable expence, are circumſtances which render the ſolution of this problem leſs eaſy than might be wiſhed. The cloſeneſs of texture which characteriſes metallic ſubſtances ſeems to afford the beſt means of removing theſe difficulties; and a quantity of an amalgam of tin is accordingly in ſome muſeums ſpread over the edges of the covers, by which means the miſchief is in a good meaſure prevented: but perhaps no method of effecting this end is ſo applicable to general purpoſes, as that which conſiſts in the uſe of tin foil, which is alſo in frequent practice. It may be applied in two ways: a piece of bladder, ſoaked in warm water, having been ſtretched tightly over the mouth of the jar, and tied, a piece of the thickeſt tinfoil, previouſly examined by interpoſing it between the eye and the ſun, in order to detect the ſmall fiſſures which are frequently found in it, is to be laid ſmoothly over it with the palm of the hand, without ſtretching, and, that being alſo tied, a ſecond piece of bladder again ſtretched over it: or, where the veſſel may require to be ſometimes opened, the foil may be laid ſmoothly over the ſurface of a bung, and, a piece of bladder being ſtretched over it, the whole applied in the uſual way.

---

[*] The whole of the preſent article was received from my anonymous correſpondent, J. F—:—:—:—r, whoſe former communications have, no doubt, engaged the attention of the reader.

The following ſeems to be another effectual method of cloſing a jar:—Let *a b* (Plate XII. Fig. 1.) be a ſection of its mouth; *c d*, that of a circular rim of tin, A, an inch high, whoſe internal diameter is half an inch larger than the exterior diameter of the mouth of the veſſel; *e f*, that of a common tin cover, B, whoſe diameter is a quarter of an inch leſs than that of the tin rim, A. The rim being cemented round the neck of the jar with ſealing-wax, or the common electrical cement, and the interval between it and the neck filled to the depth of two-tenths or a quarter of an inch, with olive-oil *, it is clear, that if the jar be placed horizontally, and the cover B put on, it becomes hermetically ſealed, except in the event of an elaſtic fluid making its way through the oil. The only care neceſſary will be, not to put ſo much oil into the tin rim as to occaſion it either to flow over or into the jar, on a change taking place in the preſſure of the atmoſphere. The mouth of the veſſel ſhould alſo be ſtopped, to prevent any of the oil from being thrown into it on removing the cover.

In the laboratory, a thouſand uſes of theſe modes of preſerving different ſubſtances muſt occur; and it ſeems not improbable that, if they were extended to the purpoſes of ordinary life, and glaſs veſſels ſubſtituted for the porous earthen-ware at preſent in uſe, ſeveral articles of culinary preparation, ſuch as pickles, preſerved fruits, potted meats, and the like, would receive leſs injury from the effects of time than they are now found to do.

---

* Lavoiſier uſed mercury for a ſomewhat ſimilar purpoſe, but its depth was conſiderably greater. It ſeems, from ſome experiments of Prieſtley's, in which he found his gaſes contaminated with atmoſpheric air, though the tubes had been immerſed to the depth of an inch or more in this fluid, that, in a thin ſtratum, oil would be more effectual. F.

If this conſtruction, as I underſtand it, be ſuch as to admit the oil into contact with the reſinous cement, this laſt will ſoon become ſoft by the proceſs of ſolution. To prevent this effect, I ſuppoſe it would be convenient to faſten the rim by a mixture of plaſter of Paris and white of egg. N.

2. *Preſervation of Gunpowder.*

GUNPOWDER, by reaſon of the nitre which enters into its compoſition, having been partially deprived of its water of cryſtallization, and the known attraction of charcoal for humidity, is always ſomewhat diſpoſed to deliqueſce; and although it does not actually liquefy, or become unfit for ſome of the purpoſes to which it is applicable, yet, for thoſe of the ſportſman, to whom the quickneſs of its communication is of the higheſt conſequence, it is generally in a ſtate very inferior to that in which it would be found, if a greater degree of care was taken in its preſervation. It is only when it has received but a very ſlight injury from damp, that the miſchief is capable of a remedy: when once it has become at all concreted, drying it will no longer reſtore its power: the nitre will be found, on examination with a magnifier, to have cryſtallized, and the ſtrength and quickneſs of the powder are conſiderably and permanently impaired; probably even before this ſymptom has appeared. It is evident that no veſſel is ſufficiently cloſe to prevent this circumſtance from taking place, but ſuch as is perfectly air-tight. There cannot perhaps be a much ſtronger proof of the inſufficiency of the packages in general uſe for this purpoſe, than the opinion of a conſiderable dealer in this article, to whom the matter was lately mentioned. He ſaid he was convinced that powder would be found to "give" in ſome ſtates of the weather, though the veſſel which contained it was ever ſo cloſe: a notion which may perhaps have contributed to prevent the adoption of more effectual means. He added, that it is found to do ſo in the tin caniſters, as much as when packed in brown paper. The remedy is however extremely eaſy. Nothing more is neceſſary, than to cut off the communication with the atmoſphere: any veſſel in which ſalt of tartar can be preſerved dry, will of courſe keep gunpowder in the ſame ſtate of perfection as when firſt encloſed. For a quantity not exceeding a few pounds, which is not intended to be frequently removed from place to place, common ten or twelve-ounce phials anſwer extremely well; and if half a dozen of them be put into a caſe, there cannot perhaps be a more convenient magazine. They ſhould be filled as full as poſſible, and the powder well corked up at the mills, the corks being tied over with bladder and tinfoil. As, however, there might be ſome danger of exploſion from the accidental fracture of one of theſe, if this method were to be adopted for large quantities, it would in that caſe be neceſſary to uſe ſome other material than glaſs; and if, inſtead of the ſlider now inſerted into the tin caniſters, a turned braſs or pewter neck, like that of a common phial, and capable of being likewiſe ſtopped with a ſmall cork, were ſoldered into the top, they would alſo anſwer as well. A projection would perhaps render them inconvenient for package; and it would therefore be proper that the neck ſhould be ſunk into the top; and, in order to get out the contents, that it ſhould be let into a ſemi-cylindrical hollow in the ſide of the caniſter. When corked up, the top of the cork might be cut off, and the whole aperture covered with a plaſter of thick drying paint, or wax and turpentine, ſpread on a piece of tinfoil. None of the flaſks, the beſt of which are thoſe of copper or tin, are fit for preſerving the powder longer than whilſt they are in uſe, during which the charger ſhould be kept corked; a precaution the effects of which will be found conſiderable.

There are ſome, perhaps, who may not conceive theſe remarks to be very materially conducive to the general intereſts of philoſophy; but he to whom it has frequently happened to miſs an excellent croſs ſhot, from his powder hanging fire—*quæque ipſe miſerrima vidi*—will ſcarcely conſider this as the leaſt important article in the September Journal.

3. *Gra-*

3. *Granulation of Shot.*

THE manufacture of common fowling ſhot conſiſts merely in cauſing the fuſed metal to fall in equal ſpherical drops into water. The lead is melted with the addition of a ſmall proportion of arſenic, which, being reduced to a metallic ſtate, by means of greaſe ſtirred in during the fuſion, renders it leſs fluid. An oblong ſhallow veſſel of iron, perhaps 10 inches wide, 14 long, and 2½ deep, called a *card*, whoſe bottom is pierced with holes proportionate to the intended ſize of the ſhot, is placed at the height of from one to three inches, over the ſurface of a tub of water, covered with a thin film of oil. The card is previouſly heated to the temperature of the metal by immerging it in the cauldron; and a ſtratum of ſoft droſs or ſcoriæ, which are found on the ſurface of the fuſed alloy, is then placed on its perforated bottom, and, being ſlightly preſſed down with the ladle, forms a kind of filter, which partly chokes up the apertures, and prevents the metal from flowing through them in continuous ſtreams. The fuſed metal is then poured by ladlefulls into this veſſel, and appears notwithſtanding to run through it with conſiderable velocity; ſo that it ſeems difficult to believe that it falls in ſeparate drops, till convinced by taking up a quantity of ſhot from the bottom of the water.

The ſhot thus made is not without conſiderable imperfections. The exterior coat of the lower part of the drop becoming ſuddenly fixed by the contact of the water, its ſuperior portion, which is ſtill liquid, as it alſo cools and contracts, neceſſarily pits, like the ſurface of metal in the channel of a mould, ſo that the greater part of the ſhot are ſomewhat hollow and of an irregular form; conſequently too light for the purpoſe to which they are deſtined, and liable to unequal reſiſtance in their paſſage through the air. Theſe defects are remedied in the patent ſhot, the manufacture of which differs only from that of the preceding kind in the addition of a larger portion of arſenic, which varies according to the quality of the lead; *in dropping it from ſuch a height that it becomes ſolid before it enters the water*, which is from 40 to 100 feet; and in ſome ſubſequent operations, which are as follows: It is firſt dried and ſifted. It is then *boarded*, which conſiſts in ſcattering it on ſeveral poliſhed ſlabs or trays of hard wood, with rims, in the form of a Π, except that the ſides converge towards the lower part, to which a ſlight inclination and alternate motion in their own planes are given by boys employed in the manufacture. The ſhot whoſe form is imperfect are detected by the ſluggiſhneſs of their motion, and remain behind, whilſt the others roll off from the board. The laſt operation is the poliſhing; which is performed by agitating it, with the addition of a very ſmall quantity of black lead, not exceeding two ſpoonfuls to a ton, in an iron veſſel, turning on an horizontal axis, like a barrel-churn. It does not appear that any higher degree of perfection than that which is thus attained remains to be deſired. The argentine brilliancy of the ſhot when newly made, the beautiful accuracy of its form, and the curious inſtance of inanimate tactics which it preſents when ſcattered on a plate, render it even an agreeable object of contemplation.

4. *Precipitation of Magneſia.*

THE moſt ſtriking characters of common magneſia, and thoſe which are moſt relied on by a purchaſer who has not the means of analyſis at hand, as indicative of its purity, are its levity and impalpability: it is therefore a matter of ſome importance to thoſe who deal in this article, that it be made to poſſeſs theſe qualities in as high a degree as is poſſible. It had long ſince occurred to the writer of this, that the effects which are attributed, by all who treat on the ſubject, to a very ſmall quantity of other earths in the alkali which is uſed in the precipitation, were ſomewhat diſproportionate to the aſſigned cauſe, and that a part of them were probably rather owing to a deficiency of carbonic acid *. An accidental piece of information which he received lately from a practical man, that magneſia was always to be obtained " beautifully light" by the addition of a ſmall proportion of ſal ſodæ to the vegetable alkali employed, and a very looſe experiment which he has ſince made with a view to this object, appear to corroborate ſuch an idea. The magneſia contained in four ounces of Epſom-ſalt was precipitated with a filtered ſolution of common pearl-aſh, waſhed, dried, and a portion of it then re-diſſolved by vitriolic acid, and again precipitated with the ſame alkali, with the addition of one-fourth of carbonate of ſoda. The powder was certainly more light and impalpable after the ſecond precipitation. An addition of carbonic acid to the alkaline ſolution employed, will probably operate in two ways: it will not only render the magneſia lighter, but in ſome degree actually purer, by precipitating the aluminous and ſiliceous earths before held in ſolution by the pot-aſh in a more cauſtic ſtate. In this reſpect, and in this only, perhaps, if a ſufficiently ſmall quantity of water be uſed, the aqua kali of the preſent Pharmacopœia is inferior to the oil of tartar per deliquium of the old ones. There is poſſibly a limit to the proportion of this ingredient, which can be admitted into the proceſs with a due regard to economy; perfectly neutraliſed carbonate of magneſia being by no means inſoluble. If an alkali, in an highly efferveſcent ſtate, be added to a weak ſolution of any magneſian ſalt, it is well known that no precipitation whatever will take place. What remained in the ſupernatant liquor might, however, if thought of ſufficient value, be afterwards precipitated with a cauſtic alkali, and reſerved for calcination; or indeed would of itſelf ſubſide during the ſubſequent evaporation for obtaining the vitriolated tartar. The beſt proceſs in all reſpects may be eaſily aſcertained by experiment, and the matter appears to deſerve it.

V.

On the Elaſtic Fluid contained in the Air-Veſſels of Fiſh.

DR. Francis Rigby Brodbelt, of Jamaica, in a letter to Dr. Duncan †, gives the following account of ſome obſervations and experiments which he has made on the gas contained in the air-bladder of the ſword-fiſh:

" I will relate to you a few experiments which I made during my paſſage to this iſland. I had often wiſhed to determine what is the nature of the gas which is contained in the air-bladder of fiſh; and I was perhaps prevented from finding it out, by hearing Dr. Monro in his lectures ſay, it was natural to ſuppoſe it fixed air. However, although this authority prevented me from putting it to the teſt of experiment for ſome time, yet one day, on our voyage, having caught a very large ſword-fiſh, I collected the contents of all the air-bladders; for in that fiſh the bladder appeared divided into innumerable cells, which had no communication with each other. They afforded ſo much air that I collected a quart bottle full. My ſurpriſe was great to find that the gas contained was oxygene. A flame was brightened, an ignited ſtick was made to re-kindle, and it was ſo ſtrong and pure, that the common experiment of a piece of ſteel wire, heated and put into it, ſucceeded well, and threw out a moſt vivid light when melting. I have committed to writing my thoughts on this ſubject at greater length, and I wiſh to infer that this pure air is to ſerve the purpoſes of life when the animal is far below the ſurface of the water."

---

* I find, on examination of the common magneſia with a deep magnifier, that its levity proceeds from an actual radiated cryſtallization, like that of ſnow: its form may be advantageouſly ſeen when juſt ſeparating into flocks in a glaſs tube. Hence perhaps it would be improved by the addition of the alkali in very ſmall portions, at intervals. Mere waſhing, by deſtroying the ramifications of the cryſtals, conſiderably augments the ſpecific gravity of magneſia: an effect which would perhaps be beſt prevented by performing this operation carefully with diſtilled water, previouſly boiled on a portion of it more than ſufficient for its ſaturation. F.

† Annals of Medicine, by Drs. Duncan, for 1796, p. 393.

The preceding diſcovery of oxygene is, I believe, perfectly new. Dr. Prieſtley, in his Experiments in Natural Philoſophy abridged and methodized, vol. ii. page 462, mentions the commencement of a courſe of experiments on the ſtate of the air which is contained in the bladders of fiſhes. He remarks, that when theſe are taken out of the fiſh, the air cannot be got from them by preſſure through any exiſting aperture, but that he was always obliged to cut or burſt them. The firſt time that it occurred to him to examine the air contained in theſe bladders, he found it in a great number of them to be not at all affected by nitrous air. But at another time he found air from the ſame kind of fiſh, namely, roaches, to be ſlightly affected by that re-agent. It appears, therefore, from theſe trials, that he ſeldom met with oxygene, and then in ſmall quantity; but what the other portion of air might conſiſt of was not aſcertained by his experiments.

Fourcroy afterwards made experiments on the air contained in the air-veſſel of the carp, which, at certain ſeaſons, he affirms, may be had very cheap and in abundance at Paris. It was perfectly pure azotic gas, for the moſt part, though ſometimes it contained a ſmall quantity of carbonic acid gas. He thinks, from the nature of this fluid, that the air in the bladders of fiſhes is produced in the ſtomach. (Ann. de Chim. I. 47.) The obſervations of Dr. Brodbelt ſeem to render this general concluſion at leaſt doubtful.

VI.

Account of certain remarkable Changes of Colour and Direction of the Clouds during a Thunder-Storm.

AMONG other circumſtances enumerated by Dr. Prieſtley in the deſcription of the clouds, in a thunder-ſtorm, in his Hiſtory of Electricity, mention is made of a certain luminous appearance, evidently independent of ſolar reflection. I have always ſuppoſed this expreſſion to denote the opake whiteneſs of the upper or arched outline of certain thunder-clouds, contraſted with others apparently in contact with them, but of a dull leaden hue; and accordingly I was diſpoſed to conclude that the whole was an optical deluſion, ariſing from the poſition of the ſpectator, who imagined, though falſely, that the latter clouds were as much expoſed to the ſun's direct light as the former. But the ſtorm which happened on Sunday morning, the 30th of July laſt, exhibited facts which ſeem to ſhew that the tranſition of electricity may cauſe the clouds to emit a ſteady permanent light, very different from the ſudden flaſh called lightning.

I was called at five o'clock in the morning. The ſky was then covered with clouds, not very denſe except to the ſouth, and flying with great rapidity to the W. by S. or W. S. W. It lightened very frequently in the N. W. and S. W. quarters, by doubled and trebled flaſhes of a bright illumination (for the actual flaſh was not ſeen), with very loud thunder, uſually at the interval of 11 or 12 ſeconds after the flaſh. The lower prominences, or ragged extremities of the clouds, were conſtantly tinged with red, and I was informed that they had been very much redder before I got up.

At about ten minutes after five, no rain having fallen, but a few heavy drops, a ſudden darkneſs came on, and the duſt roſe in Newman-ſtreet, where I reſide, beginning at the ſouth end, about 250 yards diſtant from my houſe, and proceeding to the north. It was very denſe, and roſe to the height of about ſixty feet by eſtimate, which is much higher than the houſes in the ſtreet. At about a quarter paſt five, the darkneſs being then greateſt, the houſes on the oppoſite ſide of the ſtreet appeared as if ſeen through a deep blue glaſs, particularly the white ſtone work above the windows; and upon looking upwards, the clouds were ſeen of a deep leaden blue colour, and moving ſwiftly in a direction preciſely oppoſite to that before obſerved, namely, to the E. by N. or E. N. E. Soon afterwards, the lightning and thunder continuing during the whole of theſe changes, there fell a heavy ſhower, which beat againſt the weſtern face of the houſe, and the darkneſs gradually went off. At half paſt five the clouds were much higher, and moved with a moderate angular motion to the north, while the ſmoke of a chimney oppoſite my window was gently driven to the ſouth.

We have yet much to learn concerning the theory of thunder-ſtorms. It is well known by experiments with the electrical doubler *, that almoſt all bodies poſſeſs a certain degree of electrization, which is variable from a conſiderable number of circumſtances. It is alſo known from Franklin's experiments of the can and chain, as well as from numerous other facts, that the intenſity of the electric ſtate will be augmented by diminiſhing the ſurfaces of bodies. In this way, as well as from other cauſes, it is inferred that clouds become highly electrified during the progreſs of their condenſation; ſo that flaſhes of lightning paſs between them and the earth, and between each other. From other obſervations and deductions, it has been alſo rendered highly probable that the long range of clouds in a ſtorm of this nature does ſerve as a conductor, through which flaſhes of electric fire are conveyed from one part of the earth to another part in a different ſtate. The facts above deſcribed ſeem uncommon, and, if collated with other more uſual events, may afford ſome inſtruction concerning this claſs of phenomena. I do not highly eſteem the conjectures which preſent themſelves to me on this occaſion; but ſhall communicate them, becauſe they tend to point out future objects of reſearch.

The ſingularity of this thunder-ſtorm appears to have ariſen from the maſs of aqueous vapour having been much too ſmall to afford a favourable communication between the two oppoſite ſtates of electricity on the ſurface of the earth. If the maſs of clouds be ſuppoſed to have been at firſt near the eaſtern portion of the earth, and to have become electrified and repelled, they would, on the common principle of bodies in that ſtate, be repelled, and paſs ſwiftly to the weſtern part of the ſurface to depoſit their electricity as ſoon

---

* New Experiments on Electricity. By the Rev. A. Bennet, F. R. S. octavo. London.

as they came within the ſtriking diſtance. In this ſituation, it may be imagined that the ſupply or communication might continue to be made by the eaſtern part of the cloud acting in the manner of a point, while the weſtern part emitted flaſhes; a ſuppoſition which is rendered more probable by the conſideration that moſt thunder-clouds are ragged or pointed on one ſide, and round or ſwelled on the other; and alſo that the diſpoſition of any conductor to receive electricity without exploſion is much greater than to give ſilently, even when the terminations are alike. I conjecture, therefore, that the lightning came from the eaſt, and paſſed through the clouds to the weſt; that the poſterior extremities of the clouds were illuminated as points uſually are; that the electric motion of theſe low clouds at firſt cauſed an eaſterly wind; that ſome change in the general ſtate of electricity, or perhaps the mere exhauſted ſtate of the clouds, cauſed them to paſs rapidly back to the original reſervoir, as in the moſt common experiments of electricity; that this return produced firſt a ſtrong eddy in the lower air, which threw up the duſt, and afterwards a contrary ſtream of wind, by which the rain was beaten againſt the weſt front of the houſe. But why the clouds ſhould have been illuminated with a red colour during their weſtern courſe, and afterwards with blue, does not ſeem deducible from any facts I know of. It may perhaps be analogous to the colours of the aurora-borealis. The contrary lower current, when the clouds were moving to the north, ſeems to have been a natural conſequence of their remoteneſs. It is probable that the returning current of the air, which muſt have been driven before the maſs of clouds when they were moving very near the earth, took place entirely in the upper part of the air, where, in that caſe, there was more room.

VII.

The Method of making excellent Bread without Yeaſt; as practiſed at Debretzin in Hungary[*].

By ROBERT TOWNSON, *LL. D. F. R. S. Edin.*

LIGHTER, whiter, and better-flavoured bread than that made here I never ate, nor did I ever ſee elſewhere ſuch large loaves. Were I not afraid of being accuſed of taking advantage of the privilege of travellers, I ſhould ſay they were near half a yard cubed. As this bread is made without yeaſt, about which ſuch a hue and cry is often raiſed, and with a ſubſtitute which is a dry maſs, that may be eaſily tranſported, and kept half a year or more, I think it may be of uſe to my country for me to detail the Debretzin art of making bread. The ferment is thus made: Two good handfuls of hops are boiled in four quarts of water: this is poured upon as much wheaten bran as can be well moiſtened by it: to this are added four or five pounds of leaven; when this is only warm, the maſs is well worked together to mix the different parts. This maſs is then put in a warm place for twenty-four hours, and after that it is divided into ſmall pieces, about the ſize of a hen's egg, or a ſmall orange, which are dried by being placed upon a board, and expoſed to a dry air, but not to the ſun; when dry, they are laid by for uſe, and may be kept half a year. This is the ferment, and it may be uſed in the following manner: For a baking of ſix large loaves, ſix good handfuls[†] of theſe balls are taken and diſſolved in ſeven or eight quarts of warm water. This is poured through a ſieve into one end of the bread-trough, and three quarts more of warm water are poured through the ſieve after it, and what remains in the ſieve is well preſſed out. This liquor is mixed up with ſo much flour as to form a maſs of the ſize of a large loaf: this is ſtrewed over with flour, the ſieve, with its contents, is put upon it, and then the whole is covered up warm, and left till it has riſen enough, and its ſurface has begun to crack: this forms the leaven. Then fifteen quarts of warm water, in which ſix handfuls of ſalt have been diſſolved, are poured through the ſieve upon it, and the neceſſary quantity of flour is added, and mixed and kneaded with the leaven: this is covered up warm, and left for about an hour. It is then formed into loaves, which are kept in a warm room half an hour; and after that they are put in the oven, where they remain two or three hours, according to the ſize. The great advantage of this ferment is, that it may be made in great quantities at a time, and kept for uſe. Might it not on this account be uſeful on board of ſhips, and likewiſe for armies when in the field?

---

[*] Travels in Hungary, 4to, London, 1797, page 242.

[†] I ſuppoſe broken into fragments; the balls themſelves being too large to be meaſured by handsful. N.

VIII.

Deſcription and Uſe of an Eudiometer with Sulphuret of Pot-Aſh. By Citizen GUYTON [*].

NATURAL philoſophers and chemiſts have long been deſirous of poſſeſſing an eudiometer which might accurately ſhew the quantity of oxygene mixed in any kind of gas. Citizen Berthollet has clearly ſhewn, in his late leſſons to the Normal School [†], that the eudiometer of Scheele, which he with juſtice conſiders as the beſt, has nevertheleſs great defects; becauſe the abſorption requires ſeveral hours, and becauſe there is a decompoſition of water towards the end, which conſequently diſengages hydrogene gas, and renders the meaſure of abſorption doubtful.

This conſideration induced me to ſeek a material which might immediately, and with convenience, afford a reſult more to be depended on than thoſe obtained by nitrous gas, hydrogene gas, phoſphorus, and the mixture of ſulphur and iron; the only ſubſtances which have been, as far as I know, hitherto uſed or propoſed for that purpoſe.

The ſulphuret of pot-aſh appeared to me to deſerve trial in this reſpect. I well knew that at the ordinary temperature it is only ſuſceptible of a combination ſtill more ſlow and inſenſible than the mixture of ſulphur and iron moiſtened; but I preſumed that by raiſing the temperature, merely by the approach of a ſmall taper, the action of chemical affinity might be ſo much favoured as to determine rapidly an abſorption which in that caſe would not be affected by any foreign circumſtance.

The effect has completely juſtified my conjecture; ſo that nothing more remains than to deſcribe the apparatus required to form this eudiometric inſtrument. I thought that the reverſed retort, or inflected receiver (recipient cornu) as I have named it in the article *Air* of the Encyclopédie Methodique, I. 706, would unite ſimplicity, convenience, and every deſirable advantage. The experiment was made in the laboratory of the third diviſion of the Polytechnic School, according to the following deſcription:

---

[*] Journal de l'Ecole Polytechnique, II. 166.

[†] Séance des Ecoles Normales, &c. tom. v. p. 73.

A B, Plate XII. Fig. 2. repreſents a very ſmall glaſs retort, with a long neck, its whole capacity being from 12 to 15 centilitres (between ſeven and nine ſolid inches Engliſh). It muſt be choſen of ſuch a curvature, that, when the neck is ſet upright, the bulb may form at its lower part a cavity to retain the matters introduced.

The extremity of the neck of this retort is ground with emery to enter the glaſs tube CD, which is open at both ends, and about 20 or 25 centimetres in length (eight or nine inches Engliſh). The retort then cloſes the tube in the manner of a ground ſtopper, and intercepts all external communication *.

A cylindrical glaſs veſſel, F, is provided, of the form of a common jar, in which the glaſs tube CD may be entirely plunged beneath the level of the water.

Laſtly, the ſulphuret of pot-aſh is prepared and broken into pieces ſufficiently ſmall to be introduced into the retort. Theſe are to be encloſed, dry and even hot, in a bottle for uſe.

Theſe conſtitute the whole apparatus and preparation of materials.

When it is required to examine an aëriform fluid, by ſeparating its reſpirable part, two or three pieces of the ſulphuret, of the ſize of a pea, are put into the retort. It is then filled with water, taking care to incline it ſo that all the air may paſs out from the bulb. The orifice of the retort is then to be cloſed, and inverted into the pneumatic tub, in order that the gas propoſed for examination may be transferred into it in the uſual manner.

By an eaſy manœuvre of alternately inclining the retort in different directions, all the water is made to flow out of the bulb in which the ſulphuret remains.

When this is done, the retort is placed in the vertical ſituation, and its extremity introduced into the tube of glaſs CD, which muſt always be under water. A ſmall lighted taper is then to be placed under the bulb.

To ſupport the retort in its poſition, the jar is provided with a wooden cover, in which there is a notch to receive it.

The firſt impreſſion of the heat dilates the gaſeous fluid ſo much that it deſcends almoſt to the bottom of the tube, which is diſpoſed expreſsly for its reception; otherwiſe the partial eſcape would prevent an accurate determination of its change of bulk.

But as ſoon as the ſulphuret begins to boil, the water quickly riſes, not only in the inferior tube, but likewiſe in the neck of the retort, notwithſtanding the application and even the increaſe of the heat.

If the fluid be abſolutely pure vital air, the abſorption is total. In this caſe, to prevent the rupture of the veſſel by too ſudden refrigeration, the aſcent of the water muſt be rendered ſlower, either by removing the taper, or inclining the retort †; which will not pre-

---

* Citizen Chauſſier had before conſtructed, for eudiometric experiments with phoſphorus, an apparatus little different from this, compoſed of a long tube of a ſingle piece, one end of which is bended and blown into a bulb, and has at e a projecting tube, which is cloſed with a cork, after having cauſed the water to riſe within the tube to about one-third of its height. This inſtrument may alſo be applied for experiments with the ſulphuret of pot-aſh. I muſt obſerve, however, that the practice is not ſo eaſy as it appears at firſt ſight: beſides which, if the projecting tube renders it very convenient for operating on the air of the atmoſphere, it is not the ſame with regard to the other gaſes, which cannot be introduced but by transferring them. G.

† It does not appear that inclining the retort would diminiſh the rapidity of aſcenſion. If the perpendicular height were increaſed, by partly raiſing the tube above the water, or if the aperture were partly cloſed, this effect would follow. N.

vent the abſorption from continuing while any gas remains which is proper to ſupport combuſtion.

If the fluid be common air, or vital air mixed with any other gas, the quantity of water which has entered the retort muſt be accurately meaſured after the cooling. It repreſents the volume of air abſorbed. Care muſt be taken to encloſe the remaining gas under the ſame preſſure, by plunging the retort to the level of the line at which the encloſed water reſts, before the orifice is ſtopped.

This operation of meaſuring, which is very eaſy when meaſuring veſſels are at hand, may be habitually performed by a ſlip of paper paſted on the neck of the retort, upon which diviſions are drawn from obſervation, and which muſt be covered with varniſh, to defend it from the action of the water.

IX.

Deſcription of an improved Electrometer, in which the Senſibility of the Gold-Leaf is conſiderably augmented, and the Intenſities are diſtinguiſhed by numerical Graduation.

It is ſcarcely to be ſuppoſed that any philoſopher who is converſant with electricity can be unacquainted with the electrometer of Bennet, in which two pendent ſlips of gold-leaf are ſubſtituted in the place of the pith-balls of Canton, and ſerve to indicate the nature and quality of very minute intenſities of the electric ſtate. There are two particulars in which this excellent inſtrument appears capable of improvement: the firſt, to render it portable, without danger to the gold-leaf, and the ſecond, to expreſs its various degrees of electrization by a ſcale of diviſions.

I have reflected much on the probable means of ſecuring the gold-leaf from fracture by carriage, but hitherto with little proſpect of ſucceſs. There was ſome hope that a ſingle ſlip of this gold might be preſerved in a ſheath or box, with its ſides very nearly in contact; but when I placed ſuch a ſlip upon a gilded piece of wood of the ſame ſuperficial dimenſions, to which it was faſtened at one end, its flexibility was ſuch that the leaf very readily ſlided along the ſurface of the wood, and became full of folds, by inclining the faſtened end a very few degrees lower than the other extremity. There was ſtill leſs immediate expectation that the ſlips could be actually and repeatedly confined between two leaves or cuſhions, as in the book of the gold-beaters, without their being broken by occaſional agitation. To this, however, my attention will probably be directed when I may again reſume this object. In the mean time, I recommend it to other philoſophers, as a very deſirable improvement in the mineralogical apparatus, and ſhould rejoice to be anticipated by their ſucceſsful reſearches.

The weight of one ſlip of gold-leaf, in the electrometer of Bennet, is about one-ſix-hundredth part of a grain; but this, as well as the ſenſibility of the inſtrument, muſt vary, not only from the figure and dimenſions of the piece, but the nature and thickneſs of the gold itſelf[*]. It ſeemed, therefore, unneceſſary to endeavour to render two of theſe inſtruments comparable with each other. All that could be done was, to diſtinguiſh the different intenſities as ſhewn by the divergencies of the leaf; or, as I have taken it, the diſtances at which they ſtrike a pair of uninſulated metallic bars. In Plate XII. Fig. 3, A repreſents the inſulated metallic cap, from which, at C, depend the two narrow pointed ſlips of gold-leaf. BB is the glaſs ſhade, which ſerves to ſupport the cap, and defend the leaves from the motion of the ſurrounding air. DD are two flat radii of braſs, which open and ſhut by means of one common axis, like a pair of compaſſes. By a contrivance of ſprings, they are diſpoſed to open when left at liberty; but the micrometer ſcrew E ſerves to draw a nut, which has two ſteel bars, with a claw at the end of each, that enters into a correſpondent ſlit, in two ſmall cylindrical pieces, to which the radii are fixed reſpectively. This apparatus is ſeen in another poſition in Fig. 4. KL repreſents a piece of braſs, which ſerves as the frame for the work, and fits the lower ſocket of the electrometer, FF, Fig. 3. In this the letters I H indicate the cylindrical pieces which carry the radii, and are ſeen from beneath. On the ſide of the nut G, one of the ſteel drawing pieces is ſeen; the other being on the oppoſite ſide, and conſequently not viſible. Towards L appear the two re-action ſprings. The other parts require no verbal deſcription.

---

[*] Philoſophical Journal, I. 133.

In the common conſtruction of the gold-leaf electrometer, there are two pieces of tinfoil paſted on oppoſite parts of the internal ſurface of BB; againſt which the gold-leaf ſtrikes when its electricity is at the maximum. If the radii DD be left at the greateſt opening, our inſtrument does not then differ from that in common uſe. But if the divergence produced by the contact of an atmoſpheric conductor, or any other ſource of electricity, be ſo ſmall as to render it doubtful whether the leaves be electrified or not, the radii may then be brought very gradually together by means of the ſcrew, until the increaſed divergency from their attractive force be ſufficient to aſcertain the kind of electricity poſſeſſed by the leaves. In this and all other caſes, the diviſion on the micrometer head, which ſtands oppoſite the fixed index, at the time the leaves ſtrike the radii, will ſhew the greater or leſs degree of intenſity.

X.

The improved Proceſs of Tanning. By Citizen SEGUIN [*].

IN conſequence of our knowledge of the multiplied reſearches and important diſcoveries of Citizen Seguin reſpecting aſtringent ſubſtances, and the happy application of thoſe diſcoveries to ſimplify and perfect the art of tanning, we invited him to viſit the public laboratory, for the purpoſe of communicating his proceſſes and obſervations. Far different from thoſe ſelfiſh manufacturers, who carefully conceal under the cloak of pretended myſtery, operations in themſelves ſimple, frequently tranſmitted by oral tradition, and which have coſt them neither trouble nor expenee, the Citizen Seguin did not heſitate to comply with our wiſhes, and has devoted to this object two ſittings, at which all the pupils, and the greateſt part of the inſtitutors and agents of the ſchool, were preſent. He not only exhibited without reſerve all that experience and meditation had diſcovered to him, but he likewiſe came attended by ſkilful workmen, with proper inſtruments to execute all the proceſſes. Laſtly, he furniſhed gratuitouſly the ſkins, and a conſiderable quantity of tan, in order that each of the pupils might himſelf repeat the experiments, follow the detail of all the proceſſes, and render himſelf completely maſter of the method. We could have wiſhed to render an exact account of the two lectures delivered by Citizen Seguin at the ſchool; but circumſtances oblige us to confine our narration to the principal objects.

---

[*] Reported by Citizen Chauſſier; being part of the Tranſactions of the Polytechnic School of France. From their Journal, IV. 678.

Want is the parent of the arts. Though man in the poſſeſſion of all his forces is formed to ſupport the difference of ſeaſons and climates; though the parts of his body which are ſubjected to preſſure, as in quadrupeds, are ſo diſpoſed as to acquire by habit and exerciſe a degree of denſity and compactneſs, which renders them little ſenſible to the action of foreign bodies: yet accidents, and circumſtances which it is eaſy to ſuppoſe, have determined him to ſeek the means of ſecuring his feet from the impreſſion of an unequal, flinty, or humid ſoil. The ſhepherd muſt have firſt made uſe of ſoft and flexible bark, mats, and ſimilar fabrications of different kinds; the hunter muſt have taken a piece of the freſh ſkin of ſuch animals as ſerved for his ſupport, which he muſt have faſhioned, modelled upon his foot, and retained with ſtraps *. Theſe ſimple means are ſufficient in a climate which is uſually dry; but they would be of little advantage on wet ground, or in a climate ſubject to the alterations of wet and dry weather.

Skins ſwell up, and become ſoft, by moiſture, which renders them permeable to water. Hence they are eaſily deſtroyed by the putrid proceſs which enſues, and they become dry and brittle when the moiſture is evaporated. Accident, no doubt, occaſioned the diſcovery of the means of preventing theſe inconveniences by the uſe of certain vegetable ſubſtances, particularly the bark of oak. It was ſeen that ſkins prepared with theſe ſubſtances acquired new properties; that without loſing their flexibility they became leſs permeable to water; more firm, more compact, and in ſome meaſure incapable of putrefaction. Theſe obſervations gave birth to the art of the tanner.

This art, no doubt of high antiquity, becauſe founded on one of the earlieſt wants of man in ſociety, comprehends a ſucceſſion of proceſſes which was executed by habit and imitation, without a knowledge of the eſſential objects. The preparation of ſkins accordingly required ſeveral years, and frequently, in ſpite of the care, expence, and ſlowneſs of the operation, the tanning was incomplete; the ſkin formed a ſoft and porous leather, which was ſoon deſtroyed by moiſture. Theſe defects eſſentially ſprung from ignorance of the true principles of this operation, becauſe no diſcovery had been made reſpecting the action of tan upon the ſkin, and the circumſtances or conditions which might accelerate or retard the proceſs.

To arrive at this knowledge in an accurate manner, it is neceſſary to conſider, firſt, the nature and properties of tan, and ſecondly, the ſtructure and compoſition of the ſkin.

We ſhall not enter into the detail of ſuch precautions as are requiſite in the choice of oak bark, the time and manner of ſeparating it from the tree, preſerving it, or pulveriſing it. It will be ſufficient for our object to remark, that water poured into a veſſel upon tan acquires, after ſome hours infuſion, at the common temperature of the atmoſphere, a brown

---

* According to the relations of travellers, theſe uſages are ſtill to be found among certain nations. Sparman affirms, that the Hottentots make their ſhoes with a piece of freſh ſkin, the edges of which they raiſe up, and tie with ſtraps; the hairy ſide is outwards. No other preparation is made than to beat and moiſten the ſkin. I it be ſtrong and thick (ſuch is the ſkin of the buffalo, for example), it is left for ſome hours in cow-dun which renders it ſoft and flexible.

colour, an aſtringent taſte, and becomes charged with the moſt ſoluble ſubſtances contained in the tan; that by drawing off the water, and adding a ſimilar quantity to the tan repeatedly, the whole of the ſoluble parts may be ſucceſſively extracted, the water ceaſes to acquire colour, and there remains in the tub a mere fibrous matter, or parenchymatous texture, inſoluble in water, and no longer adapted to promote the operation of tanning. This reſidue is therefore always rejected in the manufactories as uſeleſs. It is only uſed by gardeners for their hot-beds, but might probably be advantageouſly applied in the fabrication of coarſe paper.

It is therefore in the water of infuſion, or the lixiviations of tan, that we muſt ſeek for the ſoluble ſubſtances which alone are efficacious in tanning.

On examination of the water of the laſt filtration, it is found to be not only clearer, leſs impregnated, and leſs acrid than the water of the firſt lixiviation, but likewiſe that it poſſeſſes all the properties of the gallic acid. It reddens the infuſion of tournſol, acts upon metallic ſolutions, and more particularly it precipitates a black fecula from ſulphate of iron, &c. And it is alſo found that a piece of freſh ſkin, diveſted of its fat and ſanguine humours, and macerated in this liquor, inſtead of becoming compact, is ſoftened and ſwells up.

The liquor of the firſt lixiviation exhibits a very different character. It is more coloured and aſtringent; it not only exhibits the properties of the gallic acid, by the alterations it cauſes in the blue colours of vegetables, and the black precipitate it forms with the ſulphate of iron; but it likewiſe poſſeſſes the remarkable quality of forming, with animal gelatin, or glue, a yellowiſh abundant precipitate, inſoluble in water, not putreſcible, which becomes hard and brittle by drying; and if a piece of ſkin properly prepared be immerſed in this fluid, it becomes gradually more compact, and is converted into leather.

There exiſt, therefore, in the ſame fluid, two very different ſubſtances: the one, which precipitates a black matter from iron, is the gallic acid or principle; the other, which precipitates animal gelatin or glue, is called the tanning principle, on account of its efficacy in the preparation of leather.

To leave no doubt on this important point, it was proved, by a number of experiments eaſy to be repeated,

1. That the liquor of the laſt lixiviation, though coloured, and of an aſtringent taſte, affords no precipitate with glue; a fact, which ſeems to ſhew that the gallic acid contained in the bark is leſs ſoluble than the tanning principle. In fact, as has already been remarked, when water is ſucceſſively poured on the tan, an infuſion is at laſt obtained which no longer precipitates glue, though it precipitates ſulphate of iron very well.

2. The liquor of the firſt lixiviation, after having been ſaturated with glue or animal gelatin, and forming an abundant precipitate with that ſubſtance, is entirely deprived of the tanning principle. It no longer differs from the liquor of the laſt filtrations, and contains merely a portion of the gallic acid. Hence the addition of ſulphate of iron affords a new precipitate with this liquor.

3. As the tanning principle has a ſtrong attraction to the animal gelatin, with which it always forms an inſoluble precipitate, this property affords a very convenient re-agent to aſcertain its preſence immediately in any fluid, and to determine with preciſion its quantity. Accordingly, the infuſion of tan poured into milk, whey, ſerum, broth, &c. forms, with

theſe liquors, a precipitate more or leſs abundant, according to the quantity of gelatin they contain.

This peculiar property of the tanning principle affords an application which may become of great importance in the art of treating diſeaſes, to determine the nature of urine, and to aſcertain ſome of its changes. In the healthy ſubject, all whoſe functions are duly exerciſed, the urine does not contain gelatin, nor afford a precipitate with the infuſion of tan: on the contrary, in all the gaſtric affections, the urine is more or leſs charged with gelatin, and forms, with the infuſion of tan, a precipitate more or leſs abundant. The ſame obſervation is applicable to acute and chronical diſeaſes, in which the aſſimilating or digeſtive forces are troubled, deranged, or perverted.

4. The gallic acid, or, if other terms be preferred, the principle which precipitates the ſulphate of iron, is often found alone, or at leaſt without being accompanied by the tanning principle. Thus quinquina, crude or torrefied coffee, the roots of the ſtrawberry-plant, ſcrofularia, milfoil, arnica, the flowers of Roman camomile, and all the multitude of plants vaguely compriſed under the title of aſtringents, contain the gallic acid only. All theſe form with the ſulphate of iron a precipitate more or leſs coloured and abundant; but none of them produce the ſlighteſt change in the ſolution of animal glue. On the contrary, the tanning principle has never been found alone, but always united or combined with the gallic principle. It was long ſuppoſed to exiſt excluſively in the oak, the nut-gall, and ſumac, the only ſubſtances uſed at the tan-works; but it is found more or leſs abundantly in the ſiliquaſtrum, the roſe-tree, the larix, ſeveral ſpecies of pines, the acacias, the lotus, the ſquill, the roots of biſtort, of rhubarb, of parella, and ſeveral other plants, of which we ſhall hereafter give a liſt. We have alſo found this principle in the products of diſtillation of different vegetable ſubſtances, where it was in ſome meaſure formed during the operation.

From theſe different conſiderations, founded on experiment, the following general principles may be deduced: 1. Every ſubſtance of which the infuſion is capable of precipitating animal jelly, poſſeſſes the tanning property. 2. Every ſubſtance which poſſeſſes the tanning property, likewiſe precipitates the ſulphate of iron black. 3. Every ſubſtance which precipitates the ſulphate of iron, but not the ſolution of glue, does not poſſeſs the tanning property [*].

We ſhall diſmiſs the conſideration of the tanning principle, by ſhewing ſome of the remarkable changes to which its infuſion is ſubject.

1. A few days after its preparation a yellowiſh precipitate is ſpontaneouſly formed, which is more abundant in proportion as the liquor is more ſaturated, and the time of infuſion longer. This precipitate, when ſeparated, is converted by drying into a very fine, light, aſh-coloured powder, totally inſoluble in water, oil, alcohol, and ether, even at the temperature of ebullition. When thrown on burning coals, it readily takes fire, and emits a thick ſmoke. By deſtructive diſtillation it affords a conſiderable quantity of carbonic acid gas, an aqueous acid, and a browniſh oil, leaving in the retort a light ſpongy coal

---

[*] The ſolution of glue is a convenient re-agent to aſcertain the preſence of the tanning principle. It is therefore advantageous to have it in readineſs in the laboratory. To prevent the ſpeedy putrefaction to which animal ſubſtances are liable, we have added one-twentieth part of alcohol, which anſwers the purpoſe very well, without altering the properties of the fluid. C.

As this precipitate by its inſolubility approaches to the nature of wood, and as it falls down even when the lixivium of tan is preſerved in well cloſed veſſels entirely filled, it might be ſuppoſed that it is merely a gradual and ſucceſſive depoſition of the remains of the woody and parenchymatous parts of the bark diſſeminated and ſuſpended in the fluid; it might be imagined that the contact of air has nothing to do with this phenomenon. But we muſt obſerve, that this precipitate is found in the moſt limpid ſolution, even after it has been filtered; that it is moſt abundant when the infuſion has been prepared from bark ground long before, and preſerved without care; and that it is more readily depoſited when the liquor preſents a great ſurface to the contact of the air. In this caſe the ſurface of the fluid becomes tarniſhed by an extremely fine pellicle, which covers it, and becomes thicker and firmer the longer the fluid remains thus expoſed and at reſt. If it be divided by agitating the liquor, it is again renewed, and in this manner produces the precipitate we ſpeak of. We may determine, in ſome reſpect, at pleaſure, the formation of this pellicle and precipitate, by expoſing the liquor to the contact of oxygene gas, or by pouring into it the oxygenated muriatic acid. It is ſufficient even to bring a bottle containing the oxygenated muriatic acid into the vicinity of the infuſion of tan. A pellicle is inſtantly formed, reſembling a ſlight gauze, which floats and ſpreads inſtantly over the whole ſurface, aſſuming in a regular order the moſt beautiful colours of the iris, and at laſt becomes browniſh. If the experiment be continued for a few minutes, the pellicle thickens, and the precipitate begins to fall. The formation of this precipitate, and the properties it acquires, depend, therefore, on the combination of oxygene, which operates either during the infuſion and filtration of the lixivium, or after its having been drawn off clear with the contact of atmoſpheric oxygene. Hence are ſhewn the neceſſity and advantage of uſing tan recently prepared *, and of preſerving the tan as well as the infuſions defended from the contact of the air.

Theſe phenomena are common to all the preparations of vegetables made by means of water, whether by infuſion, decoction, or even diſtillation. We have ſeen theſe precipitates gradually formed in the infuſion or decoction of nut-gall, ſumac, gentian, quinquina, and even of the moſt inſipid as well as of the moſt aromatic and acrid plants. The expreſſed juice of freſh plants, ſuch as hemlock, ſorrel, anil, &c. preſents the ſame phenomena, and affords, more particularly when the action of air is promoted by agitation, a coloured pulverulent depoſition which has long been diſtinguiſhed by the name of fecula. Laſtly, all the diſtilled waters which are kept for ſeveral years become turbid, by the formation of filaments and whitiſh flocks, more or leſs abundant, but always inſoluble. It is to a combination of oxygene, as Fourcroy in his excellent analyſis of quinquina has ſhewn, that we ought to attribute the formation of theſe precipitates, the inſolubility they acquire, and their approach to the nature of the ligneous fibre.

Nevertheleſs, the ſpontaneous precipitation which is effected by the contact of atmoſpheric oxygene in the lixivium of tan does not change its properties, at leaſt in any ſenſible manner; and as in a lixivium conſiderably ſaturated the precipitate is moſt abundant on the firſt days, it might perhaps be of advantage to wait until this firſt precipitate was formed before

---

* Theſe obſervations are applicable to all vegetable ſubſtances which are reduced to powder. They not only loſe their peculiar aroma, but likewiſe change their nature by the contact of light and atmoſpheric air. Their properties being thus conſiderably altered, the doſe at which it is proper to employ them becomes uncertain. The phyſician ought not therefore to preſcribe in his formulæ any powders but ſuch as are recently prepared. C.

immerſing

immerſing the ſkins. For this ſubſtance, when depoſited, attaches itſelf to the ſurface of the ſkins, where it forms a coating more or leſs thick, which ſoils them, cloſes their pores, and retards in a ſmall degree the direct action of the tan.

2. The lixivium of tan, particularly when highly concentrated, and prepared in hot weather, acquires after a few days a vinous ſmell, which ſeems to announce a commencement of fermentation, and might lead to an apprehenſion that its progreſs might impair the tanning properties. In order to aſcertain how far theſe notions might be well founded, different vegetable acids were mixed with the tanning lixivium, but none of them ſenſibly altered its properties. And ſtill more we may add, that for near two years a bottle of the infuſion of tan has been kept in the laboratory, and, in ſpite of the alterations of temperature to which it has been expoſed, and the change neceſſarily produced by the contact of air, its properties appear the ſame, and it is ſtill daily uſed with advantage as a re-agent to aſcertain the preſence of gelatin.

As a knowledge of the properties of tan affords obſervations of importance, which conſtitute the baſis of Seguin's method, we have not been apprehenſive of dwelling too long on the ſubject; but we ſhall confine our remarks on the ſtructure and chemical compoſition of the ſkin to a few general conſiderations.

This membrane, as is ſhewn by anatomiſts, is eſſentially formed of a great number of laminæ or fibres, which are white, broad, ſhort, and cloſely adherent, but interwoven, and diſpoſed in different directions, ſo as to leave between them an infinity of ſmall ſpaces or pores. From this conſtruction it exhibits a denſe but ſoft ſpongy texture, and is ſuſceptible of extenſion and contraction. In the midſt of this ſubſtance are found a great number of nerves and veſſels of different kinds, the very fine and multiplied ramifications of which ſerve to ſupport and maintain the lamellated ſtructure of the ſkin, and convey into the vacuities between the fibres thoſe fluids which ſerve for the ſupport and nutrition of this membrane. The external part is the epidermis. This thin tranſparent membrane is of a very different texture. It contains neither the apparatus of veſſels, nor the diſpoſition of fibres or laminæ, which by their interſection form a kind of ſpongy net-work. It is in ſome meaſure a ſimple uniform covering, which preſents no diſtinct organization, but adheres ſtrongly to the ſkin, entering into its folds and numerous poroſities. Laſtly, the hairs are implanted in the ſkin by a ſort of bulb or oval root, interſperſed with ſmall veſſels filled with a kind of mucus.

The compoſition of the ſkin, the changes it undergoes by different preparations, by chemical agents, and the properties it acquires in theſe ſeveral ſtates, preſent other conſiderations.

If the freſh ſkin of an adult animal be macerated for ſome hours in water at the temperature of the atmoſphere, and if, to accelerate the effect, agitation and preſſure be uſed, a ſeparation is made of the blood, the juices, and the different ſoluble ſubſtances contained in the veſſels or vacuities; and by examining the nature of the matter thus extracted, it is found that a ſmall portion only conſiſts of gelatin, which has little conſiſtence or tenacity. Subſequent macerations, at the ſame temperature, afford no more gelatin, the fibrous texture remains inſoluble, and undergoes no further loſs. It therefore appears, that in the natural ſtate the ſkin contains but a ſmall portion of gelatin perfectly formed, and ſoluble. It may even be apprehended, that the ſmall quantity obtained by the firſt maceration was merely included in the pores of the ſkin adherent to the ſurface of the fibres; and that the vital force had not yet time to aſſimilate and convert it to the ſtate of fibre.

But if the temperature be raiſed to ebullition the fibrous texture is altered, becomes ſoft, and ſucceſſively diſſolves. It aſſumes the character of gelatin, and may be entirely converted into a viſcid and tenacious glue. The fibre which forms the ſolid ſtructure of the ſkin does not therefore eſſentially differ from gelatin, but in its texture, its concretion, and its inſolubility in cold water; and as it is obſerved that ſubſtances capable of abſorbing oxygene deprive the fibrous matter of its ſolidity, and haſten and determine its converſion into gelatin, we are authoriſed to conclude that theſe diſtinct qualities of the fibre depend only upon a proportion of oxygene which the vital action and progreſs of life combine with the gelatin. Multiplied experiments appear to leave no doubt on this point. "They have proved to me (ſays Seguin) that the fibre is oxygenated glue, which in that ſtate cannot combine with the tanning principle, but which acquires that property by paſſing to the ſtate of gelatin in conſequence of the loſs of a portion of oxygene."

The eſſential point in the operation of tanning is therefore to aſcertain, and to direct in a preciſe and invariable manner, the circumſtances and conditions which determine the tranſition of fibre to the ſtate of gelatin, and to ſeize the inſtant in the proceſs to effect a proper combination with the tanning principle. This object is too important not to recall ſome of the obſervations made by Seguin.

Glue, as has before been remarked, poſſeſſes a great attraction for the tanning principle. It immediately forms with this ſubſtance an inſoluble matter, which is not ſubject to putrefaction; but it muſt be well remarked that the precipitate is dry and brittle.

Hence it is evident that a ſkin, the pores of which might contain gelatin ready formed, or of which the fibrous texture ſhould have been altered and converted by certain preparations to the gelatinous ſtate, would tan very ſpeedily, but would afford a kind of leather which would be harſh, brittle, and diſpoſed to crack or peel in the wear. On the contrary, the operation will be long, incomplete, and will afford only a ſoft, ſpongy, and putreſcible leather, if the fibre preſerves its ſtate of oxygenation, if it do not paſs by ſucceſſive degrees into the ſtate of gelatin, or if the tanning principle do not penetrate its thickneſs in proportion to the converſion of the fibre into glue.

Theſe inconveniences are avoided, and the propoſed object is obtained with preciſion, by the uſe of the lixivium of tan. This fluid has been ſhewn to contain two very different principles, of which the union and action are alike neceſſary for the ſucceſs of the operation. On the one ſide the gallic principle, which, as is known, readily ſeizes the oxygene of metallic ſolutions, and reduces them or brings them nearer to the metallic ſtate, acts nearly in the ſame manner upon the fibre; it unburns it, or deprives it of oxygene, and converts it into the ſtate of gelatin. On the other hand, the tanning principle, which alſo exiſts in the ſolution, exerciſes its action as ſoon as the fibre is ſufficiently reduced to the ſtate of gelatin. The tanning proceſs is not, therefore, inſtantly effected, like the precipitation of animal glue; but it operates gradually, and in ſucceſſion, by ſtrata from the ſurfaces of the ſkin to the centre; and as the action of the tanning principle immediately follows that of the gallic acid, the fibre is ſurpriſed in its poſition, the felted texture of the ſkin is totally preſerved, and its compoſition alone is changed. Accordingly, the leather prepared by this proceſs has the advantage of being ſupple, flexible, inſoluble, imputreſcible, more durable, and leſs diſpoſed than any other to imbibe humidity.

The new method of Seguin for the preparation of leather is founded upon this ſeries of obſervations;

obſervations and experiments. We ſhall not in this place enter into the detail of the changes and improvements he has made in the different branches of the tanning art; but we muſt add, that the proceſſes have been repeated in all the laboratories of the Polytechnic School, by the greateſt number of the pupils; that the operations on different kinds of ſkins were all concluded in eight, ten, twelve, or fourteen days at moſt; that the leather thus manufactured was completely ſaturated with tan, and of a quality ſuperior to the leather prepared by the old method which is to be met with in the market; and laſtly, that to theſe advantages the method of Seguin unites ſimplicity, facility, and certainty of ſucceſs. A diſcovery of this importance, in an art ſo neceſſary to our wants, entitles the author to the eſteem and gratitude of the public; and theſe ſentiments are more eſpecially due to the citizen who ſacrifices his individual intereſt to the general proſperity, and is deſirous of communicating the fruits of his reſearches. The advantage of public utility conſtitutes his recompenſe, and enables him to diſregard the clamours which envy, ignorance, and the prejudice of old habits never fail to raiſe againſt every uſeful innovation.

After the two ſittings employed by Citizen Seguin to explain the nature and properties of tan, the order adopted in the courſe of vegetable chemiſtry was reſumed in the ſchool. The alkalis, the ligneous part, and the colouring matter were ſucceſſively examined; the laſt of which naturally led to an explanation of the principles of the art of dyeing, the different proceſſes for fixing colours, and aſcertaining their qualities. In treating of theſe different objects, ſome conſiderations were preſented reſpecting the new properties which ſtuffs acquire by tinctorial proceſſes.

The proceſs of dyeing ought not to afford ſimply a colour to pleaſe the eye, but ſhould effect a kind of colouring tannage capable of adding to the properties of the web, and to render it leſs ſoluble or putreſcent. This object is generally neglected or little known, but it cannot be indifferent. It is not only of importance with regard to economy, but we do not heſitate to ſay, equally ſo for the preſervation of health. This may eaſily be conceived if attention be paid to the circumſtances, that cloths of the ſame kind and fabric differ conſiderably according to the dye; that ſome are dry, brittle, of little durability, not adapted to preſerve or retain caloric; others are ſoft, ſpongy, capable of retaining damps and exhalations of every kind; and by the ſucceſſive action of light, air, and caloric, may either undergo a ſort of oxidation that deſtroys their texture, or elſe paſs to a kind of putrefaction, which forms a continual atmoſphere round the body, and more or leſs affects the health. It is in the midſt of armies under tents, in camps, where a great number of men are expoſed to the ſame kinds of fatigue and intemperance, and differ only in the colour of their clothing, that we may aſcertain the truth of theſe obſervations. It is in the magazines of the military hoſpitals, where the clothing of men is depoſited, that a very marked difference in the ſmell and poroſity of the ſtuffs is obſerved, according to the colour with which they are charged.

After a ſucceſſive examination of the different products of vegetables, the attention of the ſchool was directed to the alterations they undergo after death, by the action of light, caloric, air and water. Theſe ſubjects led to the theory of the vinous and acetous fermentations, and the production of alcohol and ether; and laſtly, the courſe was terminated by an examination of the phenomena of putrefaction, which reſtores to the atmoſphere and to the earth thoſe principles which the action of vitality had before extracted.

During the concluding months of this courſe, the pupils continued to operate in the particular laboratories appointed for their uſe, either in repeating the principal experiments which had been exhibited in the lectures, or in making others which had been pointed out or imagined by themſelves. Their attention was principally directed to the application of chemical knowledge to the progreſs of arts and manufactures. With this view they ſucceſſively prepared different kinds of ſoap, varniſhes, and pigments, and they executed in ſmall the different proceſſes of dyeing, tanning, &c. Some of the pupils attended particularly to the experimental reſearch of ſubſtances proper to form the oxalic acid; others engaged in the experimental enquiry after thoſe vegetables which contain the tanning principle, and might be uſed as a ſubſtitute for oak bark. From theſe reſearches a table has been formed, which is already very ample, wherein thoſe vegetables which afford a precipitate with the ſulphate of iron alone, are diſtinguiſhed from thoſe which afford a depoſition with the ſolution of glue. This table is already very intereſting; but it may eaſily be imagined that it ſtill preſents vacuities to be filled up by time and experiments. This will form the object of the work of another year; and when it ſhall be finiſhed, we ſhall haſten to preſent the labours of the pupils of the ſchool.

XI.

The Combuſtion of Phoſphorus in the Vacuum of the Air Pump. By DR. MARTINUS VAN MARUM.

[Concluded from page 237.]

BEING deſirous of aſcertaining the cauſe of this ſingular inflammation in air ſo rarefied, I put, on another day, a ſmall ſtick of phoſphorus, wrapped in the ſame manner in cotton powdered with reſin, under a receiver containing about four hundred cubic inches, on the plate of the air pump, in order to obſerve the phenomenon with accuracy a ſecond time. The temperature of the place where I made the experiment was nearly the ſame as on the former occaſion, namely 56 degrees of Fahrenheit's ſcale. To obſerve the degree of rarefaction more eaſily, I placed a ſhort barometer gauge under the receiver. When the air was rarefied ſo that the mercury was ſupported in the gauge to the height of about an inch, the light began to be enlarged on the ſurface of the phoſphorus, chiefly at the upper part of the ſmall cylinder. This light increaſed by degrees during the ſubſequent exhauſtion, and the inflammation took place when the mercury ſtood at the elevation of 5 lines.

The flame was much paler and weaker than is afforded by phoſphorus when burned in an atmoſphere of the uſual denſity. I obſerved the flame to become weaker and weaker, and about two minutes afterwards the phoſphorus ceaſed to exhibit any light.

IV. To aſcertain whether the cotton powdered with reſin, which was wrapped round the phoſphorus, might be the cauſe which gave place to the inflammation, I placed under the ſame receiver two ſmall ſticks of phoſphorus of the ſame ſize, one of which only was wrapped in the powdered cotton. Theſe two ſmall pieces began to ſhine at the ſame time, when the mercury ſtood at the height of about an inch in the ſhort barometer gauge. Neverthe leſs, the piece which was ſurrounded by the cotton and powdered reſin alone took fire when the rarefaction was more advanced.

I then thought there was ſome reaſon to ſuppoſe that the reſin might be the cauſe of the inflammation; and to determine this point, I put under the ſame receiver three ſimilar ſmall ſticks of phoſphorus, one of them ſimply powdered with reſin, another wrapped in cotton without reſin, and the third wrapped in cotton powdered with reſin, in the ſame manner as in the former experiments. The light began to increaſe at the ſame time in all three, namely, as ſoon as the mercury had fallen to about one inch in the barometer. The phoſphorus wrapped in cotton powdered with reſin took fire firſt; a ſhort time afterwards that which was wrapped ſimply in cotton took fire; and that alone which was ſimply powdered with reſin did not take fire at all.

V. After theſe reſults, the queſtion was, to know in what manner the cotton could cauſe inflammation in air ſo rarefied, in which every other combuſtible, though ſet on fire, would ceaſe to burn; and ſtill farther, how phoſphorus could ſpontaneouſly take fire when the experiment was made in an atmoſphere where the temperature does not exceed 56 or 58 degrees of Fahrenheit, though the phoſphorus does not take fire in the open air unleſs heated to about 112 degrees of Fahrenheit. After ſome reflections and experiments, I found that this ſingular phenomenon could be very eaſily explained according to the principles of the modern chemiſtry. The increaſe of the light which precedes the inflammation when the air is rarefied to a certain degree, ſuggeſted to me this explanation. I ſhall therefore, in the firſt place, explain the evident cauſe of the increaſe of light in the phoſphorus in the rarefied air.

VI. Exhalations continually riſe from the ſurface of phoſphorus when expoſed to the atmoſphere, as is proved by its ſpeedy diſſipation in that circumſtance: but as ſoon as the air is rarefied to a certain degree, the exhalations cannot riſe; for theſe particles will not be elevated but during the time that the ſurrounding air is heavier than themſelves. Whenever, therefore, the air is rarefied to this degree, the exhalations muſt remain, and ſurround the phoſphorus from which they came. The union of oxygene with theſe phoſphoric exhalations muſt be then made only in the vicinity of the phoſphorus, whence the light from the diſengaged caloric muſt be ſeen there only. It is evident that this light will be much ſtronger when the phoſphoric exhalations do not riſe, becauſe the diſengagement of the ſame quantity of caloric is then made in a more confined ſpace than when the phoſphoric exhalations could riſe and be diſperſed in the receiver.

VII. The caloric which is diſengaged from the oxygene, and is ſeen in the rarefied air round the phoſphorus in the form of a ſtronger light than ordinary, muſt, alſo, on account of its greater denſity, give heat to the phoſphorus. Hence may be clearly ſeen the reaſon of the combuſtion of phoſphorus on rarefying the air when it is ſurrounded with cotton, as before deſcribed. Woollen and cotton ſtuffs have the property of preventing the diſperſion of caloric [*]. The caloric which is diſengaged round the phoſphorus in the rarefied air is thus retained by the cotton, until its accumulation on the ſurface of the phoſphorus becomes at length ſufficient to ſet it on fire. When a piece of phoſphorus is not enveloped in cotton or ſome ſimilar ſubſtance, it does not take fire in the rarefied air, becauſe the caloric which is diſengaged near the phoſphorus is ſo ſpeedily diſperſed when it is not arreſted by the cotton, that the phoſphorus cannot acquire the degree of inflammation neceſſary to inflame it.

---

[*] Becauſe they impede the circulation of the air, by which the heat would elſe be conducted off. N.

VIII. Though this explanation appeared to me to be very evidently founded upon the knowledge we already poſſeſs, I was nevertheleſs deſirous of ſhewing the truth by a direct experiment. I endeavoured to aſcertain, by means of a thermometer, that the temperature near the ſurface of the phoſphorus which is wrapped in cotton, is more elevated before the inflammation, when the light is perceptibly ſtronger. For this purpoſe, I uſed a thermometer, the bulb of which was about one quarter of an inch in diameter. I faſtened the cotton with which the ſmall ſtick of phoſphorus was ſurrounded, as in the preceding experiments, to the ball of this thermometer, ſo that it was entirely ſurrounded with it, at the ſame time that its diſtance from the ſurface of the phoſphorus was about half a line. The reſult of this experiment anſwered my expectations. I obſerved that the mercury roſe, after the light had increaſed, and that from 52 degrees, which was the original temperature, it had riſen to 67 of Fahrenheit before the phoſphorus had taken fire.

The ſize of the ball of this thermometer appeared to me to be the cauſe why the mercury did not riſe higher before inflammation. I had alſo admitted ſome diſtance between the bulb and the phoſphorus, in order that its contact might not prevent the phoſphorus from acquiring the requiſite degree of heat which is neceſſary for its inflammation. I therefore reſolved to repeat the experiment with a thermometer of the ſame kind as was uſed by Dr. Hunter in his obſervations on the heat of animals and plants, the ball of which was not more than one line in diameter. I faſtened cotton to this in the ſame manner, which was attached to a ſmall ſtick of phoſphorus; but the ball of the thermometer was placed in contact with one of the ends of the ſtick of phoſphorus, which was half a line in diameter, and four lines in length. I then ſaw that while the light round the phoſphorus became ſtronger and ſtronger, the thermometer roſe from 46 to 76 degrees before the phoſphorus took fire. The ſudden heat broke the ball of the thermometer; which prevented my uſing ſimilar thermometers in the repetition of this experiment.

Though this thermometer did not mark the degree of heat which was neceſſary to ſet fire to phoſphorus in the open air, it is ſeen, nevertheleſs, from the experiment, that the temperature riſes very conſiderably at the ſurface of the phoſphorus before it takes fire. The ball of the thermometer which I uſed for this experiment had its glaſs extraordinarily thick at bottom, which was probably one of the reaſons why the thermometer did not indicate an higher temperature before the inflammation, as there is no reaſon to ſuppoſe that phoſphorus will take fire in rarefied air at a leſs temperature than in the atmoſphere *.

IX. Laſtly, I examined whether the air could be rarefied to ſuch a degree that phoſphorus ſhould be incapable of taking fire in it. I ſaw it take fire in air ſo rarefied that the mercury in the gauge ſtood at the height of one ſingle line only. The phenomenon here deſcribed is hitherto, as far as I know, the only example of a true inflammation in air rarefied to the higheſt poſſible degree by the air pump. This fact, however, does in no reſpect prove that a true inflammation can take place in vacuo.

When the mercury in the gauge placed beneath the receiver ſtands at the height of one

---

[*] As the ſolidity of the ball of the thermometer was about half a cubic line, and the phoſphorus little more than three quarters, it ſhould ſeem that, as the heat was ſhared between the mercury and the phoſphoric vapour, the thermometer could not be expected to riſe to the point of combuſtion unleſs it had remained unbroken for a longer time. N.

line, which indicates the utmoſt degree of rarefaction of the air which I have ever obſerved to take place, the rarefied air ſtill poſſeſſes 1-300th of the denſity of the air of the atmoſphere, in caſe the mercury in the barometer be ſuppoſed at 30 inches elevation.

It is certainly very ſingular, that the ſmall quantity of oxygene gas which remains in air ſo rarefied ſhould be ſufficient for the inflammation of phoſphorus; more eſpecially as all other combuſtible ſubſtances are extinguiſhed in air rarefied to a much lower degree. I have already explained the principal reaſon. There is another circumſtance which probably favours this inflammation, of which I ſhall ſpeak hereafter, when I ſhall have made ſome experiments.

X. The phenomenon exhibited by the phoſphorus in rarefied air, as here deſcribed, is certainly a real combuſtion, as is proved by the very remarkable diminution of weight of the phoſphorus after the experiment has been made in a large receiver, or has been ſeveral times repeated. The phoſphoric acid produced by the combination of oxygene with the ſubſtance burned is alſo found on the plate of the air pump.

XI. The combuſtion of phoſphorus in air highly rarefied is accompanied with ſeveral very ſingular phenomena, which I ſhall here mention.

1. The phoſphorus uſually emits, a ſhort time after the commencement of the inflammation, fiery jets, in the form of ſmall ignited balls, which are diſperſed on all ſides in the receiver, and exhibit a very curious and ſurpriſing appearance. I have not hitherto been able to explain this phenomenon.

2. The flame which ſurrounds the phoſphorus when it burns in the rarefied air, extends farther and farther in a globular form. Its light at the ſame time becomes paler and paler, and at length diſappears. This enlargement of the flame, and diminution of the light, are probably to be attributed to the oxygene, to which the phoſphoric exhalations may unite, being gradually exhauſted. The light at laſt entirely diſappears, when all the oxygene gas which exiſted in the rarefied air, and could be reached by the phoſphoric exhalations, is combined with them; for, as ſoon as there is no more oxygene to which the phoſphoric exhalations may unite, there ceaſes to be any further ſeparation of caloric, which cauſes the flame and the illumination.

3. When a ſmall quantity of atmoſpheric air or oxygene gas is ſuffered to enter the upper part of the receiver by a cock after the combuſtion or light of the phoſphorus has diſappeared, a pale light is then ſeen to diſperſe itſelf through the whole capacity of the receiver. This light muſt certainly be attributed to the combination of the oxygene, of the gas which has entered, with the phoſphoric exhalations which exiſt in the receiver. The exhalations which the phoſphorus emits after it has been heated by the inflammation, are apparently more ſubtle and light than thoſe which preceded that phenomenon; and it is probably owing to this reaſon that they can ſupport themſelves in the gas which remained in the receiver, though very highly rarefied.

4. When the apparatus is left untouched for ſome time after the combuſtion of the phoſphorus in the rarefied air has ceaſed, and the whole has become cooled, then the phoſphoric exhalations deſcend in this rarefied air. In proof of this, when the experiment is made in a receiver, on the plate of the air pump, and air is ſuffered to enter by a cock beneath the plate, as uſual, it is then ſeen that the light occaſioned by the entrance of the air takes place only near the plate upon which the phoſphoric exhalations have fallen.

XII. When the atmoſpheric air or oxygene gas enters the receiver by a cock from above, a little before or the inſtant after the light of the combuſtion has diſappeared, and when care is taken to admit no more air or gas than ſhall be ſufficient to raiſe the gauge one or two lines, the phoſphorus uſually takes fire a ſecond time. In this manner the phenomenon may be made to appear repeatedly; and accordingly as a greater or leſs quantity of air is ſuffered to enter, the circumſtances are found to vary in a very remarkable manner. This communication would be too long if I were to attempt to deſcribe the variety of appearances which I have obſerved in theſe experiments. They are not entirely the ſame, in circumſtances which appear perfectly ſimilar. I have obſerved that pieces of phoſphorus obtained by different operations have exhibited different phenomena. The fiery globules emitted by the burning phoſphorus are in ſome caſes larger and more numerous than in others.

MATHEMATICAL AND PHILOSOPHICAL CORRESPONDENCE.

QUESTION V. *Anſwered by the* PROPOSER.

LET x denote the weight of the ſalt, and s its ſpecific gravity; w the weight of the bottle of diſtilled water, and W that of the ſolution; the magnitude, or content, of the bottle, and the ſpecific gravity of the water, being each conſidered as 1.

Then, ſince the magnitudes of bodies of the ſame kind are as their weights, $w : W-x :: 1 : \frac{W-x}{w}$ = magnitude of the aqueous part of the ſolution: and, becauſe the magnitudes of bodies of different kinds are as their weights divided by their ſpecific gravities, $\frac{w}{1} : \frac{x}{s} :: 1 : \frac{x}{ws}$ = magnitude of the ſalt. Whence $\frac{W-x}{w} + \frac{x}{ws} = 1$; from which equation x is found $= \frac{s}{s-1}(W-w)$ = weight of the ſalt. And ſince $s = 2.8$, $W = 2810$ gr. and $w = 2506$ gr. by the queſtion, we ſhall have $x = \frac{2.8}{2.8-1}(2810-2506) = \frac{2.8}{1.8} \times 304 = 472\frac{8}{9}$ gr. the quantity of ſalt required.

And if the caſe be reverſed, by ſuppoſing x to be known, the value of s will be readily found from the general formula; by which means we ſhall be enabled to make a proper allowance for the difference in the ſpecific gravity of the ſalt, ariſing from chemical condenſation, as was done in the propoſing of the queſtion, where it is taken as 2.8 inſtead of $2\frac{1}{2}$, which it is ſuppoſed to be in its ſeparate ſtate.

QUESTION VI. *Anſwered by C. W.*

LET m be put for the mean temperature at the equator, n for the difference between this temperature and that of the north pole, and l for the latitude of any place, radius being 1; then the mean temperature of that latitude will be $m - n \times \text{ſine}^2 l$. For the heat produced by the direct action of the ſun, is as the ſine of the ſun's altitude; ſo that if the earth were equally heated at the equator and poles, the quantity to be added to the polar heat would be $n \times$ ſine of the ſun's altitude, radius being 1. But it has been aſcertained from obſervation, that the heat on the earth's ſurface (*ceteris paribus*), and conſequently the heat emitted by the earth itſelf, is always proportional to the direct ſolar heat; whence the quantity to be added muſt be $n \times$ the ſquare of the ſine of the ſun's altitude: or, ſince the mean annual altitude of the ſun, in any latitude, is equal to the complement of that latitude, the quantity to be added to the polar heat $(m - n)$ will be $n \times \text{coſ.}^2 l$; but $\text{coſ.}\, l = \sqrt{1 - \text{ſine}^2 l}$; conſequently, $m - n + n \times \text{coſ.}^2 l = m - n + n - n \times \text{ſine}^2 l = m - n \times \text{ſine}^2 l$. Hence, if the mean temperature of any two latitudes be known, the temperature under the equator, at the pole, and in every intermediate latitude, may be readily aſcertained.

The mean temperature of lat. 40° is found to be 62.1, and of lat. 50° to be 52.9; but the ſquare of the ſine of 40° is ·413, and the ſquare of the ſine of 50° is ·586; whence

$$m - {\cdot}413n = 62{\cdot}1$$
$$m - {\cdot}586n = 52{\cdot}9;$$

From which equations n is found $= 53$ nearly, and $m = 84$. So that, the mean annual temperature at the equator being 84°, that of the pole muſt be $84 - 53 = 31°$; and the mean annual temperature of every intermediate latitude will be $84 - 53 \times$ the ſquare of the ſine of that latitude.

NEW MATHEMATICAL QUESTION.

QUESTION X. *By W. THOMSON.*

IT is required to find at what time of the longeſt day it is the hotteſt in London, ſuppoſing the heat to be as the ſine of the ſun's altitude, and the time of its continuance above the horizon.

PHILOSOPHICAL QUESTION.

THERE is an optical appearance ſo frequent, that it is rather ſurpriſing that writers on that ſcience have never mentioned it. Whenever the ſun ſhines upon agitated water not abſolutely loaded with opake matter, and the ſpectator is ſo placed that the ſhadow of his head may be projected upon the ſurface of the fluid, he will ſee an innumerable quantity of divergent rays within the water, of which that ſhadow is the centre. They are inceſſantly ſhifting their place laterally; and if more perſons than one are preſent, each ſees a ſyſtem of radiations or glory round his own head, but no ſuch appearance round the ſhadows of the other perſons, though theſe alſo are very viſible to him. As this phenomenon has a ſtriking effect when obſerved, and may be accounted for upon the common principles of optics and perſpective, it is offered to correſpondents as an object for explanation.

To Mr. Nicholson.

SIR, *London, 22d Auguſt* 1797.

UPON peruſing the account given in your laſt number, of the phenomenon of the Fata Morgana, and upon conſulting the plate annexed by way of explanation, I was very much puzzled to comprehend how the ſpectator, " placed on an eminence of the city, with his back to the riſing ſun, and his face to the ſea," looking of courſe towards the weſt, could at once enjoy the ſight of this truly admirable phenomenon, a diſtant view of the *weſtern* aſpect of the city, and alſo of the mountains *behind* him. A few words in a future number, in explanation of this ſtrange combination, will much oblige,

Sir,
Your conſtant reader,
DAVUS.

⁂ THE difficulty ſtated by this correſpondent, together with ſeveral others, engaged my attention at the time the account (p. 225) was drawn up: but as they ſeemed ſcarcely capable of being cleared up by reaſoning, while the theory remained ſo very uncertain, I thought it beſt to avoid entering the ample field of conjecture which offered itſelf. It ſeems altogether improbable, that the rays of light ſhould be *reflected* immediately back to the city, and every part of the drawing directly contradicts this ſuppoſition. I am therefore inclined to conclude that the deſcription, though literally tranſlated, is faulty, ſo far as it contradicts the notion of the obſerver being ſo placed as to view the city over a portion of the bay. I imagine that the people, when they run haſtily to the ſea exclaiming Morgana! do not run to the ramparts of the town, but to the ſouthern point of the bay, at the diſtance of half a mile or leſs from the town; whence, with the ſun* behind them, they may have an oblique view of Reggio ſtrongly illuminated, and a more direct proſpect of the northern ſhore of the bay.

NEW PUBLICATIONS.

An Account of two Caſes of the Diabetes Mellitus, with Remarks as they aroſe during the Progreſs of the Cure; to which are added, A general View of the Nature of the Diſeaſe, and its appropriate Treatment; including Obſervations on ſome Diſeaſes depending on Stomach Affection, and a Detail of the Communications received on the Subject ſince the Diſperſion of the Notes on the firſt Caſe. By John Rollo, M. D. Surgeon General, Royal Artillery.——With the Reſults of the Trials with various Acids and other Subſtances in the Treatment of the Lues Venerea; and ſome Obſervations on the Nature of Sugar, &c. By William Cruickſhank, Chemiſt to the Ordnance, &c. in two Volumes Octavo, 14s. Dilly.

IN this treatiſe Dr. Rollo has conſidered the diabetes mellitus as a diſeaſe of the organs of digeſtion, and not of the kidneys; he conceives that the ſugar which paſſes off by urine

---

[*] The ſun's rays can never make an angle of 45° on the ſea at Reggio, from the azimuth of the image in the drawing.

is formed in the ſtomach, and depends chiefly upon ſome vitiated, but increaſed action of this organ. He was led to take this view of the diſeaſe from reflecting on the ſtate of the ſtomach and habits of life which preceded, the voracious appetite which always accompanies it, and the ſtate of the blood, the ſerum of which, although not ſenſibly ſweet, had not the uſual ſaltiſh taſte.

The Doctor ſuppoſes, that in this complaint the vegetable matter taken into the ſtomach has not, from ſome defect in this organ, undergone a ſufficient change to form proper chyle; that in conſequence of this much ſaccharine matter is evolved, which, when carried into the circulation, proves a general ſtimulus, producing head-aches, and quickneſs of pulſe, but that it acts more remarkably on the kidneys, occaſioning a conſtant and copious ſecretion of ſweet urine. From this hypotheſis, he was naturally led to adopt a plan of cure, which has proved completely ſucceſsful. The indications he lays down are: 1. To prevent the formation of ſaccharine matter in the ſtomach; and, 2. To remove the morbidly increaſed action of this organ, and reſtore it to a healthful condition. Theſe indications are to be anſwered by a complete diet of animal food, and by the uſe of ſuch medicines as ſhall diminiſh the action of the ſtomach, and at the ſame time counteract the formation of ſaccharine matter. The remedies employed for this purpoſe have been emetics, kali ſulphuratum, lime-water, hepatized ammonia, and vegetable narcotics. But the principal dependence is to be placed on a total abſtinence from all vegetable matter, which alone can ſupply the ſaccharine principle. By a regular perſeverance in this plan, the firſt patient was completely cured in four weeks, although the diſeaſe had been of ſeven months continuance. The urine, which at the commencement of the treatment was ſweet, and amounted to 24 pints daily, was at laſt reduced to 1½ pint, being at the ſame time free from any ſaccharine impregnation.

The ſecond patient, from his age and other circumſtances, although relieved from the diabetic affection, did not regain his wonted ſtate of health; but even in this caſe, the effects produced by the treatment, when properly attended to, were moſt decidedly in confirmation of this plan of cure.

The Doctor has received ſeveral communications in conſequence of the diſperſion of the printed notes on the firſt caſe. The moſt important are the reſult of two caſes treated in this way by Dr. Cleghorn of Glaſgow, and one by Drs. Currie and Gerard at Liverpool; all of which afford the ſtrongeſt corroboration of the efficacy of this mode of treatment.

To this account of the diabetes are added ſome experiments on ſugar, and the effects of different acids in the lues venerea. The caſes of lues venerea treated in this way, and detailed at ſome length, are: 17 by Mr. Cruickſhank, 2 by Dr. Irwin, 5 by Dr. Jameſon, and 8 by Dr. Wittman—making in all 32. Of theſe 19 were cured by the nitrous acid, 4 by the oxygenated muriatic acid, 3 by lemon-juice, or the citric acid, and 6 by the oxygenated muriate of pot-aſh. The affections in all theſe were of the primary kind, being chancres and buboes, but diſtinctly marked. The effects produced by the different remedies were nearly the ſame, and ſuch as ſeemed to indicate a general increaſed action of the ſyſtem. There was an increaſe of appetite, more thirſt than uſual, a white tongue, an augmentation in the quantity of urine, and the blood when drawn was generally ſizy: nothing, however, like ſalivation was obſerved.

The cures in general ſeemed to have been performed in leſs time than would have been neceſſary under the mercurial plan; and without any confinement or particular regimen. The effect is ſuppoſed to have been produced by the diſengagement of oxygene from the different ſubſtances employed, inducing a new diſeaſe in the ſyſtem. Of the different remedies, the preference is given to the nitrous acid and the oxygenated muriate of pot-aſh:—of the firſt, from one to three drachms were given daily, diluted with about a quart of water, and of the oxygenated muriate of pot-aſh from ſix to ſixteen grains four times a-day. No external applications were employed, but milk and water, or a very dilute ſolution of the ceruſſa acetata, merely to keep the parts clean. At the time this publication went to preſs, no relapſes had taken place, although ſome of them had been cured upwards of three months.

Annals of Medicine for the Year 1796. Exhibiting a conciſe View of the lateſt and moſt important Diſcoveries in Medicine and Medical Philoſophy. By Andrew Duncan, Sen. M. D. and Andrew Duncan, Jun. M. D. Fellows of the Royal College of Phyſicians, Edinburgh, Vol. I. octavo, 469 pages. Edinburgh, printed for Mudie and Son, and for Robinſons, London, 1796.

The Medical Commentaries of the elder Dr. Duncan, in twenty volumes, publiſhed at Edinburgh, are well known to the public. The Annals of Medicine are offered as a continuation of that work, from which the plan will not materially differ. The editors, from whoſe preface I give this account, expect that, when peace ſhall be again eſtabliſhed, their accounts of foreign medical literature will be ſuperior to what the Engliſh reader has been hitherto accuſtomed to receive.

Reflecting practitioners are invited to uſe this work occaſionally as a channel of public communication of ſuch practical obſervations and facts as they may think worthy of being ſo diffuſed. Theſe may be tranſmitted either to Dr. Duncan of Edinburgh, or Dr. Pearſon of Leiceſter Square, London.

The volume conſiſts of four ſections. The firſt contains analyſes of books, twenty in number; the ſecond, medical obſervations or caſes; the third, medical news; and the fourth, a liſt of new books.

Profeſſional men will not require to be informed of the utility of publications of this nature; and there is no queſtion reſpecting the ability of the editors. If it were practicable, with conſiſtent brevity, to give any analyſis of the contents of ſuch a work, it would on theſe accounts be the leſs neceſſary to make the attempt. I ſhall therefore only remark, that the number of new and intereſting articles in this volume is conſiderable.

A Narrative of the ſucceſsful Manner of cultivating the Clove-Tree, in the Iſland of Dominica, one of the Windward Charibbee Iſlands. By William Urban Buée, Eſq. London, printed 1797. Quarto, 31 Pages, with an Engraving of the Clove-Tree, and ſome Implements for planting it. No bookſeller's name, nor price known.

This pamphlet was printed by order of the Privy Council, in conſequence of a Report from the Right Hon. Sir Joſeph Banks, Bart to whom it was referred, as appears in a letter from that gentleman to the Earl of Liverpool, which forms part of the Appendix. From that communication it appears, that Mr. Buée is the firſt perſon who has obſerved that the pimento tree proſpers beſt in thoſe ſterile ſoils where trees whoſe wood is of a hard texture abound, and that ſugar cannot be cultivated to advantage in ſuch places; and alſo, on the other hand, that where trees whoſe wood is ſoft are naturally found, pimento trees are rarely met with, and ſugar plantations will ſucceed.

Mr. Buée obſerves, that in the Weſt Indies, particularly in Dominica, moſt lands facing the eaſt are of a yellowiſh or reddiſh ſtiff clay; a ſoil which, with few exceptions, is hardly fit for any cultivation, but is productive of the hard wood trees. Partly by the obſervation of incidents which preſented themſelves, and partly from a rational proceſs of inveſtigation of the ſubject, the author has applied theſe facts to the clove-tree, which is of a hard cloſe grain, though not ſo tough as the pimento. In the Moluccas, where the clove-trees grow, the ground is covered with them, and will not admit the culture of any thing elſe. It was not, however, till after a number of experiments, during a ſeries of years, that he arrived at the poſſeſſion of ſeveral bearing clove-trees. The hiſtorical account of his proceedings with regard to the beſt method of planting the tree, and of rendering the cloves merchantable, is intelligent and clear. For this, however, I muſt refer the reader to the work itſelf. Mr. Buée has alſo ſucceeded in propagating the cinnamon-tree, of which he poſſeſſes a great number, and promiſes to make it the ſubject of his future remarks.

Theſe obſervations, to uſe the words of Sir Joſeph Banks, open to the cultivators of hot climates a new ſource of wealth, which will not probably be confined to the growth of cloves. Other ſpices may alſo proſper beſt in the barren ſoils of the Weſt Indies, as lavender, thyme, and other aromatic plants are known to do in thoſe of Europe.

Beſides the letter of Sir Joſeph Banks, the Appendix contains a liſt of the uſeful plants cultivated in the Royal Gardens at the Iſle of France in 1790; and a letter from Mr. Rutton of Charing-Croſs, expreſſing the uniform opinion of ſeveral eminent grocers, that certain ſamples of Mr. Buée's cloves, forwarded to the Council by Sir Joſeph, will anſwer every culinary purpoſe as well as thoſe of the Spice Iſlands in the Eaſt Indies.

◆

Fig. 1.

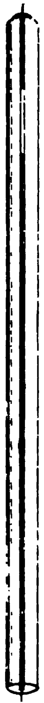


Fig. 2.

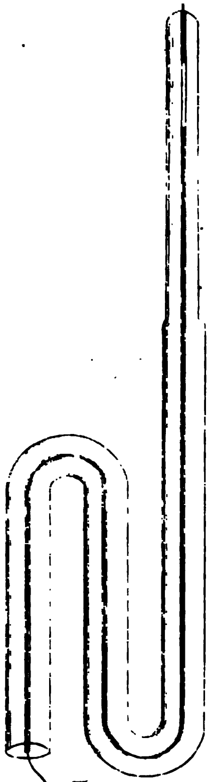


Fig. 3.

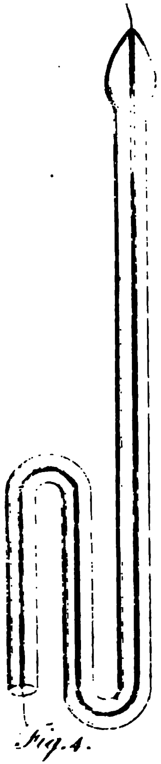


Fig. 4.

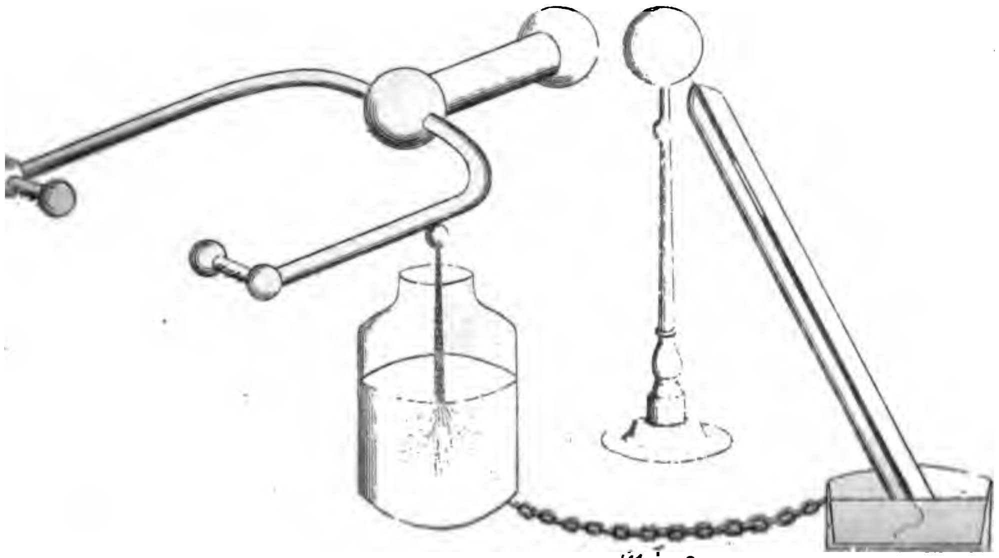


Fig. 5.

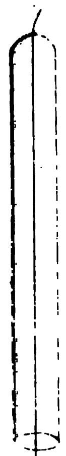


Fig. 6.

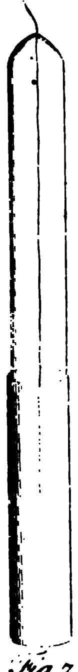


Fig. 7.

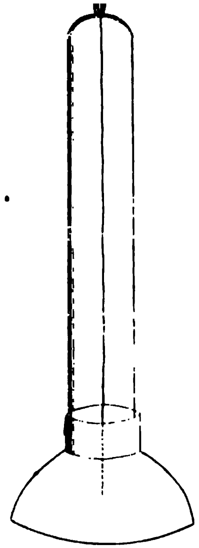


Fig. 8.

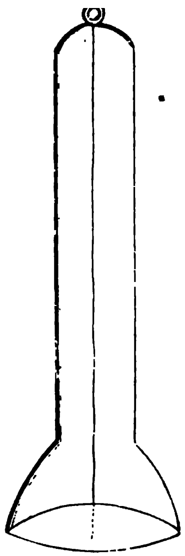


Fig. 9.

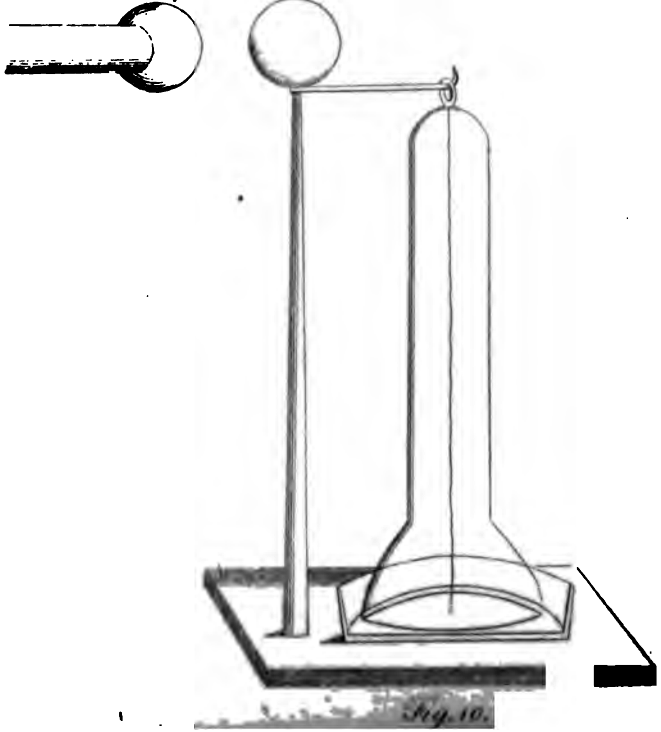
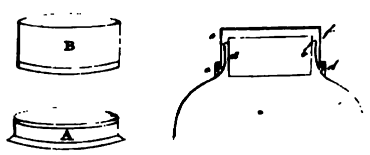


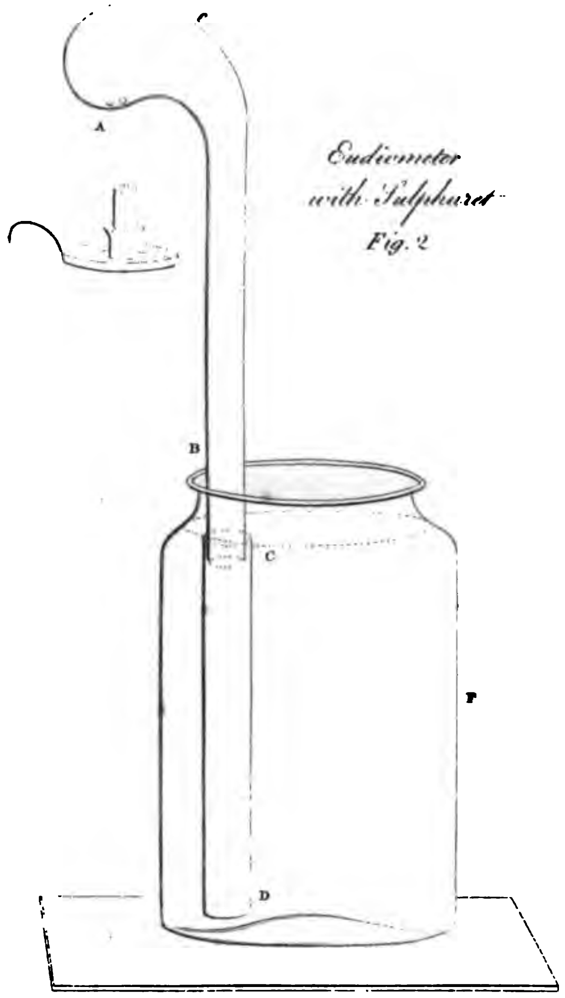
Fig. 10.

THE NEW [illegible]
PUBLIC [illegible]

Method of closing Vessels

Fig. 1





Eudiometer with Sulphuret

Fig. 2

Electrometer

Fig. 3

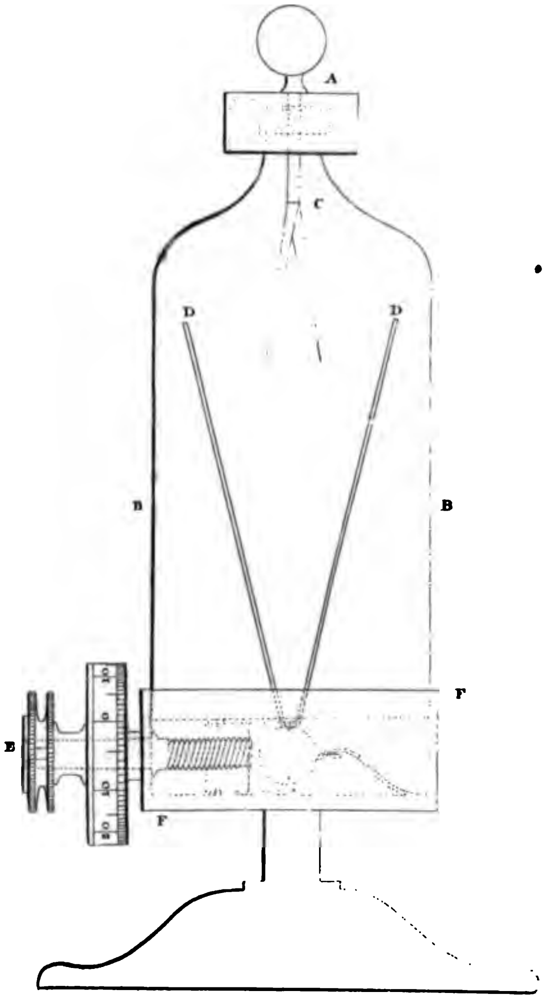
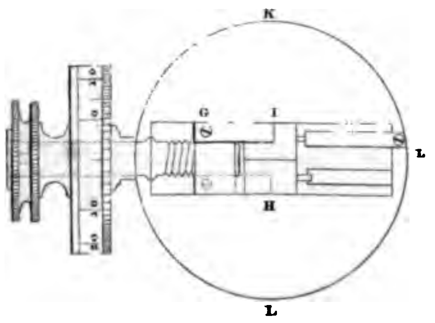


Fig. 4



THE NEW YORK
PUBLIC LIBRARY

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER 1797.

ARTICLE I.

An Account of the Manner in which Heat is propagated in Fluids, and its general Conſequences in the Economy of the Univerſe. By BENJAMIN *Count of* RUMFORD [*].

THE doctrine of heat is of ſuch ſingular importance, not only in the experiments of philoſophers, but in the whole economy of animated and of inanimate beings, that at firſt ſight it appears wonderful the greateſt part of the diſcoveries relative to this natural power ſhould have been made by our cotemporaries. Not many years ago, our knowledge amounted to little more than that bodies, by the communication of heat, acquire a common temperature; a fact which, ſimply expreſſed, denotes nothing more than that there is a ſtate of equilibrium, at which two bodies may be in contact, without the communication of heat from the one to the other; and again, that the communication of heat is more rapidly effected through ſome bodies than through others. The diſcoveries of Doctors Black, Irvine, Crawford, and others, have taught us, that when bodies equal in weight or bulk, or alike in any other attribute (their intimate nature or compoſition excepted), are brought into contact, they do not, by acquiring the common temperature, occaſion an equal change in the ſenſible heat of each. In ſome inſtances the heavier body muſt part with a greater number of degrees of its temperature before an equilibrium of communication becomes eſtabliſhed between itſelf and a lighter body; and in other inſtances the contrary will be the caſe. If, therefore, water be uſed as a ſtandard, and applied in like temperatures and circumſtances to different bodies, the common temperature will differ accordingly as the bodies themſelves require a greater or leſs portion of heat to occaſion ſimilar changes in their ſenſible heat. Theſe important reſults conſtitute the foundation of the theory of the different capacities of bodies for heat, or their ſpecific heats when at the ſame temperature.

---

[*] Abſtract or abridgement from his writings, chiefly the VIIth Experimental Eſſay.

Another diſcovery[*], of no leſs magnitude, ſeemed to render us acquainted with the principal cauſes by which the mutations of temperature are originally effected. It was found by experiment, not only that different bodies require greater or leſs quantities of heat to be communicated in order to raiſe their temperature through equal numbers of degrees, but alſo that the capacity of any individual ſubſtance is leaſt when in the ſolid ſtate, greater in the fluid ſtate, and greateſt of all when converted into elaſtic vapour or air. So that, as a natural conſequence, a much greater portion of heat will be required to convert ice into water, and raiſe its temperature ten degrees, than would have been neceſſary to have equally elevated the temperature of cold ice without melting it. And ſo likewiſe the vaporization of water, without raiſing its temperature beyond the boiling-water point, requires a much larger quantity of heat than would have heated the ſame maſs of water from the freezing to the boiling point.

A great number of intereſting deductions, from theſe facts, have been applied to explain the general mutations of heat in the univerſe around us. For, in the firſt place, ſince every change of chemical combination is attended with a greater or leſs change of the capacities of the aggregate, it almoſt invariably happens that the temperature of the new compound is either above or below the common temperature of the atmoſphere; and as the fuſibilities of theſe compounds are alſo affected according to laws of which at preſent we know nothing, there is very frequently a change of temperature on this account alſo.

Such is the moſt luminous and beautiful theory of heat which our cotemporaries have begun to develop, and which is at preſent explained at conſiderable length in moſt elementary works. But as this ſubject appears to depend upon facts ſcarcely capable of being extended farther by hypotheſis or analogical reaſoning, it has happened that inductions experiments and applications, though of the greateſt value, have been but ſlowly made. The effect of chemical operations on the temperature of bodies has been applied with great ſucceſs to explain the act of combuſtion; and the general conſequences of the change of capacity in the ſolid, fluid, and vaporous ſtates have been conſidered with regard to their extenſive influence upon the face of the globe. But thoſe reſults which originate chiefly from the differences of conducting power in bodies with regard to heat, have ſcarcely conſtituted an object of direct enquiry among philoſophers, excepting by Count Rumford in the Philoſophical Tranſactions, and in his Experimental Eſſays, at preſent in the progreſs of publication. On the preſent occaſion I ſhall not enter into the conſideration of the extenſive economical uſes to which this philoſopher has applied the reſults of his experiments and deductions, but ſhall chiefly confine the preſent memoir to the conſequences which he has ſhewn to ariſe from the imperfect conducting power of fluids with regard to heat.

The free paſſage of heat, in all directions, through all kinds of bodies, has never yet been called in queſtion, though the rapidity of its tranſmiſſion is well known to differ exceedingly in various kinds of bodies. Under the influence of this opinion Count Rumford began his experiments on heat. His former experiments ſhewed that air is a non-conductor of heat; and the late experiments contained in his ſeventh Eſſay aſcertain that water is in the ſame predicament. He thinks that all other fluids have the ſame property. He was led to an experimental inveſtigation of this curious ſubject by certain accidental events which he relates. Theſe ſhewed, that when the circulation of water is impeded by mucilage or by fibrous matter, as in apple-pie and thick rice-ſoup, the heat is not only a long time in eſcaping, but may be very conſiderable in one part of the maſs, while other parts are nearly cold. The baths of Baia afforded another inſtance of the ſame nature, which is very ſtriking. When the Count was ſtanding on the ſea-ſhore, near the baths, where the hot ſteam was iſſuing out of every crevice of the rocks, and even riſing up out of the ground, he had the curioſity to put his hand into the water. He was not ſurpriſed to find that the waves of the ſea, which inceſſantly followed each other over the beach, ſhould feel cold; but he was more than ſurpriſed when, on running the ends of his fingers into the ſand beneath the water, he found the heat quite intolerable. The ſand was perfectly wet, and yet the temperature was ſo very different at the ſmall diſtance of two or three inches! He even found that the ſurface of the ſand was to all appearance quite as cold as the water which flowed over it. He could not reconcile this to the ſuppoſed great conducting power of water; and then, for the firſt time, in conſequence of his doubts reſpecting the exiſtence or intenſity of this power, he determined to make experiments to aſcertain the fact. Theſe however were delayed, and probably might have remained undone, if another unexpected appearance had not revived his curioſity.

In the courſe of a ſet of experiments on heat, Count R. had occaſion to uſe thermometers of uncommon ſize, their globular bulbs being above four inches in diameter filled with various liquids. One of theſe, containing alcohol much heated, was placed in a window where the ſun happened to be ſhining; when, caſting his eyes on the tube, which was quite naked, and divided by means of a diamond, he ſaw the whole maſs of its contents in a moſt rapid motion, in two oppoſite directions, up and down at the ſame time. Theſe motions were rendered viſible by ſome particles of fine duſt that happened to be in the ball before it was filled. The tube was $\frac{43}{100}$ths of an inch in diameter; and upon examination with a lens, the riſing current of ſpirit was ſeen to occupy the axis or internal part of the tube, and the deſcending ſtream was contiguous to the ſides. When the tube was inclined, the riſing current occupied the uppermoſt ſide, and the deſcending ſtream the lower. The velocities were perceptibly increaſed by wetting the tube with ice-cold water; they became gradually leſs as the thermometer was cooled, and ceaſed when the fluid had acquired the common temperature of the room; and the motion was greatly prolonged when the cooling of the bulb was impeded by wrapping it in furs or any other warm covering.

The ſame experiments, with motion of the ſame kind, and quite as rapid, were repeated with a ſimilar thermometer filled with linſeed-oil.

From theſe facts Count Rumford was led to conclude that the fluids he had tried, and probably all others, are in fact non-conductors* of heat; that they tranſmit this matter or quality, by actual motions, from the heated to the cold bodies with which they may ſucceſſively come into contact; and that every means of obſtructing or retarding thoſe motions would render the propagation of heat more ſlow and difficult. He had before found that this was actually the caſe with air, and on the preſent occaſion he proceeded to give the ſubject a thorough and careful inveſtigation.

---

* Throughout this moſt valuable eſſay, the author ſpeaks of fluids, and particularly water, as *perfect non-conductors of heat*; which ſeems to me to be an inaccurate expreſſion of the facts. His experiments, hereafter to be related, may prove that the direct communication of heat through a line of particles of a fluid, on the ſuppoſition of their continuing immoveable, is extremely ſlow; but not that it does not take place. If each individual particle were not capable of receiving and giving heat, the proceſs by circulation could not happen; and if on the contrary they be capable, the propagation of heat from particle to particle is poſſible, and muſt doubtleſs happen; though from the facts it appears that the heat conveyed by the internal motion is very much greater than what paſſes in this way. Fluids then are very imperfect conductors of heat, but not perfect non-conductors. N.

The apparatus made uſe of for the firſt courſe of experiments conſiſted of a large thermometer, which he calls the paſſage thermometer, conſiſting of a cylindrical veſſel with hemiſpherical ends, forming the bulb, and a glaſs tube fitted into a neck by means of a good cork. Its dimenſions were as follow:—Diameter of the bulb 1.84 inches—Length 4.99—Capacity or contents 13.2099 cubic inches—External ſuperficies 28.834 ſuperficial inches—Thickneſs of the ſheet copper 0.03 inch—Weight when empty 1846 grains, and it contains 3344 grains of water at 55° of temperature. The glaſs tube is 24 inches long, and $\frac{4}{10}$ths of an inch in diameter, and the cylindrical neck of copper into which it is fitted by means of cork is one inch long, and $\frac{65}{100}$dths of an inch in diameter.

This thermometer, being filled with linſeed-oil, and its ſcale graduated, was fixed in the axis of a hollow cylinder of thin ſheet copper, 11½ inches long, and 2.3435 inches in diameter internally. This cylinder, which is open at one end, is cloſed at the other with an hemiſpherical bottom with its convex ſide outwards. The bulb of the thermometer was confined in the axis of this caſe, by three ſmall wooden pins, inſerted in ſockets within the large braſs tube, and the upper end was properly ſecured by cauſing the glaſs tube to paſs through a cork ſtopper adapted to the ſame metallic cylinder. The bottom of the bulb reſted on a wooden pin fixed in a ſocket in the middle of the hemiſpherical bottom of the caſe. All the pins terminated in blunt wooden points, to reduce the contacts as much as poſſible.

The ſpace between the thermometer when in its place, and the internal ſurface of the ſurrounding cylinder, was deſigned to contain the ſubſtance through which the heat was made to paſs into or out of the thermometer; the temperature of this laſt maſs being ſhewn by the graduations on the glaſs tube. The quantity of water required to fill the ſpace, and cover the bulb of the thermometer about one quarter of an inch, was found to weigh 2468 grains.

When the bulb of the thermometer was ſurrounded in its place by water or any other liquid or mixture intended to be tried, a cylinder of cork, rather leſs in diameter than the internal cavity, was ſlipped down upon the tube, not quite ſo low as the water or mixture. Above this was placed a quantity of eider-down, ſufficient to fill the remaining cavity, except what was occupied by the cork ſtopper, laſt of all to be inſerted. The thermometer was divided according to Fahrenheit's ſcale, and the whole ſcale, from the freezing point to the boiling-water point, was above the ſtopper.

The operations with this apparatus were performed by placing the prepared inſtrument in melting ice till the thermometer fell to 32°. It was then immediately plunged into a large veſſel of boiling water, and the conducting power of the ſubſtance under examination was eſtimated by the time the heat employed in paſſing through it into the thermometer; the time being carefully noted when the liquid in the thermometer arrived at the 40th degree of its ſcale; and alſo when it came to every 20th degree above it.

In the reverſe operation, the inſtrument was kept in boiling water till its temperature appeared ſtationary, when it was taken out, and immediately plunged in melting ice, and the times of its deſcent carefully noted.

As ſoon as this apparatus was completed, the Count was deſirous of aſcertaining whether apples which continue hot ſo long in pies made of that fruit, do really poſſeſs a power of retaining heat greater than that of pure water, of which for the moſt part they conſiſt; but in the firſt place he aſcertained the quantity of fibrous matter in apples, by ſtewing two ounces troy of the pulp, and waſhing off the ſoluble matter with a large quantity of cold water. The fibrous remainder, when thoroughly dried, weighed only 25 grains, which, by remaining for ſeveral days in a plate on the top of a heated German ſtove, was further reduced to $18\frac{9}{10}$ths, or leſs than 1-50th part of the whole maſs.

The reſults obtained by ſurrounding the bulb of the paſſage thermometer with a quantity of ſtewed apples, ſo conſiſtent as not to exhibit ſigns of fluidity, and then expoſing the apparatus to the heating and cooling proceſſes, are tabulated in the original Eſſay. The conducting power of the ſtewed apples proved to be little more than half that of pure water; that is to ſay, the heat was nearly twice as long in paſſing through the former as the latter.

As the attention of our author was ſteadily fixed on the poſition that heat is communicated by fluids, only by its being tranſported by virtue of their inteſtine motion produced by the change of ſpecific gravity, he concluded that there may be two ways of obſtructing this propagation of heat; namely, by diminiſhing their fluidity, which may be done by ſolution of any mucilaginous ſubſtance; or, more ſimply, by impeding the motion of their particles, which may be effected by mixing any ſolid ſubſtance with them which is an imperfect conductor of heat, and of an enlarged ſurface by being divided into ſmall maſſes.

In the experiments with ſtewed apples, the paſſage of the heat in the water, which conſtituted by far the greateſt part of the maſs, was doubtleſs obſtructed in both theſe ways. The mucilaginous part of the apples diminiſhed very much the fluidity of the water, at the ſame time that the fibrous parts ſerved to embarraſs its motions.

In order to diſcover the comparative effects of theſe two cauſes, water was boiled with about 1-12th part of its weight of ſtarch, and examined by the apparatus. The ſame weight of eider-down was alſo boiled with a like quantity of water. The intention of this laſt boiling was to free the eider-down from water. From the tabulated experiments, it appears that theſe ſeveral additions impaired the conducting power of water nearly to a degree of equality with the ſtewed apples.

In the Philoſophical Tranſactions for 1792, where Count Rumford has aſcertained that heat is actually propagated in air in the ſame manner as it is here ſtated to be propagated in water, he found that the thickneſs of a ſtratum of air, which ſerved as a barrier to heat, remaining the ſame, the paſſage of heat through it was ſometimes rendered more difficult by increaſing the quantity of the light ſubſtance, which was mixed with it to obſtruct its internal motion. To ſee if ſimilar effects would be produced with water, he repeated the experiments with eider-down, reducing the quantity of it mixed with water to one fourth of the quantity uſed in the former experiments. The reſiſtance to the paſſage of heat was conſiderably diminiſhed.

The reſults of theſe experiments are extremely intereſting; they not only make us acquainted with a new and very curious fact, namely, that feathers, and other like ſubſtances, which in air are known to form very warm coverings for confining heat, may ſerve the ſame purpoſe in water, but that their effects in preventing the paſſage of heat are even greater in water than in air.

This diſcovery is very happily applied by the Count to elucidate ſome of the moſt intereſting parts of the economy of nature, on which ſubject I ſhall give his inferences very nearly in his own words.

As liquid water is the vehicle of heat and nouriſhment, and conſequently of life in every living thing; and as water, left to itſelf, freezes with a degree of cold much leſs than that which frequently prevails in cold climates, it is agreeable to the ideas we have of the Creator of the world, to expect that effectual meaſures would be taken to preſerve a ſufficient quantity of that liquid in its fluid ſtate, to maintain life during the cold ſeaſon: and this we find has actually been done; for both plants and animals are found to ſurvive the longeſt and moſt ſevere winters: but the means which have been employed to produce this admirable effect have not been inveſtigated; at leaſt not as far as they relate to vegetables.

But as animal and vegetable bodies are eſſentially different in many reſpects, it is very natural to ſuppoſe that the means would be different which are employed to preſerve them againſt the fatal effects which would be produced in each by the congelation of their fluids.

Among organized bodies which live on the ſurface of the earth, and which of courſe are expoſed to the viciſſitudes of the ſeaſon, we find, that as the proportion of fluids to ſolids is greater, the greater is the heat which is required for the ſupport of life and health; and the leſs are they able to endure any conſiderable change of their temperature.

The proportion of fluids to ſolids is much greater in animals than in vegetables; and in order to preſerve in them the great quantity of heat which is neceſſary to the preſervation of life, they are furniſhed with lungs, and are warmed by a proceſs ſimilar to that by which heat is generated in the combuſtion of inflammable bodies.

Among vegetables, thoſe which are the moſt ſucculent are annual. Not being furniſhed with lungs to keep the great maſs of liquids warm which fill their large and ſlender veſſels, they live only while the genial influence of the ſun warms them, and animates their feeble powers; and they droop and die as ſoon as they are deprived of its ſupport.

There are many tender plants to be found in cold countries which die in the autumn, the roots of which remain alive during the winter, and ſend off freſh ſhoots in the enſuing ſpring. In theſe we ſhall conſtantly find the roots more compact and denſe than the ſtalk, or with ſmaller veſſels and a ſmaller proportion of fluids.

Among the trees of the foreſt, we ſhall conſtantly find that thoſe which contain a great proportion of thin watery liquids not only ſhed their leaves every autumn, but are ſometimes frozen and actually killed in ſevere froſts. Many thouſands of the largeſt walnut-trees were killed by the froſt in the Palatinate during the very cold winter in the year 1788; and it is well known that few, if any, of the deciduous plants of our temperate climate would be able to ſupport the exceſſive cold of the frigid zone.

The trees which grow in thoſe inhoſpitable climates, and which brave the cold of the ſevereſt winters, contain very little watery liquids. The ſap which circulates in their veſſels is thick and viſcous, and can hardly be ſaid to be fluid. Is there not the ſtrongeſt reaſon to think that this was ſo contrived for the expreſs purpoſe of preventing their being deprived of all their heat, and killed by the cold during the winter?

We have ſeen by the foregoing experiments, how much the propagation of heat in a liquid is retarded by diminiſhing its fluidity; and who knows but this may continue to be the caſe as long as any degree of fluidity remains?

As the bodies and branches of trees are not covered in winter by the ſnow which protects their roots from the cold atmoſphere, it is evident that extraordinary meaſures were neceſſary to prevent their being frozen. The bark of all ſuch trees as are deſigned by nature to ſupport great degrees of cold, forms a very warm covering; but this precaution alone would certainly not have been ſufficient for their protection. The ſap, in all trees which are capable of ſupporting a long continuance of froſt, grows thick and viſcous on the approach of winter. What more important purpoſe could this change anſwer, than that here indicated? And it would be more than folly to pretend that it anſwers no uſeful purpoſe at all.

We have ſeen by the reſults of the foregoing experiments, how much the ſimple embarraſſment of liquids in their internal motions tends to retard the propagation of heat in them, and conſequently its paſſage out of them: and when we conſider the extreme ſmallneſs of the veſſels in which the ſap moves in vegetables, and particularly in large trees;—when we recollect that the ſubſtance of which theſe ſmall tubes are formed is one of the beſt non-conductors of heat known *;—and when we advert to the additional embarraſſments to the paſſage of the heat which ariſe from the increaſed viſcoſity of the ſap in winter, and to the almoſt impenetrable covering for confining heat which is formed by the bark, we ſhall no longer be at a loſs to account for the preſervation of trees during the winter, notwithſtanding the long continuation of the hard froſts to which they are annually expoſed.

On the ſame principles we may, I think, account, in a ſatisfactory manner, for the preſervation of ſeveral kinds of fruit; ſuch as apples and pears, for inſtance, which are known to ſupport, without freezing, a degree of cold which would ſoon reduce an equal volume of pure water to a ſolid maſs of ice.

At the ſame time that the compact ſkin of the fruit effectually prevents the evaporation of its fluid parts, which, as is well known, could not take place without occaſioning a very great loſs of heat, the internal motions of thoſe fluids are ſo much obſtructed by the thin partitions of the innumerable ſmall cells in which they are confined, that the communication of their heat to the air ought, according to our hypotheſis, to be extremely ſlow and difficult. Theſe fruits do, however, freeze at laſt, when the cold is very intenſe; but it muſt be remembered that they are compoſed almoſt entirely of liquids, and of ſuch liquids as do not grow viſcous with cold; and moreover that they were evidently not deſigned to ſupport for a long time very ſevere froſts.

Parſnips and carrots, and ſeveral other kinds of roots, ſupport cold without freezing ſtill longer

---

* I lately by accident had occaſion to obſerve a very ſtriking proof of the extreme difficulty with which heat paſſes in wood. Being preſent at the foundry at Munich when cannons were caſting, I obſerved that the founder uſed a wooden inſtrument for ſtirring the melted metal. It was a piece of oak plank, green, or unſeaſoned, about ten inches ſquare, and two inches thick, with a long wooden handle, which was fitted into a hole in the middle of it. As this inſtrument was frequently uſed, and ſometimes remained a conſiderable time in the furnace, in which the heat was moſt intenſe, I was ſurpriſed to find that it was not conſumed; but I was ſtill more ſurpriſed, on examining the part of the plank which had been immerſed in the melted metal, to find that the heat had penetrated it to ſo inconſiderable a depth, that at the diſtance of one-twentieth of an inch below its ſurface the wood did not ſeem to have been in the leaſt affected by it. The colour of the wood remained unchanged, and it did not appear to have loſt even its moiſture. R.

longer than apples and pears; but theſe are leſs watery, and I believe the veſſels in which their fluids are contained are ſmaller: and both theſe circumſtances ought, according to our aſſumed principles, to render the paſſage of their heat out of them more difficult, and conſequently to retard their congelation.

[*To be continued.*]

II.

Experiments and Obſervations on the fulminating Preparations of Gold and Silver *.

THE beſt proceſs for the preparation of fulminating gold being ſhewn; the principles furniſhed by the acids during the ſolution of the gold, and thoſe furniſhed by the ammoniac during the precipitation, and the order neceſſary in the application of them, were conſidered, and divers experiments were related, by which it appeared that this ſubſtance conſiſts of oxygen, azote and hydrogen ſeverally combined with caloric, and ſeverally attached to the gold as a common baſe.

Twelve grains of this fulminating gold being placed in a conical heap on a thin plate of braſs, were gradually heated. At a temperature between 300 and 400°, the whole exploded, with a very ſharp and loud report, and the plate was pierced and torn. The round aperture was about an inch in diameter, and the lacerations extended much farther.

It was obſerved that the ſame effect might be produced by applying a ſpark to fulminating gold leſs heated, and that the accenſion of all ſuch fulminating compounds, by a ſpark applied to any part, or by due augmentation of the temperature of the whole, is eaſily explicable on the grounds mentioned in the minutes of the laſt meeting, provided due attention be paid to the ſtate of the active ingredients.

In fulminating gold, for inſtance, the attractive powers tending to produce the new combinations, which take place in the inſtant of combuſtion, ſeem to be almoſt equal to thoſe by which the aggregation of this compound is maintained at low temperatures. For a ſmall augmentation of temperature, or friction, or percuſſion, or any thing which diſturbs the arrangement of the gaſeous principles and caloric which adhere but weakly to the gold, is ſufficient for the exploſion of the whole, provided the fulminating gold be pure and dry.

In reſpect to the perforation of the braſs plate, it was obſerved that all bodies which explode inſtantaneouſly would impreſs it in a ſimilar manner. For the reſiſtance of the air to projectiles or to expanſions of this kind, being in the ratio of the ſquares of the velocities of the moving or expanding bodies, the reſiſtance of the air to ſuch inſtantaneous exploſions as thoſe lately mentioned, is almoſt equal to that of a ſolid, or of the metallic plate on which the fulminating body is heated.

SMALL portions of fulminating ſilver, each weighing leſs than a grain, were ſucceſſively exploded, ſome by the touch of a ſlender braſs wire, others by that of a feather.

---

* From the Minutes of a Society for Philoſophical Experiments and Communications: B. Higgins, M. D. Operator. I do not hear that the ſociety is continued ſince the departure of the able Operator for the Weſt-Indies. N.

A deſcription was given of many other ſpecimens which had been inadvertently exploded and loſt; ſome by overheating them to about 90°, in the place where they were to be dried; and others by an accidental concuſſion of a great iron plate, on which they were placed in ſeparate cups.

The experimenter ſaid, that he had often exploded fulminating ſilver, in covered veſſels of the ſmalleſt capacity that could be uſed with ſafety; and that this ſubſtance had frequently exploded unexpectedly in his hand, with a report louder than that of a muſket.

A luminous and momentary gleam was always viſible, but he could not diſcover any other adequate effect of the emitted caloric; and therefore he concluded, that in ſuch inſtantaneous exploſions the caloric was expelled with velocity ſufficient to conſtitute light.

Mr. Berthollet, the inventor of fulminating ſilver, having contented himſelf with a general and conciſe deſcription of this ſubject, many practical chemiſts have failed in their attempts to prepare it; and others, forming their opinions from the ſpecimens which they had made, have been expoſed to great danger; as will appear from the following relation, which is the only part of the Minutes on this ſubject that can be introduced in the preſent publication.

An ounce of fine ſilver was diſſolved in the courſe of eight hours in an ounce of pure nitrous acid, of the London Pharmacopœia, diluted previouſly with three ounces of diſtilled water, in a glaſs matraſs. The ſolution being poured off, the reſiduary black powder and the matraſs were waſhed with ſeven or eight ounces of warm diſtilled water, and this was added to the ſolution. The black powder being gold was rejected; ſome gold being thus ſeparable from any ſilver of commerce.

To the foregoing diluted ſolution, pure lime-water prepared with diſtilled water was added gradually; for the ſolution ought not to be poured into the lime-water. When about thirty pints of lime-water had been expended, and the precipitate had ſubſided, more lime-water was added, by ſucceſſive pints, as long as it cauſed any precipitation. For it was deemed fitter that the precipitation ſhould not be perfected, than that an exceſs of lime-water ſhould be uſed; the earthy pellicle of the exceſſive lime-water being apt to mix with the precipitate. The clear liquor being poured away, the precipitate was poured off, and waſhed into a filter.

When the ſaline liquor had drained from it, two ounces of diſtilled water were poured on the magma; and when this water had paſſed, freſh portions were ſucceſſively added and paſſed, until the whole quantity of water thus expended in waſhing away the nitrous calcareous ſalt amounted to a quart.

The filter being then unfolded, to let the magma of oxide of ſilver ſpread on the flattened paper, it was placed on a chalk-ſtone to accelerate the exſiccation, and was gradually dried in the open air; a cap of paper being placed looſely over it to exclude the duſt.

When the weather ſerved, the cap was removed, to expoſe the oxide to the rays of the ſun; although this was not deemed neceſſary; and the exſiccation was promoted by cutting the oxide into thin ſlices. When perfectly dry it weighed 1 oz. 4 dwts. and about one-fifth of it was conſidered as oxygene.

"When aqua ammoniæ puræ of any Pharmacopœia is uſed with this oxide, either in the ſmall quantity which blackens it completely, or in a greater quantity; the black matter which ſubſides, and which has been repreſented by ſyſtematic writers as the fulminating compound, has no ſuch property, any farther than may be owing to the matter depoſited from the alkaline ſolution during the exſiccation.

"The alkaline liquor containing the fulminating ſilver ought to be poured off from the inſoluble powder, and expoſed in a ſhallow veſſel to the air. In conſequence of the exhalation, black ſhining cryſtals form on the ſurface only, and ſoon join to form a pellicle. As this pellicle adheres a little to the ſides of the veſſel, or maintains its figure, the liquor may be poured off by a gentle inclination of the veſſel.

"This liquor will yield another pellicle in the ſame way; but the third or fourth pellicle will be paler than the former, and weaker in the exploſion. The firſt pellicles, when ſlowly dried, explode by the touch of a feather, or by their being heated to about 96°.

"The quantity of water in the ordinary aqua ammoniæ puræ renders it leſs active in the ſolution of the oxide, and is an impediment to the ſpeedy formation and ſeparation of the fulminating ſilver; and an experimenter who has often uſed twenty grains of the oxide to produce ſucceſſive pellicles of fulminating ſilver, which may be ſeparately exploded with ſafety, and who has perceived that the pellicles never explode whilſt wet, if they be not heated, would in all probability reſolve on the following improvement, and expoſe himſelf to the unforeſeen danger of it."

DISTILLED water was impregnated with as much pure ammoniac as it could eaſily retain under the ordinary temperature of the air. A quantity of this ſtrong ammoniacal liquor, equal in bulk to a quarter of an ounce of water, was placed in a ſmall bottle, and 24 grains of the oxide of ſilver, ground to fine powder, were added. The bottle, being almoſt filled, was corked, to prevent the formation of that film which uſually appeared in conſequence of the exhalation of the ammoniac in other experiments.

During the ſolution of the oxide, bubbles of the gaſeous kind aroſe from it, and the ſolution acquired a blue colour.

As no film appeared, the bottle was agitated three or four times in the courſe of as many hours, in order to promote the ſolution of a ſmall quantity of blackened oxide which remained at the bottom.

The experimenter conſidering this as an ample proviſion for twenty different charges, to be exploded in different circumſtances, in the preſence of the ſociety, intended to pour off the ſolution into as many ſmall veſſels, and to weigh the reſiduary black powder, after allowing two hours more for the ſolution.

On the ſixth hour he took his uſual precaution of wearing ſpectacles; and obſerving that a ſmall quantity of black powder ſtill remained undiſſolved, and that no film was yet formed at the ſurface, he took the bottle by the neck to ſhake it; knowing that it might explode by the heat of his hand, if he were to graſp it, and that the exploſion in this circumſtance might wound him dangerouſly.

In the inſtant of ſhaking, it exploded with a report that ſtunned him. The bottle was blown into fragments ſo ſmall as to appear like glaſs coarſely powdered. The hand which held it was impreſſed as by the blow of a great hammer, and loſt the ſenſe of feeling for ſome ſeconds; and about fifty-two ſmall grains of glaſs were lodged, many of them deeply, in the ſkin of the palm and fingers. The liquor ſtained his whole dreſs, and every part of the ſkin that it touched.

Thus it appeared that fulminating ſilver may be made which will explode even when cold and wet, by the mere diſturbance of the arrangement of its parts, in the aqueous fluid.

In ſubſequent experiments, privately and carefully conducted, it ſeemed that the property of exploding in the cold liquor, by mere commotion, depended on the unuſual quantity or proximity of the exploſive molecules in a given bulk of the liquor. And the flat bottoms, as well as the ſides, of the thick veſſels of glaſs or potters-ware, whether they ſtood on boards or on iron plates, were always beaten to ſmall fragments.

This afforded a curious inſtance of the poſſible equilibrium between the powers tending to retain the caloric, and thoſe which effect the expulſion of it; and experiments and conſiderations of this kind ſeemed to promiſe a true ſolution of the phenomena of Rupert's drops.

SMALL charges, each conſiſting of a grain of oxygenated muriate of pot-aſh, finely powdered, and mixed with an equal quantity of flowers of ſulphur, were exploded by mere trituration. And Mr. Godfrey's relation of the danger of keeping ſuch a mixture in a bottle was duly noticed; for, after he had kept it thus for ſome time, he found that it had exploded ſpontaneouſly.

III.

Experiments and Obſervations made with the View of aſcertaining the Nature of the Gaz produced by paſſing Electric Diſcharges through Water; with a Deſcription of the Apparatus for theſe Experiments. By George Pearson, M. D. F. R. S.

[Continued from page 248.]

SECTION II.

EXPERIMENTS.

FROM my Journal of the numerous experiments made during the courſe of nearly two years, I ſhall ſelect thoſe which will ſerve to explain the nature of the proceſs, and ſhew the power of the plate electrical machines; and I ſhall particularly relate thoſe experiments which afforded the moſt uſeful reſults concerning the nature of the gaz obtained.

1. *With interrupted Diſcharges.*

Experiment A. About 1600 of theſe diſcharges by means of a 34 inch ſingle plate electrical machine, in nearly three hours, produced, from New River water taken from the ciſtern, and which had not been freed from air by the air pump or boiling, a column of gaz, two-thirds of an inch in length, and 1-9th of an inch wide. On paſſing through this gaz, between the two wires of the tube in which it was produced, a ſingle electrical ſpark, its bulk was inſtantly diminiſhed to two-thirds. In other experiments the bulk of gaz was only diminiſhed to about one half. And the reſult was the ſame with diſtilled water.

B. The experiment A being repeated ſeveral times with diſtilled, and New River water, freed from air by the air pump or long boiling, the quantity of gaz juſt mentioned was obtained in about four hours.

On paſſing an electric ſpark through this gaz in the ſituation above mentioned, its bulk was inſtantly diminiſhed in ſome caſes 15-16ths, and in others 19-20ths.

C. 1600 interrupted diſcharges, by means of a 32 inch plate machine, produced from New River water, and diſtilled water, freed from their air by the air pump, a column of gaz, about 3-4ths of an inch in length and 1-9th of an inch in diameter, in the ſpace of three hours. It was reduced in bulk 19-20ths by paſſing through it a ſingle electrical ſpark.

D. 500 revolutions of the 32 inch plate machine, in three quarters of an hour, produced 600 interrupted diſcharges in river water freed from air by the air pump, by which a column of gaz, half an inch in length and 1-10th of an inch in diameter, was obtained. It was diminiſhed as uſual by an electric ſpark 19-20ths of its bulk.

E. Nearly four days inceſſant labour with the 32 inch machine produced only 56,5488 cubes of gaz, of 1-10th of an inch each, on account of the uſual accidents during the proceſs. The air had been exhauſted by ſetting the water under the receiver of the air pump.

F. It was found that 6000 interrupted diſcharges produced about three inches in length of gaz, meaſured in a tube 3-20ths of an inch in width from water out of which its air had been drawn by the air pump.

G. It appeared from many experiments, that the ſame unboiled water, or water from which the air had not been exhauſted by the air pump, which had repeatedly yielded gaz by paſſing through it electrical diſcharges, always left a reſidue of gaz which the electrical ſpark did not diminiſh; and this reſidue was in nearly the ſame quantity after ſix or ſeven experiments, each of which afforded a column of gaz half an inch in length and 1-9th of an inch in diameter, as was left on paſſing the electric ſpark through the gaz afforded by the third or fourth experiment.

Hence it ſeems that water is decompounded by the electric diſcharge, before the whole of the common or atmoſpherical air is detached from the water by merely the impulſe of each diſcharge. Yet I think it probable that, after the diſcharges have been paſſed through the ſame water for a certain time, the whole of the air contained in water will be expelled, and no gaz be produced, but that compounded by means of the electric fire from water; in which caſe, ſuppoſing the gaz ſo produced to be at leaſt merely hydrogen and oxygen gaz, it will totally diſappear on paſſing through it an electrical ſpark. But I have never been able to determine this point, becauſe the tubes were always broken after obtaining a few products, or long before it could reaſonably be ſuppoſed the whole of the air of the water was expelled from it.

H. To the gaz obtained in the experiment E was added, over water, an equal bulk of almoſt pure nitrous gaz. Fumes of nitrous acid appeared, and the gaz examined was reduced almoſt one-third of its bulk. A ſmall bubble more of nitrous gaz being let up, no further diminution took place. To this reſidue was added half its bulk of oxygen gaz, obtained from oxy-muriate of pot-aſh. This mixture of gazes having ſtood ſeveral days over well burnt lime and boiled quickſilver, an electric ſpark was paſſed through the mixture over quickſilver, by which its bulk was inſtantly diminiſhed one-fourth. But no moiſture could be perceived upon the ſides of the tube or on the quickſilver. The failure of the appearance of moiſture was imputed to a bit of lime accidentally left in the tube which was burſt by the exploſion, and diſperſed through the tube; or elſe the quantity of water produced was ſo ſmall, comparatively with the reſiduary gaz, that the water was diſſolved by it in the moment of its compoſition. For, ſuppoſing water to have been compounded, it could not amount to the 1-100th part of a grain, and the reſiduary gaz was at leaſt 2000 times this bulk.

That a quantity of water can be compounded under the ſame circumſtances as in this experiment, and be apparently diſſolved in air, ſo as to eſcape obſervation, even with a lens, was proved by paſſing an electric ſpark through a mixture of hydrogen and oxygen gaz, well dried by ſtanding over lime.

2. *With complete or uninterrupted Diſcharges.*

THE gaz obtained by the firſt deſcribed kind of apparatus for the uninterrupted diſcharges, p. 145, and Fig. 6, and 7, always left a reſidue of at leaſt one-fourth of its bulk, on paſſing through it the electric ſpark; even when water was uſed which had been freed from air by boiling or the air pump. Nor will this reſult appear ſurpriſing, when it is conſidered how liable the water in this apparatus is to mix and abſorb air during the experiment. However, this method would have been extremely valuable, if the next other method had not been diſcovered; for gaz may be obtained by it with fewer accidents, and much more rapidly, than with the interrupted diſcharges. The apparatus is alſo much more eaſily fitted up, and is more ſimple. But I think it unneceſſary to particularly relate any experiments, as they afforded the ſame reſults as thoſe already deſcribed, and as thoſe next to be related.

The following experiments were made with the apparatus deſcribed p. 146, and ſhewn by Fig. 8, 9, and 10:

Experiment I. At 0 h. 40′ P. M. began to produce diſcharges with a double plate twenty-four inch machine, in water taken from the ciſtern; and at 12 h. 6′ P. M. of the ſame day there had been written down 10200 diſcharges, each of which occaſioned air to aſcend from the bottom of the wire and braſs cup. The quantity of air obtained was now apparently about one-fourth of a cubical inch, and it occupied nearly half of the tube, the water in which was by this time very muddy.

After ſtanding till the day following at noon, when the proceſs was again commenced, it did not appear that any of the gaz had been abſorbed by the water over which it ſtood.

At 2 h. 35′ P. M. began to produce diſcharges, and at 8 h. P. M. had paſſed 6636; which, together with thoſe of the preceding day, amounted to 16,836. The tube was now 5-8ths full of gaz, and there ſeemed to be almoſt half a cubical inch; for it was obſerved that the gaz was this day yielded at double the rate it had been the day before. This was accounted for from the diminiſhed preſſure upon the electric fire, by the tube containing gaz inſtead of water.

At this time, namely at 8 h. P. M. I was ſurpriſed on the paſſing of a diſcharge by a vivid illumination of the whole tube, and a violent commotion within it, with, at the ſame time, the ruſhing up of water, inſtantly to occupy rather more than 5-8ths of the ſpace which had been occupied by gaz.

The reſidue of gaz was not diminiſhed further by an electric ſpark; and to the teſt of nitrous gaz it appeared to be rather worſe than atmoſpherical air, as it conſiſted of rather leſs than one part of oxygen, and three parts of nitrogen or azotic gaz.

It ſeemed as if the electrical diſcharge had kindled the oxygen and hydrogen gaz of the decompounded gaz, by flying from the bottom of the wire to the braſs funnel, ſo that the fire returned into the tube where it paſſed through the gaz. Or the combuſtion might be occaſioned by a chain of bubbles reaching from the braſs diſh to the ſurface of the water in the tube, which was ſet on fire in its aſcent, and thus produced combuſtion of the whole of the gaz of decompounded water.

That this phenomenon was from the combuſtion here ſuppoſed, was in ſome degree proved, by finding that the mixture of hydrogen gaz and atmoſpherical air, under the ſame circumſtances, was kindled in the ſame manner.

Experiment II. With a double plate electrical machine, 24 inches in diameter, and a ſimilar apparatus to that in the laſt experiment, 14,600 diſcharges produced at leaſt one-third of a cubical inch of gaz. While I was meaſuring with a pair of compaſſes the quantity of gaz produced, the points of them being in contact with the part of the tube occupied by gaz, I was again ſurpriſed, on the paſſing of a diſcharge, by an illumination of the whole tube, and the ruſhing up with conſiderable commotion of water, to occupy about two-thirds of the ſpace filled by gaz.

The reſiduary air was found, as in the former experiment, to be rather worſe than atmoſpherical air.

It was concluded that the points of the compaſſes had attracted electrical fire from the wire to the ſides of the glaſs, and thereby kindled the hydrogen and oxygen gaz of decompounded water. But to determine this queſtion, I introduced into the ſame tube a mixture of one meaſure of oxygen and two meaſures of hydrogen gaz, to occupy nearly the ſame ſpace in the tube as the gaz had occupied; then paſſing an electrical diſcharge through it, no combuſtion was excited; but on paſſing a diſcharge, while the compaſſes were in contact with the tube, as juſt mentioned, an illumination and violent commotion were produced, with the ruſhing up of water, to leave only 1-8th of the gaz as a reſidue. On repeating this experiment with two meaſures of atmoſpherical air, and one of hydrogen gaz, combuſtion could not be excited; nor with one meaſure of atmoſpherical air, and two of hydrogen; but on adding to this laſt mixture one meaſure of oxygen gaz, the electrical diſcharge produced the phenomena of combuſtion juſt mentioned, with the ruſhing up of water, to occupy about two-thirds of the ſpace which was occupied by the gazes.

Experiment III. Having paſſed 12,000 diſcharges through water, with the apparatus of the preceding experiment, and thereby obtained only one-fifth of a cubical inch of gaz; and having obſerved that the quantity of gaz was not greater than it was when only 8000 diſcharges had been paſſed, and yet bubbles had been ſeen to be produced on each diſcharge, as copiouſly, or more ſo, by the laſt three or 4000 diſcharges, as before; I began to ſuſpect that part of the gaz had been deſtroyed during the proceſs, or had been abſorbed. While I was conſidering how to account for this diſappearance of gaz, and was at the ſame time looking at the tube through which the diſcharges were paſſing, I obſerved one of them to be attended with a diminution, inſtantly, of about one-fifth of the gaz produced, and with a ſlight exploſion. I was now ſure, from this phenomenon, and from the unequal augmentation of the bulk of the gaz at given times during the proceſs, that combuſtion had been excited ſeveral times before, not only in the preſent experiment, but perhaps in the former ones, without obſerving it. I conceived that a gradual combuſtion alſo very probably took place in this proceſs by the kindling of bubbles of gaz in their aſcent through the water. I now perceived that the diſcharges ought to be produced more ſlowly, or the tubes to be wider, to allow the bubbles to paſs quite through the water, in order to avoid the aſcenſion of gaz during the proceſs. My calculation, alſo, that 35 to 40,000 diſcharges were requiſite to produce one cubical inch of gaz from water containing its uſual quantity of common air, was rendered much more vague by this aſcenſion, ſo often liable to be occaſioned.

To the gaz which remained in the tube in this experiment was added an equal bulk of nitrous gaz; the mixture diminiſhed to 1,5; and on adding to the reſidue half its bulk of oxygen gaz, and paſſing through it the electrical ſpark, no accenſion or diminution of bulk was produced. Hence all the hydrogen gaz and oxygen gaz, produced by the decompoſition of the water, had been burnt during the proceſs; the oxygen gaz thus detected being conſidered to be only that expelled from the water.

Experiment IV. By means of electrical diſcharges with the apparatus uſed in the preceding experiment, I obtained gaz from New-River water, letting it up into a reſervoir, as ſoon as about 1-20th of a cubic inch was produced, till I had collected 1-8th of a cubic inch. To this was added an equal bulk of nitrous gaz, on which the mixture diminiſhed to 1,2; and on the addition of a little more nitrous gaz, no further diminution took place. To this reſidue half its bulk of oxygen was added; and this mixture of gazes being well dried, by ſtanding over lime and boiled quickſilver, an electric ſpark was paſſed through it, by which a diminution of one-ſixth of its bulk took place. A little dew was then ſeen upon the ſides of the tube where the quickſilver had riſen; and with the aid of a lens the ſame appearance was perceived on the part of the tube containing the reſidue of gaz.

It may now be expected, that I ſhould have made the experiment with this apparatus on diſtilled water, freed from its air, not only by long boiling or the air pump, but by ſending through it ſeveral hundred electrical diſcharges. It would alſo have been, to ſome perſons, more ſatisfactory, if the experiment had been made upon a larger ſcale, ſo as to have produced the combuſtion of a much larger quantity of gaz, and conſequently have produced a greater quantity of water. As, however, I apprehend, the experiments contained in this paper, when well conſidered by competent judges, will be found to explain the nature of the gaz procured from water by electric diſcharges; and as another very important ſubject demands my attention, the honour of more ſplendid and convincing experiments muſt be reſerved for other enquirers. If the ſame ſacrifices be made by them, as have been made in performing the preſent experiments, I think it is ſcarcely poſſible but that ſtill further light concerning the compoſition of water ſhould be procured, as well as concerning oils, alcohol, acids, &c.; to the inveſtigation of the compoſition of which, the mode of analyſis and ſyntheſis here indicated may be applied.

SECTION III.

On the Mode of Action of Electric Diſcharges.

THE mere concuſſion by the electric diſcharges, appears to extricate not only the air diſſolved in water, which can be ſeparated from it by boiling and the air pump, but alſo that which remains in water, notwithſtanding theſe means of extricating it have been employed.

The quantity of this air varies in the ſame, and in different waters, according to circumſtances. New-River water from the ciſtern yielded one-fifth of its bulk of air, when placed by Mr. Cuthbertſon under the receiver of his moſt powerful air pump; but in the ſame ſituation, New-River water taken from a tub expoſed to the atmoſphere for ſome time yielded its own bulk of air. Hence the gaz procured by the firſt one, two, or even three hundred exploſions in water containing its natural quantity of air, is diminiſhed very little by an electric ſpark.

The gaz thus ſeparable from water, like atmoſpherical air, conſiſts of oxygen and nitrogen, or azotic gaz; which may be in exactly the ſame proportions as in atmoſpherical air; for the water may retain one kind of gaz more tenaciouſly than the other; and on this account the air ſeparated may be better or worſe than atmoſpherical air at different periods of the proceſs for extricating it.

With regard to the gaz which inſtantly diſappears on paſſing through it an electric ſpark, its nature is ſhewn by *(a)* this very property of thus diminiſhing; and by the following properties:

(b) A certain quantity of nitrous gaz inſtantly diſappeared, apparently compoſing nitrous acid, on being added to the gaz *(a)* p. 303, Exp. IV.

Oxygen gaz being added to the reſidue after ſaturation with nitrous gaz, and an electric ſpark being applied to the mixture of gazes, well dried, a conſiderable diminution immediately took place, and water was produced.

(c) Combuſtion from hydrogen and oxygen gaz took place when the tube was about three-fourths full of gaz, p. 301, Exp. I.; which was confirmed by paſſing an electric diſcharge, under the ſame circumſtances, through a mixture of hydrogen and oxygen gaz, p. 302, Exp. II.

(d) Combuſtion from hydrogen and oxygen gaz took place when the points of the compaſſes were accidentally applied to the part of the tube containing gaz, p. 302; which was confirmed by paſſing a diſcharge, under the ſame circumſtances, through a mixture of hydrogen and oxygen gaz, while the points of the compaſſes were applied to the tube.

(e) The obſervations made of the kindling of gaz, in ſmall quantities, from time to time, during the proceſs of obtaining it, particularly while it was aſcending in chains of bubbles, or was adhering to the funnel of the tube, p. 302, Exp. III. confirm the evidence in favour of this gaz being hydrogen and oxygen gaz.

The evidence contained under the above heads *(a)*—*(e)* conſidered ſingly and conjunctively, I apprehend, muſt be admitted by the moſt rigorous reaſoner, or ſevereſt logician, to be demonſtrative that hydrogen and oxygen gaz were produced by paſſing electric diſcharges through water.

With regard to the origin and mode of production of theſe two gazes, our preſent obſervations and experiments do *not* afford complete demonſtrative evidence; but although ſome hypotheſes muſt be admitted, I conceive that the body of evidence we poſſeſs can afford a ſatisfactory interpretation of the phenomena.

It is demonſtrable that the electric diſcharge and ſpark contain fire; and very probably they are merely a ſtate of fire. Fire may be conſidered as conſiſting of caloric and light; but it is at leaſt as conſiſtent with the phenomena, and it is more philoſophical, becauſe it is more ſimple, to conſider light not as a diſtinct ſpecies of matter, but as a ſtate of caloric, which is manifeſted by its producing the ſenſation termed viſion. It is demonſtrable alſo, that the ponderable parts of oxygen and hydrogen gaz conſtitute water. There is ſtrong evidence that theſe gazes conſiſt of a peculiar ſpecies of matter which is ponderable; and of imponderable matter, which is that which is ſeparable from them in the ſtate of fire, or flame. If fire could be applied in a ſufficiently denſe ſtate and quantity, it is warrantable, from a full induction of facts, to conclude, that it is able to diſunite the conſtituent ſubſtances of all the compound ſubſtances in nature.

[*To be concluded in the next Number.*]

IV.

Experimental Reſearches to aſcertain the Nature of the Proceſs by which the Eye adapts itſelf to produce diſtinct Viſion.

THE ſtructure of the eye, and particularly its proviſions for adjuſtment to produce diſtinct viſion, have engaged the attention of ſeveral philoſophers during the laſt five years, who have delivered papers on theſe ſubjects to the Royal Society. I purpoſe to give the ſubſtance of their diſcoveries in the preſent communication.

In the year 1793, Mr. Thomas Young's Obſervations on Viſion appeared in the Tranſactions. He gives a ſhort ſummary of the theories of adjuſtment propoſed by various earlier authors. Kepler ſuppoſed the ciliary proceſſes to contract the diameter of the eye, and lengthen its axis by a muſcular power. Deſcartes imagined the ſame effect to be produced by a muſcularity of the cryſtalline humour, but did not attribute much to the change of figure which he ſuppoſed to take place in that lens. De la Hire, and alſo Haller, adopted the opinion that the eye undergoes no change to produce diſtinct viſion, but the contraction and dilatation of the pupil. Pemberton ſuppoſed the exiſtence of muſcular fibres in the cryſtalline humour, by which the curvatures of its ſurfaces are changeable. Dr. Porterfield conceived that the ciliary proceſſes draw forward the cryſtalline, and render the cornea more convex. Dr. Jurin maintained the hypotheſis, that the uvea, at its attachment to the cornea, is muſcular, and capable of increaſing the convexity of that humour, by its contraction towards the axis of the eye. Muſchenbroek conjectures that the relaxation of his ciliary zone, which appears to be nothing but the capſule of the vitreous humour, permits the coats of the eye to puſh forward the cryſtalline and cornea. And, laſtly, an elongation of the axis of the eye has been ſuppoſed to be produced by the preſſure of the external muſcles, eſpecially the two oblique muſcles; and, on the other hand, the muſcular action has been ſuppoſed to produce a contrary effect, namely, a contraction of the axis.

In the enumeration of theſe reſpective hypotheſes, Mr. Young makes remarks tending to their ſeveral refutation. I have not tranſcribed them, principally becauſe they chiefly tend to ſhew that effects of this nature deſerve, if poſſible, to be ſubmitted to actual experiment.

From the conſideration of the whole ſubject, our author concluded that the rays of light emitted by objects at a ſmall diſtance could only be brought to foci on the retina by a nearer approach of the cryſtalline to a ſpherical form; and he could imagine no other power capable of producing this change than a muſcularity of part or the whole of its capſule.

But on cloſely examining, with the naked eye, the cryſtalline from an ox turned out of its capſule, he diſcovered a ſtructure which he thinks ſufficient to remove all the difficulties with which this branch of optics has long been obſcured. The cryſtalline lens of the ox is an orbicular, convex, tranſparent body, compoſed of a conſiderable number of ſimilar coats, of which the exterior cloſely adhere to the interior. Each of theſe coats conſiſts of ſix muſcles, intermixed with a gelatinous ſubſtance, and attached to ſix membranous tendons. Three of the tendons are anterior, three poſterior; their length is about two-thirds of the ſemi-diameter of the coat; their arrangement is that of three equal and equidiſtant rays, meeting in the axis of the cryſtalline; one of the anterior is directed towards the outer angle of the eye, and one of the poſterior towards the inner angle; ſo that the poſterior are placed oppoſite to the middle of the interſtices of the anterior; and planes paſſing through each of the ſix, and through the axis, would mark on either ſurface ſix regular equidiſtant rays. The muſcular fibres ariſe from both ſides of each tendon; they diverge till they reach the greateſt circumference of the coat; and having paſſed it, they again converge till they are attached reſpectively to the ſides of the neareſt tendons of the oppoſite ſurface. The anterior or poſterior portion of the ſix viewed together exhibits the appearance of three penniform-radiated muſcles. The anterior tendons of all the coats are ſituated in the ſame planes, and the poſterior ones in the continuations of theſe planes beyond the axis. Such an arrangement of fibres can be accounted for on no other ſuppoſition than that of muſcularity. This maſs is encloſed in a ſtrong membranous capſule, to which it is looſely connected by minute veſſels and nerves; and the connection is more obſervable near its greateſt circumference. Between the maſs and its capſule is found a conſiderable quantity of an aqueous fluid, the liquid of the cryſtalline.

He conceives, therefore, that when the will is exerted to view an object at a ſmall diſtance, the influence of the mind is conveyed through the lenticular ganglion formed from branches of the third and fifth pairs of nerves, by the filaments perforating the ſclerotica to the orbiculus ciliaris, which may be conſidered as an annular plexus of nerves and veſſels; and thence, by the ciliary proceſſes, to the muſcle of the cryſtalline; which, by the contraction of its fibres, becomes more convex, and collects the diverging rays to a focus on the retina. The diſpoſition of fibres in each coat is admirably adapted to produce this change; for ſince the leaſt ſurface that can contain a given bulk, is that of a ſphere (Simpſon's Fluxions, p. 486), the contraction of any ſurface muſt bring its contents nearer to a ſpherical form. The liquid of the cryſtalline ſeems to ſerve as a ſynovia in facilitating the motion, and to admit a ſufficient change of the muſcular part, with a ſmaller motion of the capſule.

To aſcertain whether theſe fibres can produce an alteration in the form of the lens ſufficiently great to account for the known effects, this author ſtates, that the diameter of the cryſtalline of the ox is 700 thouſandths of an inch, the axis of its anterior ſegment 225; of its poſterior 350. In the atmoſphere it collects parallel rays, at the diſtance of 235 thouſandths. From theſe data he finds, by means of Smith's Optics, article 366, and a quadratic, that its ratio of refraction is as 10000 to 6574. Haukſbee makes it only as 10000 to 6832,7; but we cannot depend on his experiment, ſince he ſays, that the image of the candle which he viewed was enlarged and diſtorted; a circumſtance that he does not explain, but which was evidently occaſioned by the greater denſity of the central parts. Suppoſing, with Haukſbee and others, the refraction of the aqueous and vitreous humours equal to that of water, viz. as 10000 to 7465, the ratio of refraction of the cryſtalline in the eye will be as 10000 to 8806, and it would collect parallel rays at the diſtance of 1226 thouſandths of an inch; but the diſtance of the retina from the cryſtalline is 550 thouſandths; and that of the anterior ſurface of the cornea 250; hence (by Smith, art 367.) the focal diſtance of the cornea and aqueous humour alone muſt be 2329. Now ſuppoſing the cryſtalline to aſſume a ſpherical form, its diameter will be 642 thouſandths, and its focal diſtance in the eye 926. Then, diſregarding the thickneſs of the cornea, he deduces (by Smith, art. 370.) that ſuch an eye will collect thoſe rays on the retina which diverge from a point at the diſtance of 12 inches and 8-10ths. This is a greater change than is neceſſary for an ox's eye: for, if it be ſuppoſed capable of diſtinct viſion at a diſtance ſomewhat leſs than 12 inches, yet it probably is far ſhort of being able to collect parallel rays. The human cryſtalline, he ſays, is ſuſceptible of a much greater change of form.

The ciliary zone may admit of as much extenſion as this diminution of the diameter of the cryſtalline will require; and its elaſticity will aſſiſt the cellular texture of the vitreous humour, and perhaps the gelatinous part of the cryſtalline, in reſtoring the indolent form.

He queſtions whether the retina takes any part in ſupplying the lens with nerves; but from the analogy of the olfactory and auditory nerves, he thinks it more reaſonable to ſuppoſe that the optic nerve ſerves no other purpoſe than that of conveying ſenſation to the brain.

Although a ſtrong light and cloſe examination are required in order to ſee the fibres of the cryſtalline in its entire ſtate, yet their direction was demonſtrated, and their attachment ſhewn, without much difficulty. In a dead eye, the tendons are diſcernible through the capſule, and ſometimes the anterior ones even through the cornea and aqueous humour. When the dry cryſtalline falls, it very frequently ſeparates, as far as the centre, into three portions, each having a tendon in its middle. If it be carefully ſtripped of its capſule, and the ſmart blaſt of a fine blow-pipe be applied cloſe to its ſurface, in different parts, it will be found to crack exactly in the direction of the fibres above deſcribed, and all theſe cracks will be ſtopped as ſoon as they reach either of the tendons. The application of a little ink to the cryſtalline is of great uſe in ſhewing the courſe of the fibres.

Mr. Young was not at firſt aware that the muſcularity of the cryſtalline had ever been ſuſpected, either by Deſcartes or any other perſon. But the laborious and accurate Lewenhoek, to whoſe writings he refers, has deſcribed the courſe of the fibres of the cryſtalline in a variety of animals, and has even gone ſo far as to call it a muſcle. He did not, however, attempt to account for the focal adjuſtment of the eye from its muſcularity.

The remaining part of Mr. Young's paper is employed in the ſolution of ſome optical queries, not immediately relating to the preſent object.

In Plate XIII, Fig. 1. repreſents a vertical ſection of the ox's eye, of the natural ſize. A, the cornea covered by the tunica conjunctiva; BCB, the ſclerotica, covered at BB by the tunica albuginea and tunica conjunctiva; DD, the choroid, conſiſting of two laminas; EE, the circle of adherence of the choroid and ſclerotica; FG, FG, the orbiculus ciliaris;

HI, HK, the uvea; its anterior ſurface the iris; its poſterior ſurface lined with pigmentum nigrum; IK, the pupil; HL, HL, the ciliary proceſſes covered with pigmentum nigrum; MM, the retina; N, the aqueous humour; O, the cryſtalline lens; P, the vitreous humour; QR, QR, the zona ciliaris; RS, RS, the annulus mucoſus.

Fig. 2. The ſtructure of the cryſtalline lens, as viewed in front, likewiſe of the natural ſize;—Fig. 3. a ſide view of the cryſtalline.

In the following year, 1794, a communication was made to the Royal Society, by Everard Home, Eſq. F. R. S. of ſome facts relative to a preparation for the Croonian Lecture, by the late Mr. John Hunter. He ſtates that this celebrated anatomiſt had for many years entertained the notion, that the cryſtalline humour was enabled by its own internal actions to adjuſt itſelf ſo as to adapt the eye to different diſtances; and when the tænia hydatigena firſt came under his obſervation as a living animal, he was ſurpriſed to ſee the quantity of contraction that took place in a membrane devoid of muſcular fibres; but made uſe of the fact in his inveſtigation of the ſtructure of the cryſtalline humour of the eye. Some time after this, he diſcovered the fibrous ſtructure of the cryſtalline humour in the eye of the cuttle-fiſh, in which it is peculiarly diſtinct; and thence he was led to conſider the exterior part of this humour as ſimilar in all animals. It was his intention to have aſcertained by experiment, whether muſcular action does in fact take place; and having found that a certain degree of heat applied through the medium of water will excite muſcular action, after almoſt every other ſtimulus had failed, it was propoſed to apply this to the cryſtalline humour, and aſcertain its effects. The cryſtalline humour taken from animals recently killed muſt be conſidered as being ſtill alive. Such humours were to be immerſed in water of different temperatures, and placed in ſuch a manner as to form the image of a lucid, well-defined object, by a proper apparatus for that purpoſe, ſo that any change of the place of that image, from the ſtimulating effects of the warm water upon the humour, would be readily aſcertained. Theſe were the experiments which Mr. Hunter had inſtituted and begun, but in which he had not made ſufficient progreſs before his death to enable him to draw any concluſions.

This communication contains an unfiniſhed letter from Mr. Hunter to Sir Joſeph Banks, containing ſome prefatory obſervations leading to the ſtatement made by Mr. Home. It appears, in fact, as that gentleman remarks, that the diſcovery of a fibrous appearance in the cryſtalline appertains to Lewenhoek; but that the diſcovery of an eye in which the ſtructure is uncommonly diſtinct, is due to Mr. Hunter.

To this paper is annexed a plate exhibiting two ſections of the cryſtalline humour of the cuttle-fiſh.

In the ſecond part of the Tranſactions for the ſame year, I find a paper of ſome length on viſion, by Dr. Hoſſack. This author ſhews in the firſt place, that the enlargement or contraction of the pupil of the eye is inſufficient to produce the adjuſtment by which the rays of light ſhall converge to a point in the retina; and in fact a ſlight obſervation of what happens is ſufficient to ſhew that the variations of the pupil are governed by different circumſtances. He controverts Mr. Young's deductions with regard to the muſcularity of the cryſtalline, and even diſputes the facts. He quotes ſome authorities to ſhew that the eye is capable of accommodating itſelf to different diſtances without the aſſiſtance of the cryſtalline, as after couching or extraction. For theſe and other reaſons his attention was more particularly directed to the external muſcles.

Upon carefully removing the eyelids, the muſcles of the eye preſent themſelves to view in number ſix; four called recti or ſtraight, and two oblique, ſo named from their direction. In Plate XIII. Fig. 4, AAAA repreſent the tendons of the recti muſcles, where they are inſerted into the ſclerotic coat, at the anterior part of the eye. B, the ſuperior oblique, or trochlearis, as ſometimes called, from its paſſing through the loop or pulley connected to the lower angle of the orbit or notch in the os frontis; it paſſes under the ſuperior rectus muſcle, and backwards to the poſterior part of the eye, where it is inſerted by a broad flat tendon into the ſclerotic coat. C, the inferior oblique, ariſing tendinous from the edge of the orbit or proceſs of the ſuperior maxillary bone, paſſing ſtrong and fleſhy over the inferior rectus, and backwards under the abductor to the poſterior part of the eye, where it is alſo inſerted by a broad flat tendon into the ſclerotic coat. DDD, the fat in which the eye is lodged. In Figure 5, the bones forming the external ſide of the orbit, with a portion of the fat, are removed, by which we have a diſtinct view of the abductor. ABC, three of the recti muſcles, ariſing from the back part of the orbit, paſſing ſtrong, broad and fleſhy over the ball of the eye, and inſerted by flat broad tendons into the ſclerotic coat, at its interior part. D, the tendon of the ſuperior oblique muſcle. E, the inferior oblique. In Fig. 6, A repreſents the abductor of the eye. B, the fleſhy belly of the ſuperior oblique, ariſing ſtrong, tendinous and fleſhy from the back part of the orbit. C, the optic nerve. D and E, the recti muſcles.

The uſe aſcribed to theſe different muſcles is that of directing the axis of the eye towards the different objects, or to expreſs the paſſions of the mind. But Dr. Hoſack rationally infers, from the general application of the combined forces of muſcles through the whole of the animal ſyſtem, that this ſet of muſcles, which he conceives to be well adapted to produce the focal adjuſtment, may alſo be employed on that object. He aſſumes, as the neceſſary conſequence of contraction in theſe muſcles, that the axis of the eye will be elongated, and the elaſtic cornea rendered more convex; both which circumſtances would tend to preſerve diſtinctneſs of viſion with regard to near objects. For as ſuch objects afford a focal image more diſtant from the refracting ſurface, through which the light may paſs, the elongation of the axis will be of advantage, by removing the retina further back; and the increaſed convexity and ſhortened focus of the cornea will conduce to the ſame end.

How far the action of the recti muſcles might produce an elongated figure, is perhaps capable of diſpute; though this effect will probably be admitted without heſitation, as a conſequence of the contraction of the oblique muſcles. But to put the matter out of doubt, whether this organ be capable of having its focal adjuſtment conſiderably varied by external preſſure, our author applied the common ſpeculum oculi to his own eye. With a very moderate preſſure, while directing his attention to an object at the diſtance of about twenty yards, he ſaw it diſtinctly, as alſo the different intermediate objects; but endeavouring to look beyond it, every thing appeared confuſed: he then increaſed the preſſure conſiderably; in conſequence of which he was enabled to ſee objects diſtinctly, though placed much nearer than the natural focal diſtance. For example: he held before his eye, at the diſtance of about two inches, a printed book. In the natural ſtate of the eye, he could neither diſtinguiſh the lines nor letters; but, upon making preſſure with the ſpeculum, he was enabled to diſtinguiſh both the lines and the letters of the book with eaſe.

Such being this author's conception of the action and effect of the external muſcles, he has proceeded to apply the doctrines in explanation of the changes the eye is known to undergo at different periods of life or habits of occupation, from their ſtrength in early life, their debility in old age, and their habitual action from uſe. Theſe and other general facts are accounted for with conſiderable addreſs.

Mr. Home, the brother-in-law of Hunter, and vindicator of his poſthumous fame, was appointed to read the Croonian lecture on muſcular motion, for the ſeſſion of the Royal Society beginning in the year 1794. His lecture is peculiarly valuable for the experimental reſults it exhibits. In proſecuting the enquiry projected by Mr. Hunter, he had the great advantage of the aſſiſtance of his friend Mr. Ramſden, who, in converſing upon the different uſes of the cryſtalline humour, made the following obſervations:

He ſaid that, as the cryſtalline humour conſiſts of a ſubſtance of different denſities, the central parts being the moſt compact, and from thence diminiſhing in denſity gradually in every direction, approaching the vitreous humour on one ſide, and the aqueous humour on the other, its refractive power becomes nearly the ſame with that of the two contiguous ſubſtances. That ſome philoſophers have ſtated the uſe of the cryſtalline humour to be, for accommodating the eye to ſee objects at different diſtances; but the firmneſs of the central part, and the very ſmall difference between its refractive power near the circumference, and that of the vitreous or the aqueous humour, ſeemed to render it unfit for that purpoſe; its principal uſe rather appearing to be for correcting the aberration ariſing from the ſpherical figure of the cornea, where the principal part of the refraction takes place, producing the ſame effect that, in an achromatic object-glaſs, we obtain in a leſs perfect manner by proportioning the radii of curvature of the different lenſes. In the eye the correction ſeems perfect, which in the object-glaſs can only be an approximation; the contrary aberrations of the lenſes not having the ſame ratio; ſo that, if this aberration be perfectly corrected, at any given diſtance from the centre, in every other it muſt be in ſome degree imperfect.

Purſuing the ſame compariſon: In the achromatic object-glaſs we may conceive how much an object muſt appear fainter from the great quantity of light loſt by reflection at the ſurfaces of the different lenſes, there being as many primary reflections as there are ſurfaces; and it would be fortunate if this reflected light was totally loſt. Part of it is again reflected towards the eye by the interior ſurfaces of the lenſes; which, by diluting the image formed in the focus of the object-glaſs, makes that image appear far leſs bright than it would otherwiſe have done, producing that milky appearance ſo often complained of in viewing lucid objects through this ſort of teleſcope.

In the eye, the ſame properties that obviate this defect ſerve alſo to correct the errors from the ſpherical figure, by a regular diminution of denſity, from the centre of the cryſtalline outward. Every appearance ſhews the cryſtalline to conſiſt of laminæ of different denſities; and if we examine the junction of different media, having a very ſmall difference of refraction, we ſhall find that we may have a ſenſible refraction without reflection. Now, if the difference between the contiguous media in the eye, or the laminæ in the cryſtalline, be very ſmall, we ſhall have refraction without having reflection: and this appears to be the ſtate of the eye; for although we have two ſurfaces of the aqueous, two of the cryſtalline, and two of the vitreous humour, yet we have only one reflected image; and that being from the anterior ſurface of the cornea, there can be no ſurface to reflect it back, and dilute an image on the retina.

This hypotheſis may be put to the teſt whenever accident ſhall furniſh us with a ſubject having the cryſtalline extracted from one eye, the other remaining perfect in its natural ſtate; at the ſame time we may aſcertain whether or no the cryſtalline is that part of the organ which ſerves for viewing objects at different diſtances diſtinctly. Seeing no reflection at the ſurface of the cryſtalline, might lead ſome perſons to infer that its refractive power is very inconſiderable; but many circumſtances ſhew the contrary: yet what it really is may be readily aſcertained by having the focal length and diſtance of a lens from the operated eye, that enables it to ſee objects the moſt diſtinctly; alſo the focal length of a lens, and its diſtance from the perfect eye, that enables it to ſee objects at the ſame diſtance as the imperfect eye: theſe data will be ſufficient whereby to calculate the refractive power of the cryſtalline with conſiderable preciſion.

Again, having the ſpherical aberration of the different humours of the eye, and having aſcertained the refractive power of the cryſtalline, we have data from whence to determine the proportional increaſe of its denſity as it approaches the central part, on a ſuppoſition that this property corrects the aberration.

An opportunity preſented itſelf for bringing the obſervations of Mr. Ramſden reſpecting the uſe of the cryſtalline lens, to the proof. A young man came into St. George's Hoſpital, with a cataract in the right eye. The cryſtalline lens was readily extracted, and the union of the wound in the cornea took place unattended by inflammation, ſo that the eye ſuffered the ſmalleſt degree of injury that can attend ſo ſevere an operation. The man himſelf was in health, 21 years of age, intelligent, and his left eye perfect: the other had been an uncommonly ſhort time in a diſeaſed ſtate, and 27 days after the operation appeared to be free from every other defect but the loſs of the cryſtalline lens.

A number of experiments were made on the imperfect eye, aſſiſted by a lens, and compared with the perfect eye. The aim of theſe trials, which were judiciouſly varied, was to aſcertain whether the eye which had been deprived of the cryſtalline lens was capable of adjuſting itſelf to diſtinct viſion at different diſtances. Among other reſults, the perfect eye, with a glaſs of $6\frac{1}{2}$ inches focus, had diſtinct viſion at three inches; the near limit was $1\frac{7}{8}$ inch, the diſtant limit leſs than 7 inches. The imperfect eye, with a glaſs $2\frac{2}{10}$ths inches focus, with an aperture $\frac{3}{40}$ths of an inch, had diſtinct viſion at $2\frac{7}{8}$ inches, the near limit $1\frac{7}{8}$ inch, and the diſtant limit 7 inches. The accuracy with which the eye was brought to the ſame point, on repeating the experiments, proved it to be uncommonly correct; and as he did not himſelf ſee the ſcale uſed for admeaſurement, there could be no ſource of fallacy. From the reſult of this experiment it appears that the range of adjuſtment of the imperfect eye, when the two eyes were made to ſee at nearly the ſame focal diſtance, exceeded that of the perfect eye. Mr. Ramſden ſuggeſted a reaſon why the point of diſtinct viſion of the imperfect eye might appear to the man himſelf nearer than it was in reality; namely, that from the imperfection of this organ, he might find it eaſier to read the letters when they ſubtended a greater angle than at his real point of diſtinct viſion. The experiments, however, appear to ſhew that the internal power of the eye, by which it is adjuſted to ſee at different diſtances, does not reſide in the cryſtalline lens; at leaſt not altogether; and that if any agency in this reſpect can be proved to reſide in the cryſtalline, the other powers, whatever they may be, are capable of exertion beyond their uſual limits, ſo as to perform its office in this reſpect.

From theſe conſiderations, and in conſequence of other reflections tending to ſhew that an elongation of the optical axis is not probably the means of adjuſtment, theſe philoſophers directed their enquiries to aſcertain how far the curvature of the cornea might be ſubject to change. They found by trial that this part of the organ poſſeſſes a degree of elaſticity which is very conſiderable, both for its perfection and its range; and by anatomical diſſection it was found, that the four ſtraight muſcles of the eye do in effect terminate in the cornea at their tendinous extremities; that the whole external lamina of the cornea could by gentle force be ſeparated, by means of theſe muſcles, from the eye; ſo that the tendons ſeem loſt in the cornea, and this laſt has the appearance of a central tendon. It was alſo ſeen that the central part of the cornea is the thickeſt and the moſt elaſtic.

Theſe were conſiderable advances towards eſtabliſhing the hypotheſis of adjuſtment by the external curve of the eye. It remained to be ſhewn, by experiments on the living ſubject, that this curve does really vary in the due direction, when the mind perceives the diſtinct viſible ſenſation of objects at different diſtances. For this purpoſe Mr. Ramſden provided an apparatus, conſiſting of a thick board ſteadily fixed, in which was a ſquare hole large enough to admit a perſon's face; the forehead and chin reſting againſt the upper and lower bars, and the cheek againſt either of the ſides; ſo that when the face was protruded, the head was ſteadily fixed by reſting on three ſides; and in this poſition the left eye projected beyond the outer ſurface of the board. A microſcope, properly mounted, ſo as with eaſe to be ſet in every requiſite poſition, was applied to view the cornea with a magnifying power of thirty times. In this ſituation, the perſon whoſe eye was the object of experiment was deſired to look at the corner of a chimney, at the diſtance of 235 yards, through a ſmall hole in a braſs plate, fixed for that purpoſe, and afterwards to look at the edge of the hole itſelf, which was only ſix inches diſtant. After ſome management and caution, which the delicate nature of theſe experiments requires, the motion of the cornea, which was immediately perceptible, became very diſtinct and certain. The circular ſection of its ſurface remained in a line with the wire in the field of the microſcope, when the eye was adjuſted to the diſtant object, but projected conſiderably beyond it when adapted to the near one. When the diſtant object was only 90 feet from the obſerver, and the near object ſix inches, the difference in the prominence of the cornea was eſtimated at 1-800th of an inch. Theſe experiments were repeated and varied at different times and on different ſubjects. The obſerver at the microſcope found no difficulty in determining, from the appearance of the cornea, whether the eye was fixed on the remote or the near object.

From theſe different experiments Mr. Home conſiders the following facts to have been aſcertained:

1. That the eye has a power of adjuſting itſelf to different diſtances when deprived of the cryſtalline lens; and therefore the fibrous and laminated ſtructure of that lens is not intended to alter its form, but to prevent reflections in the paſſage of the rays through the ſurfaces of media of different denſities, and to correct ſpherical aberration.

2. That the cornea is made up of laminæ; that it is elaſtic, and when ſtretched is capable

of being elongated 1-11th part of its diameter, contracting to its former length immediately upon being left to itſelf.

3. That the tendons of the four ſtraight muſcles of the eye are continued on to the edge of the cornea, and terminate, or are inſerted, in its external laminæ: their action will therefore extend to the edge of the cornea.

4. That in changing the focus of the eye from ſeeing with parallel rays to a near diſtance, there is a viſible alteration produced in the figure of the cornea, rendering it more convex; and when the eye is again adapted to parallel rays, the alteration by which the cornea is brought back to its former ſtate is equally viſible.

The remaining part of Mr. Home's lecture contains ſome obſervations upon the muſcular and elaſtic powers, which by their oppoſition produce ſo curious an effect in the adjuſtment of the eye. The ſeparate action of the muſcles produces a change in the direction of the axis of the eye. A ſmall force of contraction in the whole ſyſtem will ſteady the eye, and a greater force will compreſs the lateral and poſterior parts of the eye, and render the cornea more convex. The experiments prove that the eye-ball cannot recede in its orbit by theſe actions. Other inſtances in the animal œconomy, by means of which the expenditure of muſcular action is ſaved by the operation of elaſtic antagoniſts, are alſo ſtated by this author. The exertion required to adjuſt the eye to near diſtances, and the eaſe with which it was adapted to remote objects, prove that the firſt was a poſitive action, and the ſecond a relief. The defect of elaſticity inferred to ariſe from age is happily applied to explain the changes of viſion which take place in advanced life.

Two other communications to the Royal Society by this philoſopher, and one by Mr. Smith on the eyes of birds, remain to be conſidered; but, on account of the length of this communication, I ſhall for the preſent defer them.

V.

Concerning the Properties of the Sulphureous Acid, and its Combinations with earthy and alkaline Baſes. By Citizens FOURCROY *and* VAUQUELIN *.

SEVERAL philoſophers have paid attention to the properties of the ſulphureous acid, and ſome of its combinations; but no one has given a complete account of this acid.

Berthollet is almoſt the only chemiſt who has opened this inveſtigation. He is the firſt who publiſhed any accurate account of the ſubject †.

We ſhall not in this place deſcribe either the apparatus or method of preparing this acid, becauſe both are well-known to ſuch as are moderately acquainted with chemiſtry.

I. Phyſical Properties of the Sulphureous Acid.

THIS acid conſtitutes a permanent elaſtic fluid at the ordinary preſſure and temperature of our atmoſphere. Its odour is ſtrong and ſuffocating. It cannot maintain combuſtion, nor the reſpiration of animals.

Bergman, in his Treatiſe on Elective Attraction, affirms that the ſulphureous acid, prepared in the pneumatic apparatus over mercury, cannot be reduced to the liquid ſtate by any known means. Monge and Clouet affirm, on the contrary, that by the application of extreme cold, and a ſtrong preſſure exerted at the ſame time on this gas, they rendered it liquid.

---

* Journal de l'Ecole Polytechnique, cahier IV. p. 445.

† Annales de Chimie, II. 54.

Its ſpecific gravity, according to Bergman, is 0,00246, and 0,00251, according to Lavoiſier; which correſponds with 1.508 grain the inch cube, and 4 oz. 5 gros the foot cube (French weights and meaſures).

II. *Chemical Properties of the Sulphureous Acid.*

Action of Caloric.] Prieſtley and Berthollet affirm, that the ſulphureous acid gas, expoſed to an elevated temperature, depoſits a portion of ſulphur, and becomes converted into ſulphuric acid. Bergman relates the ſame fact. This experiment, repeated in ſeveral different ways, did not afford us the ſame reſult.

III. *Action of Oxygene Gas.*

ONE part of dry oxygene gas, and two parts of ſulphureous acid gas, prepared in the mercurial apparatus, and mixed together, ſuffered no remarkable change during the courſe of ſeveral months. If a ſmall quantity of water be added to the mixture, a ſucceſſive abſorption, after the diminution which is produced by the combination of a portion of the ſulphureous acid, is perceived; which proves the exiſtence of a true combination between theſe two bodies. In fact, when the mixture is waſhed, after the expiration of ſeveral months, the reſidue of oxygene is found to be leſs than the original quantity.

The attraction of the water for the ſulphuric acid is therefore favourable to the union of the oxygene with the ſulphureous acid. We were not able, however, to convert the whole of the ſulphureous acid into ſulphuric acid, though in all our experiments oxygene gas remained in the apparatus.

The ſulphureous acid is immediately converted into ſulphuric acid, by paſſing a mixture of this gas and of oxygene gas through an ignited tube. A very denſe white fume is formed, which becomes condenſed into the liquid form, in a bottle placed for that purpoſe at the other extremity of the tube.

It ſeveral times happened, when by accident we had uſed the exact proportions, that the two gaſes were entirely deſtroyed, and not an atom of (elaſtic) reſidue was perceived.

IV. *The Action of Water.*

WHEN the ſulphureous acid gas was paſſed into water cooled by means of ice, the combination was made with ſuch rapidity that not a ſingle bubble roſe to the ſurface of the liquor until it was ſaturated. The ice is ſpeedily melted; which proves that much caloric is diſengaged. The water acquires at this temperature the 0,15th part of its weight. Its ſpecific gravity, compared with that of pure water, is as 1020 to 1000.

Water thus ſaturated with the ſulphureous acid, and expoſed to the temperature of 15 degrees above zero (Reaumur), immediately becomes filled with an infinity of ſmall bubbles, which ſucceſſively increaſe, and riſe to the ſurface. If a bottle full of this acid be plunged in hot water, it boils with aſtoniſhing rapidity, and loſes by this operation part of its odour and acidity. It cannot, however, be eaſily deprived of theſe qualities, even by boiling. This acid freezes at a few degrees below zero, or the freezing point of water; and what may appear aſtoniſhing is, that not the ſmalleſt portion of gas is diſengaged, as happens when the other gaſes are diſſolved in water.

Theſe experiments prove, 1. That the colder the water the more it abſorbs of ſulphureous acid. 2. That the combination of theſe bodies is accompanied with heat. 3. That ſuch a combination cannot preſerve at a more elevated temperature all the acid it was charged with at a lower degree of heat. 4. That this combination paſſes to the ſolid ſtate by cooling, without undergoing decompoſition; a fact which ſhews a great affinity of water for the ſulphureous acid, and a weak attraction of this laſt for caloric.

V. Action of Acids upon the Sulphureous Acid.

Action of the Sulphuric Acid.] We cauſed ſulphureous acid gas to paſs through the ſulphuric acid cooled by a mixture of ice and ſalt. In proportion as the acid gas was extricated, it combined with the ſulphuric acid without one ſingle bubble arriving at the upper part. Soon afterwards the whole of the acid became congealed; notwithſtanding which the combination continued to be made.

From time to time, the tube through which the ſulphureous acid paſſed was obſtructed, and required to be withdrawn out of the fluid. By this treatment the matter flowed, and a quantity of gas was diſengaged in the form of bubbles. The ſolid combination had no very perceptible ſmell while it retained the form of ice; but when a portion was taken out, and laid upon a plate of glaſs, it exhibited an effervſcence like that of marble when an acid is poured on it, and ſoon afterwards it became liquid and very odorant.

When the temperature of the mixture of ſalt and ice had riſen to the freezing-water point, part of the ſolid maſs became liquid, and bubbles were formed which roſe up in the empty part of the vaſe; but as this was exactly cloſed, they cryſtallized on the ſides in the form of the leaves of fern.

Action of the Nitric Acid.] The ſulphureous acid decompoſes the nitric acid, particularly when this laſt is in a concentrated ſtate. The fluid immediately becomes red, and ſoon afterwards hot, with a diſengagement of nitrous gas frequently mixed with a ſmall quantity of ſulphureous acid. The ſulphureous acid is changed by this operation into ſulphuric acid, for it abundantly precipitates the muriate of barytes. Hence it is proved, that the ſulphureous acid has more affinity with oxygene than the nitrous oxide. This truth is confirmed by the converſion of ſulphur into ſulphuric acid by means of the nitric acid.

Action of the Oxygenated Muriatic Acid.] If the oxygenated muriatic acid gas and the ſulphureous acid gas be brought into contact, white fumes are afforded; the two gaſes become immediately liquid, and loſe their odour, if they have been employed in ſuitable proportions. The ſame effects take place when theſe two acids are mixed together in the liquid ſtate, namely, the loſs of odour and colour. From the action of theſe two bodies on each other, ſulphuric acid and common muriatic acid are produced. The theory of this operation is nearly the ſame as that of the preceding experiment.

VI. Action of combuſtible Bodies on the Sulphureous Acid.

The Action of Hydrogene.] Theſe two ſubſtances have no mutual action in the cold; but if they be paſſed through a tube of glaſs or porcelaine well heated, in the proportion of three parts by meaſure of hydrogene gas and one part of ſulphureous acid gas, the latter is decompoſed. Sulphurated hydrogene gas is formed; and at the extremity of the tube oppoſite to that at which the gaſes were introduced, very abundant cryſtals of ſulphur are depoſited.

Hydrogene therefore, at an elevated temperature, has more affinity with oxygene than ſulphur has. Conſequently it is not probable, as ſome perſons imagine, that ſulphur may be burned by means of water to convert it into ſulphuric acid,

The Action of Phoſphorus.] Phoſphorus does not alter the ſulphureous acid. When theſe two ſubſtances are ſtrongly heated in a glaſs tube, nothing paſſes which indicates the decompoſition of the ſulphureous acid. The phoſphorus becomes fixed in cooling in the form of tranſparent drops which do not contain an atom of ſulphur, and the ſulphureous acid gas is ſtill found to poſſeſs all its properties.

In this caſe, therefore, there is a ſtronger adheſion between the ſulphur and oxygene than between the phoſphorus and the ſame principle. And accordingly this combuſtible ſubſtance, heated with ſulphuric acid, does not deprive it of more oxygene than exceeded the conſtitution of the ſulphureous acid.

Action of Phoſphorated Hydrogene Gas.] As ſoon as the ſulphureous acid gas and phoſphorated hydrogene gas come into contact, a white fume appears, and they loſe their fluid ſtate. Plates of a yellow matter are precipitated on the ſides of the containing veſſel, which take fire on a hot iron, firſt, in the manner of phoſphorus, and afterwards with the characters of ſulphur. It follows from this experiment, that the hydrogene is the only ſubſtance which burns, or combines with oxygene, in theſe circumſtances, ſince the gaſes loſe their elaſticity, and the phoſphorus and ſulphur are found combined together.

Action of Sulphurated Hydrogene Gas.] The ſulphureous acid gas is decompoſed by the ſulphurated hydrogene gas. The hydrogene takes the oxygene from the ſulphur, and this laſt principle is ſeparated on both ſides. It therefore forms a very abundant depoſition. When theſe gaſes are diſſolved in water, they mutually undergo the ſame decompoſition, and the ſulphur then precipitates to the bottom of the liquor *. If ſuitable quantities of theſe ſolutions be taken, the odour of both diſappears.

Action of Carbone.] We paſſed ſulphureous acid gas through a tube of glaſs containing ignited charcoal. At the extremity of the apparatus cryſtallized ſulphur was depoſited, and carbonic acid was produced, together with a ſmall quantity of ſulphurated hydrogene gas, from a portion of the water which was decompoſed. This decompoſition does not take place in the cold.

VII. Combinations of the Sulphureous Acid with Alkalis.

THE ſulphureous acid readily unites with alkalis and earths. The generic name of ſulphites has been given to theſe combinations. They may be prepared in two ways; either by preſenting the aqueous ſolution of the ſulphureous acid to the baſes, or by applying the acid in the ſtate of gas to theſe bodies, either diſſolved or mixed with water. The latter method is preferable, for ſeveral reaſons too long to be detailed here.

The ſulphureous acid being very different from the ſulphuric acid, it may eaſily be conceived that its combinations ought to poſſeſs properties not at all reſembling thoſe of the ſulphates. It will in fact be ſeen, that theſe ſalts poſſeſs peculiarities of taſte, ſolubility and form, which belong only to themſelves; and that they are ſubject to laws of attraction and decompoſition altogether different from thoſe of the ſulphates.

---

* Fourcroy, Analyſe de l'Eau d'Enghien.

The proportions of principles which conſtitute the ſulphites not having been yet determined, we endeavoured to aſcertain a method which might lead us as near as poſſible to the truth; but it could not be applied to all the kinds of ſulphite, on account of the diſagreement of their properties; ſo that we were under the neceſſity of employing ſeveral methods.

The Sulphite of Pot-aſh.] Sulphureous acid gas was paſſed into a ſaturated ſolution of very pure carbonate of pot-aſh, until the effervefcence entirely ceaſed. During this combination, a ſmall quantity of caloric is diſengaged, and the ſolution cryſtallizes by cooling.

This ſalt is uſually white and tranſparent; ſometimes it is ſlightly yellow and ſemi-tranſparent, if its ſolution has been very concentrated, and the cryſtallization confuſed. Its taſte is penetrating and ſulphureous, its figure that of a long rhomboidal plate; its cryſtallization often preſents ſmall needles diverging from a common centre.

When expoſed to a ſudden heat it decrepitates, and loſes its water of cryſtallization; afterwards by ignition it emits ſome vapours of ſulphureous acid; and at length a portion of ſulphur is ſeparated, and the reſidue is ſulphate of pot-aſh with a ſlight exceſs of alkali.

By expoſure to air it ſlightly effloreſces, becomes opake and hard, its penetrating ſulphureous taſte diſappears, and it acquires another, which is acrid and bitter. In this ſtate it no longer effervefces with acids.

If this experiment be made in a cloſed apparatus with oxygene gas, it is found that the volume of the gas is diminiſhed, and that it is even totally abſorbed when the quantity is properly regulated. We ſee therefore that the ſulphite of pot-aſh may be converted into ſulphate by depriving it of a portion of its ſulphur by fire, or by introducing a quantity of oxygene at a low temperature.

The ſulphite of pot-aſh is ſoluble in a quantity of water nearly equal to its own maſs; and this ſolubility is increaſed by heat.

This ſalt is decompoſed by lime and by barytes, as may be ſhewn by pouring lime water into a ſolution of the ſulphite of pot-aſh. A white precipitate is afforded, which is the ſulphite of lime, and the pot-aſh remains diſengaged in the water.

The ſulphureous acid does not therefore follow the ſame laws of affinity as the ſulphuric acid, ſince this laſt adheres more ſtrongly to pot-aſh than to lime.

The alkalis do not change the nature of the ſulphite of pot-aſh.

Among the acids, ſome decompoſe the ſulphite of pot-aſh by ſeparating the ſulphureous acid; others change its nature without driving off its acid, but by affording a portion of oxygene, and converting it into ſulphuric acid. The firſt of theſe effects is produced by the ſulphuric, muriatic, phoſphoric and fluoric acids; the ſecond is effected by the nitric and the oxygenated muriatic acids. The acids of borax and of carbo do not occaſion any change in the cold.

When the ſulphuric acid is poured on the ſulphite of pot-aſh, a rapid effervefcence takes place with a crackling noiſe, at the ſame time that much caloric is diſengaged.

The nitric acid, on the contrary, emits red vapours, mixed only with a ſmall quantity of ſulphureous acid, and the reſidue is compoſed of a mixture of the ſulphate and nitrate of pot-aſh.

The oxygenated muriatic acid mixed with the ſulphite of pot-aſh immediately loſes its ſmell, and the fluid is deprived of its ſulphureous taſte.

If the ſolution of the ſulphite of pot-aſh, and alſo that of the oxygenated muriatic acid, be both concentrated, cryſtals of the ſulphate of pot-aſh are formed immediately on the mixture.

Charcoal converts the ſulphite of pot-aſh into the ſulphuret of pot-aſh.

Salts with baſes of pot-aſh do not decompoſe this ſalt.

Thoſe with baſes of ſoda, except the borate and the carbonate, are decompoſed by the ſulphite of pot-aſh.

All the other ſalts, of which the acids are ſtronger than the ſulphureous acid, are equally decompoſed by the ſulphite of pot-aſh.

The ſuper-oxygenated nitrate and muriate of pot-aſh, when heated with the dry ſulphite of pot-aſh, take fire, and are changed into ſulphate.

Several metallic oxides act upon this ſalt. Some are entirely reduced to the metallic ſtate; ſuch as the oxides of gold, ſilver and mercury: others are brought nearer that ſtate; ſuch as thoſe of lead, iron and manganeſe, at the maximum of oxygenation. There are others which change the nature of the ſulphite of pot-aſh in an oppoſite direction to thoſe which take place in the foregoing caſes; that is to ſay, which convert it into ſulphate by depriving it of a certain quantity of ſulphur, with which they form ſulphurated oxides, ſuch as the oxides of arſenic, and of iron ſlightly oxided; but in order that this operation may ſucceed, it is neceſſary to boil theſe ſubſtances a long time in water, and afterwards to add to the ſolution an acid, which occaſions a coloured precipitate, at the ſame time that the ſmell of ſulphurated hydrogene gas is emitted.

All the metallic ſolutions except the carbonates are decompoſed by the ſulphite of pot-aſh; and as moſt of the metallic ſulphites are inſoluble, different coloured precipitates are formed, according to the nature of the metal and its ſtate of oxidation.

[*To be continued.*]

VI.

A Memoir on the Nature of the Alum of Commerce, on the Exiſtence of Pot-aſh in this Salt, and on various ſimple or triple Combinations of Alumine with the Sulphuric Acid. Read before the National Inſtitute of France. By Citizen VAUQUELIN [*].

IN my memoir on the leucite or white grenate, I have announced that many natural earths and ſtones contain pot-aſh in a ſtate of combination; and I founded my opinion at that time on the impoſſibility of obtaining ſolid cryſtallized alum in octahedrons by the immediate combination of ſulphuric acid and pure alumine, whatever precautions were taken to clear it of the exceſs of acid, without the addition of alkali.

The neceſſity of this addition has long been known in the alum works, more particularly in the treatment of the mother waters; and it was thought that the uſe of pot-aſh in this circumſtance was merely to ſaturate the exceſs of acid, which was ſuppoſed to prevent the cryſtallization of the alum. Nevertheleſs, the remark made by Bergman, that ſoda and lime, employed inſtead of pot-aſh or ammoniac in the treatment of the mother waters, do not favour the cryſtallization of the alum, ought to have produced a change in the general opinion reſpecting the action of pot-aſh or ammoniac [*]. This learned chemiſt had likewiſe aſcertained, that ſeveral alums decompoſed by ammoniac afforded by evaporation the true ſulphate of pot-aſh, the baſis of which might be afforded, according to him, either from the argillaceous earth in which vegetables had been decompoſed, or from the wood-aſhes intentionally added, or, laſtly, from a caſual mixture during the calcination of the ores. And he concluded from theſe obſervations, that the ſulphates of pot-aſh and of alumine unite together in the ſtate of a triple ſalt [†].

---

[*] This important Memoir is inſerted in the Annales de Chimie, XXII. 258, whence I have tranſlated it.

Though Bergman appears to ſuſpect that pot-aſh is neceſſary to the formation of alum, yet he does not venture to affirm it; as may eaſily be ſeen in the courſe of his diſſertation: ſo that this queſtion remained ſtill undecided. In fact, we ſee by another paſſage [‡], that Bergman falls into the common opinion, by confining the effect of the alkalis to the ſimple ſaturation of the exceſs of acid, exiſting, according to him and all other authors, in the aluminous waters; and even by conſidering the new ſalts formed by theſe alkaline ſubſtances as foreign bodies, leſs noxious in fact than the exceſs of acid, but which cannot however be ſold for alum [§].

If the only effect of alkalis in the management of aluminous lixivia were to ſaturate the exceſs of acid they are ſuppoſed to contain, it is evident that any other ſubſtance capable of abſorbing this acid would anſwer the ſame purpoſe. But experience has proved the contrary, and it has been long known that this remarkable property belongs excluſively to pot-aſh and ammoniac.

In order to explain this obſcure circumſtance by experiment, I diſſolved very pure alumine in ſulphuric acid of equal purity. I evaporated the ſolution ſeveral times ſucceſſively, even to dryneſs, for the purpoſe of expelling the ſuperabundant acid. I rediſſolved the dry and pulverulent reſidue in water, and reduced the ſolution to different degrees of ſpecific gravity, with a view to ſeize the point moſt favourable to cryſtallization: but whatever precautions I took, I could not obtain any thing but a magma formed of ſaline plates without conſiſtence or ſolidity. The ſolution here deſcribed, which had conſtantly refuſed to afford cryſtallized alum alone, afforded it immediately by the addition of a few drops of the ſolution of pot-aſh; and as I had employed theſe two ſubſtances in the requiſite proportion, the reſt of the ſolution afforded to the very end pure alum without any mixture of ſulphate of pot-aſh.

---

[*] Notatu dignum eſt, quod hoc cryſtalliſationis obſtaculum alcali volatili æquè tollatur, non vero alcali minerali et calce. Bergman de Confect. Aluminis, pag. 325, tom. i. Opuſcula.

[†] Hoc alcali quod ineſt, vel ex ipſâ argillâ repetendum quæ vegetabilium putrefactorum reſiduis fuit inquinata, vel ex cineribus ſtudio additis, vel denique ſub calcinatione et uſtione fortuito immixtis. Interea hinc conſtat, quod alumen et alcali vegetabile vitriolatum facilè connubium ineant, quò ſal oritur triplex. Berg. ibid.

[‡] Allata momenta ſuſpicionem movent, quod alcali vegetabile alumine perficiendo ſit neceſſarium, ideoque omne alumen perfectum inſtar ſalis triplicis reſpiciendum: ſed hæc conjectura vacillat; nam eadem perfectio alcali volatili et ſpontaneâ evaporatione obtinetur. Non tamen improbandam puto additionem alcali vegetabilis et depurati, nam heterogenea magis nocent quam juvant. Berg. ibidem.

[§] Ut eo purius obtineatur alumen in alterâ cryſtallizatione nonnullis in locis additamenta uſurpantur alcalina, calx et urina. Scilicet multorum annorum experientiâ compertum eſt, lixivium aliquando tantam et adquirere conſiſtentiam (quod in officinis pingueſcere dicitur) ut et cryſtalli ægrè ſecernantur, et quæ prodeunt variis heterogeneis irretitæ reperiantur. His incommodis alcalinis præſertim obicem ponere tentatum fuit, quùm lixivium aciditate abundaret. Cineris clavettati et calx, ſive uſta ſive cruda, acidum abſorbent, et ſi juſtâ addentur doſi, peregrina noxia re-vera præcipitando minuunt, quod, cognitâ lixiviorum indole, S. IX. luculentius patebit. Urina tamen nihil efficit, niſi quatenus alcali volatili prædita. Negari tamen non poteſt quin novi ſales peregrini immiſceantur, nimirum alcali vegetabile vitriolatum, vel alii pro diverſo additamento, ſine dubio, ſublatis magis innocui, ſed nihilominus pro alumine vendendi. Berg. ibid. p. 316.

Into another portion of the ſame ſolution of pure alumine I dropped the ſame quantity of carbonate of ſoda as I had added of that of pot-aſh to the former. No cryſtallization was formed, even by the help of evaporation.

Lime and barytes produced no better effect.

Theſe experiments began to confirm the opinion I had, that the cryſtallization of alum is not prevented by an exceſs of acid, as has hitherto been thought, and that pot-aſh was not of uſe ſimply to ſaturate this acid, but that it performed an office of more importance. For I reaſoned, that if the common opinion were true, ſoda, lime, barytes, and all the ſubſtances which by a more powerful force would take this acid from alum, ought to give the ſame reſult. Another argument likewiſe preſented itſelf, which ſeemed deciſive. If the alkalis, pot-aſh and ammoniac do nothing more than unite to the ſuperabundant acid of the alum, the ſulphates of pot-aſh and of ammoniac ought not to occaſion any change in pure alum in its acidulated ſtate; but if theſe alkalis enter as a conſtituent part into the alum, and are neceſſary to its exiſtence, they ought to produce the ſame effects as pure pot-aſh or ammoniac.

I therefore added to a third portion of the ſolution of ſulphate of alumine before mentioned, ſome drops of the ſolution of ſulphate of pot-aſh; immediately upon which octahedral alum was formed. The ſulphate of ammoniac preſented the ſame effect.

This reſult gave ſtill greater confirmation to my firſt notions, though it did not yet afford a demonſtration perfectly without objection. For it might have happened that the two ſalts I made uſe of might determine the cryſtallization of the alum, ſimply by abſorbing the ſuperfluous acid, of which they are very greedy *.

To determine this poſſible fact, I mixed in the uncryſtallizable ſolution of alumine ſome of the acid ſulphate of pot-aſh, and obtained a cryſtallization no leſs abundant than with the neutral ſulphate of pot-aſh.

This laſt experiment does not therefore leave any doubt with regard to the influence and mode of action of pot-aſh and ammoniac in the fabrication of alum. This action is ſtill more ſtrongly confirmed, by the examination of the alums which have been formed by the proceſſes above related. For in this manner it is proved that they contain notable quantities of the ſulphates of pot-aſh and ammoniac.

Theſe experiments naturally led me to an examination of the different alums of commerce. Bergman had already announced, though in an indiſtinct manner, that not only the common alum, but likewiſe that of Rome, when decompoſed by ammoniac, afforded traces of the ſulphate of pot-aſh; and Scheele had remarked on his ſide, that alum which does not contain pot-aſh is not fit for making pyrophorus. Bergman, quoting this fact from Scheele, ſhews likewiſe that he conſidered the ſulphate of pot-aſh in alum as a foreign ſubſtance †.

---

* Ceterum alumen non tantum vulgare, ſed etiam romanum alcali volatili præcipitatum liquorem exhibet, qui haud raro alcali vegetabile vitriolatum continet. ibid.

† Alumen hoc inquinamento ſpoliatum pyrophoro generando ineptum eſt: quod facile experiri licet, nam magma aluminoſum diſtinctam reſpuens cryſtallizationem, nullum præbet pyrophorum, modo conſueto tractatum, quamvis idem addito alcali vegetabilis pauxillo, eximium porrigat, &c.

In fact, if this chemiſt had thought pot-aſh eſſential to the conſtitution of alum, he certainly would not have thought it adviſable to uſe a portion of the clay of Cologn to deſtroy the exceſs of acid in aluminous waters. For this addition is extremely vicious, and I am convinced that it was ſuggeſted by reaſoning rather than by experiments *.

Out of all the kinds of alum which I had ſubmitted to analyſis, I did not find one which did not afford ſulphate of pot-aſh or of ammoniac, and frequently both at once. I uſed the following method of analyſis: In the firſt place, to direct my operations, I take a ſmall quantity of the alum I wiſh to aſſay, which I diſſolve in the aqueous ſolution of pure pot-aſh, and ſlightly heat the mixture. If it contain ſulphate of ammoniac, a ſtrong ſmell of volatile alkali is immediately perceived. I then put into a tubulated retort a given quantity of this ſulphate of alumine; I adapt a receiver containing a ſmall quantity of water, and then pour on the alum a ſolution of pot-aſh in a proper quantity to decompoſe the ſulphate of ammoniac and alumine at once. I boil this mixture for a quarter of an hour, at the end of which all the ammoniac is volatilized, provided no more than three or four docimaſtic quintals be operated upon. I combine this ammoniac to ſaturation with the ſulphuric acid, and the quantity of ſalt which I obtain indicates that which was contained in the ammoniacal ſulphate of alumine.

When the pot-aſh does not indicate the preſence of ammoniac, which is very ſeldom, I follow another method to ſeparate the ſulphate of pot-aſh. I decompoſe the alum by means of ammoniac, and, after having waſhed the precipitated alumine, I evaporate the liquor to dryneſs, and heat the remaining ſalt in a crucible, till it emits no more white vapours of the ſulphate of ammoniac. The remainder is the pure ſulphate of pot-aſh. By theſe analytical methods, I found that one pound of cryſtallized alum contains about one ounce 64 grains of ſulphate of pot-aſh; but as the alum contains about 0,44 water of cryſtallization, this raiſes the quantity of the ſulphate to one ounce 7 gros 17 grains, for a pound of alum, or otherwiſe for cryſtallized alum, about 0,070, and for dry alum, 0,125. When the alum has been formed with volatile alkali, it is found to contain the ſulphate of ammoniac, nearly in the ſame proportion as the ſulphate of pot-aſh. Whence it follows, that a quintal of alum prepared with pot-aſh contains,

1. Sulphate of alumine — — —	49
2. Sulphate of pot-aſh — — —	7
3. Water — — — —	44
	100

When the alums contain both the ſalts here mentioned at once, which frequently enough happens, I uſe lime inſtead of pot-aſh to diſengage the ammoniac, and proceed with the reſidue as before.

It may therefore be aſcertained, by theſe ſimple eſſays, whether pot-aſh or ammoniac or both together have been uſed in a manufactory for the preparation of alum. This proof

---

* Hujus (lixivii magiſtralis) cantharo duas addidi drachmas argillæ Colonienſis in ſubtilem comminatæ pulverem, et pauci aquæ humectatæ; calore ebullitionem provocavi, qua per decem minutæ continuata, et poſtea, refrigeratione peractâ, reſidnam ſeparavi argillam; lotam ſiccavi; tandemque ponderatione inveni 25.5 grana ſoluta, quæ aluminis augmentum 141 granorum indicant. Berg. ibidem.

may be of ſome utility; for Bergman pretends, I don't know on what foundation, that urine communicates to alum properties which are hurtful in dyeing: it is not probable, however, that this chemiſt aſſerted the fact without proof. In all the works where putrid urine is employed for the treatment of alum waters, the alum contains the ſulphates of pot-aſh and of ammoniac, becauſe the combuſtibles which ſerve to roaſt the ores depoſit a certain quantity of alkali, which unites with the ſulphuric acid, and contributes, according to its proportion, to the formation of a greater or leſs quantity *.

It is known, from the experiments of Bergman and ſeveral other chemiſts, that by boiling a ſolution of ordinary alum with pure alumine in a very divided ſtate, this laſt combines with the alum, and renders it inſoluble in water; that is to ſay, it converts it into the ſtate of neutral ſulphate of alumine, or ſaturated with its earth. I have repeated this experiment with the deſign to aſcertain whether the ſulphates of pot-aſh and ammoniac are precipitated with the alum; and I immediately obſerved, that the combination does not take place but by means of heat, though I uſed alumine recently precipitated from a ſolution, and ſtill humid; and that at the end of a certain time the alum was entirely precipitated, and ſcarcely any ſigns of it left in the water. I re-diſſolved the alum thus precipitated, in diluted ſulphuric acid, and this ſolution afforded very fine cryſtals of octahedral alum by cooling; whence it follows that the pot-aſh and ammoniac did fall down with the ſulphate of alumine, and joined in the formation of this quadruple, earthy, taſteleſs ſalt. I at firſt imagined that this fact might ſerve to explain how it ſometimes happens that aluminous waters, paſſed over materials leſs rich than thoſe from which they were obtained, diminiſhed in denſity by the loſs of a portion of the acid neceſſary to the ſolution of the alum; but as the combination is not made without heat, and it requires a great diviſion in the alumine, I preſume that this effect is owing to another cauſe: nevertheleſs, it might be poſſible, in the proceſs of time, eſpecially in hot weather, that ſomething of this nature might happen. It was intereſting to determine what happens with regard to the ſulphates of pot-aſh and ammoniac in the precipitation of alum by its own baſe. I therefore boiled a ſolution of pure ſulphate of alumine, that is to ſay, which contained no alkali, and was not cryſtallizable, with a certain quantity of the earth in a very divided ſtate. It diſſolved a ſmall quantity, loſt its ſlight acidity, but did not become inſoluble. Having afterwards dropped a very ſmall quantity of the ſolution of pot-aſh into this liquor, a precipitate was formed in a ſhort time, which was of the ſame nature as that of the foregoing experiment; that is to ſay, what is called alum ſaturated with its earth. It is therefore proved that the ſulphates of pot-aſh or ammoniac are neceſſary to render the alum capable of being precipitated by its earth, or to cauſe it to paſs, as it were, to the earthy ſtate. It is alſo proved, that the aluminous waters which do not contain pot-aſh may remain as long as may be deſired on their materials, without becoming ſaturated with too great a quantity of earth, or ſuffering alum to precipitate.

From the whole of what we have thus far explained, it will be eaſy to draw a number of conſequences of importance to the arts, chemiſtry, and natural hiſtory.

1. It is not, at leaſt in the greateſt number of circumſtances, the exceſs of acid which impedes

---

* When alums contain at the ſame time the ſulphate of ammoniac and of pot-aſh, the quantity of the latter is leſs; and in this reſpect there muſt be great varieties in the proportions, according to the doſe of urine or of pot-aſh which is added. V.

pedes the cryſtallization of alum; but it is the want of pot-aſh or ammoniac. For it is difficult indeed to imagine that the ſulphuric acid could remain diſengaged after ſo long remaining upon alumine in a ſtate of extreme diviſion, and always ſuperabundant. It is true that the aluminous waters redden the vegetable tinctures, but this property is not owing to a diſengaged acid. This portion of acid is a conſtituent part of theſe waters, and it appears to have more affinity with the neutral ſulphate of alumine than with a new quantity of this earth at the temperature of the atmoſphere.

2. The ſulphate of pot-aſh may be uſed, as well as pure pot-aſh, to cauſe the cryſtallization of alum. It even has the advantage over the latter ſalt, becauſe, if the aluminous waters do not really contain a diſengaged acid, the pot-aſh, in its combination, will precipitate a portion of alumine, and diminiſh the product of the boiling; whereas the ſulphate of pot-aſh does not produce the ſame effect: but if the lixiviums contain diſengaged acid, which muſt very ſeldom be the caſe, it is not converted into alum by the ſulphate of pot-aſh, and is loſt with regard to the product. I think, therefore, that with regard to ſuch waters as really contain an exceſs of acid, or a very oxided ſulphate of iron, the uſe of pot-aſh may be preferable to that of the ſulphate of pot-aſh. But with regard to the price of theſe ſubſtances, I think that in many places it would be profitable to uſe the ſulphate of pot-aſh, becauſe it is a ſalt indirectly produced in a great number of manufactories, where it may of courſe be obtained at a very moderate price. The reſidues of the diſtillation of aqua fortis by the ſulphuric acid would be excellent for this operation. I mean that they would be preferable to the neutral ſulphate of pot-aſh. For I have remarked that this laſt precipitates a portion of aluminous earth, which the other does not. This ſalt would more eſpecially poſſeſs a decided preference before pot-aſh, in thoſe caſes where the aluminous waters contain at the ſame time a great quantity of ſulphate of iron intended to be uſefully employed, becauſe it would act immediately on the ſulphate of alumine, without touching that of the iron; whereas the pot-aſh does not begin to form alum until the whole of the ferruginous ſalt is decompoſed. It would be in particular of much greater advantage than putrid urine, becauſe this fluid always contains phoſphoric ſalts, which decompoſe a portion of the ſulphate of alumine, and conſiderably diminiſh the product.

This inconvenience might, however, be avoided by adding a certain quantity of lime to the urine, to precipitate the phoſphoric acid.

3. Alumine cannot be uſed in the treatment of mother waters, as Bergman propoſes. This earth is incapable of favouring the cryſtallization of alum, beſides which, it decompoſes a portion of alum by the aſſiſtance of ebullition; in which circumſtance it ſeizes the acid neceſſary to its ſolution, and precipitates it in the form of that powder which is called alum ſaturated with its earth.

4. Many alum ores muſt naturally contain pot-aſh, becauſe perfect alum is often obtained from the firſt cryſtallization of new alum waters without the addition of this alkali. It is true that an objection may be made with regard to the wood uſed in calcining theſe ores, which may be ſuppoſed to have furniſhed the alkali; but it is not probable that the ſmall quantity of wood employed, in compariſon to the quantity of ore and the alum it affords, could ſupply enough of pot-aſh for the cryſtallization.

5. All the earths and ſtones which have given, or ſhall hereafter afford, by analyſis with the ſulphuric acid, perfect alum without addition of pot-aſh, muſt contain this alkali naturally,

turally. For it is well proved, by the experiments related above, that alum cannot exiſt without pot-aſh or ammoniac; and as there is little probability that this laſt ſhould be found combined in earths or ſtones, unleſs perhaps in very rare caſes, we may almoſt conſtantly be aſſured, when alum is obtained from any of theſe ſubſtances, that its formation was effected by pot-aſh. The quantity of alum will immediately ſhew in what proportion this alkali exiſted in the ſubſtances analyſed. I have announced to the Inſtitute in the communication of my experiments on the leucite, that I had begun a ſeries of experiments on ſeveral earths and ſtones which I preſumed to contain pot-aſh. At preſent I can give the reſults of ſome of my experiments.

The crude alum ore of La Tolfa afforded me 2,3 per cent.; but as it is difficult to extract more than two-thirds of this ſubſtance from the ſtones which contain it, we may, without fear of miſtake, eſtimate the quantity at 3,4 per quintal *. The zeolite of the Iſle of Ferro afforded 1,78, which makes according to our eſtimation 2,37 per cent. The argillaceous earth of Forges in Normandy, of which the pots of the glaſs works at Seves are made, likewiſe afforded it, but in a very ſmall quantity †. The adamantine ſpar, on the analyſis of which Citizen Guyton is at preſent employed, muſt contain a conſiderable quantity; for it affords much alum when treated immediately with the pure ſulphuric acid. In reading over the analyſes of ſtones which have hitherto been made, an almoſt certain proof will be found, no doubt, either by the loſs in the ſum of their products, or by the alum they afforded with the ſulphuric acid, that they contained pot-aſh, though no mention either was made or could be made reſpecting it.

6. The alum of commerce ought not to be conſidered as a ſimple ſalt, but as a combination in the ſtate of a triple and ſometimes quadruple ſalt of ſulphate of alumine, ſulphate of pot-aſh, or of ammoniac. Among theſe laſt we may diſtinguiſh two ſpecies; the one without exceſs of acid, inſoluble in water and inſipid, being what is improperly called alum ſaturated with its own earth; and the other, which contains an exceſs of acid ſoluble in water, very ſapid and aſtringent, is the common alum.

There is likewiſe a pure ſulphate of alumine, very aſtringent, very difficult of cryſtallization, in the form of brilliant pearl-coloured plates without conſiſtence, and which cannot be rendered inſoluble by the addition of a new quantity of its baſe. This laſt ſalt may with the greateſt propriety be called the ſulphate of alumine.

7. It follows from the comparative analyſis, and the knowledge acquired reſpecting the

---

* 100 parts of the ore of Tolfa contain

1. Alumine - - - -	43,92
2. Sulphuric acid - -	25,00
3. Pot-aſh - - -	3,08
4. Water - - -	4,00
5. Silex - - - - -	24,00
	100,00

† 100 parts of the earth of Forges, calcined, contain:

Alumine - - -	40
Silex - - - -	60
	100

different ſtates of the combination of alumine with the ſulphuric acid united at the ſame time with other baſes, that we muſt diſtinguiſh ſeven ſtates in this combination, and that it is neceſſary to expreſs them according to the rules of the methodical nomenclature. Here follow the ſeries, the nature, and the names of theſe ſeven ſulphates of alumine.

1. Sulphate of alumine, or the artificial combination of ſulphuric acid and alumine. This ſalt is aſtringent; it cryſtallizes in laminæ or flexible leaves, ſoluble in water. It has never been deſcribed nor named by chemiſts.

2. Acid ſulphate of alumine. It is the foregoing ſalt, with exceſs of acid, from which it differs by reddening blue vegetable colours. It is eaſily made by diſſolving that ſalt in the ſulphuric acid, but it is not eaſy to convert this into the neutral ſulphate of alumine but by boiling it a long time with its earth. This ſalt, like the firſt, has not been deſcribed.

3. Saturated ſulphate of alumine and of pot-aſh. It is the alum of the chemiſts ſaturated with its earth. I have deſcribed the manner of making it. It is pulverulent, inſipid, inſoluble, not cryſtallizable, and is eaſily converted into true alum by the addition of ſulphuric acid.

4. The acid ſulphate of alumine and of pot-aſh. It is eaſy to prepare it chemically. It greatly reſembles common alum; but I have found none but that of La Tolfa which is of the ſame nature.

5. The acid ſulphate of alumine and of ammoniac. It is eaſily made in our laboratories. I have not yet found it pure in commerce. It has all the properties of alum, and may be uſed for the ſame purpoſes.

6. The acid ſulphate of alumine, pot-aſh, and ammoniac. It is remarkable enough, that this ſhould be the nature of the alum moſt frequently made in the arts, and that to expreſs its combination ſo many words ſhould be neceſſary. This, however, may be avoided by reſerving the name of alum to this ſubſtance, which will be ſufficient to diſtinguiſh it perfectly.

7. The acidulous ſulphate of alumine and of pot-aſh. I am leſs acquainted with this than the preceding ſpecies. The name which I propoſe to characteriſe it has been ſuggeſted to me, becauſe by adding a ſmall quantity of pot-aſh to the ſolution more than is neceſſary to obtain octahedral cryſtals, it manifeſtly paſſes to the cubic form.

8. Laſtly, the phyſician, the chemiſt, and the manufacturer, with whom the uſes of alum are greatly multiplied, will hereafter poſſeſs a knowledge of the ſubſtance they employ, and may appreciate its effects on the animal economy, and other bodies to which it is ſo frequently applied.

VII.

Deſcription of an Inſtrument proper to meaſure the Volume of a Body without plunging it in any Liquid. By H. SAY, Capitaine du Génie [*].

THE ordinary method of determining the ſpecific gravity of ſolids by immerſion in a liquid is inconvenient for powders and fluids, and impracticable with regard to ſuch bodies as may be acted upon by the ſtandard liquid. The learned author of the memoir from which the following extract is taken, has propoſed to remove this difficulty by plunging the ſubject of experiments in common air or a gaſeous fluid, by means of an inſtrument he calls a ſtereometer.

---

[*] Abſtract of a Memoir printed in the Annales de Chimie, XXIII. 1.

If the capacity of a veſſel, or, which is the ſame thing, the volume of air contained in that veſſel, be meaſured, when the veſſel contains air only, and alſo when the veſſel contains a body whoſe volume is required to be known, the volume of air aſcertained by the firſt meaſurement, deducting the volume aſcertained by the ſecond, will be the volume of the body itſelf. Again, if it be admitted as a law, that the volume of any maſs of air be inverſely as the preſſure to which it is ſubjected, the temperature being ſuppoſed conſtant, it will be eaſy to deduce, from the mathematical relations of quantity, the whole bulk, provided the difference between the two bulks under two known preſſures be obtained by experiment.

Let it be ſuppoſed, for example, that the firſt preſſure is double the ſecond, or, which follows as a conſequence, that the ſecond volume of the air be double the firſt, and that the difference be fifty cubic inches, it is evident that the firſt volume of the air will likewiſe be fifty cubic inches. The ſtereometer is intended to aſcertain this difference at two known preſſures.

The inſtrument is a kind of funnel, AB, Fig. 1, Plate XIV. compoſed of a capſule A in which the body is placed, and a tube B as uniform in the bore as can be procured. The upper edge of the capſule is ground with emery, in order that it may be hermetically cloſed with a glaſs cover M ſlightly greaſed. A double ſcale is paſted on the tube, having two ſets of graduations; one to indicate the length, and the other the capacities, as determined by experiment.

When this inſtrument is uſed, it muſt be plunged in a veſſel of mercury with the tube very upright, until the mercury riſes within and without to a point C of the ſcale, ſee Fig. 2.

The capſule is then cloſed with the cover, which being greaſed will prevent all communication between the external air and that contained within the capſule and tube.

In this ſituation of the inſtrument, in which the mercury ſtands at the ſame height within and without the tube, the internal air is compreſſed by the weight of the atmoſphere, which is known and expreſſed by the length of the mercury in the tube of the common barometer.

The inſtrument is then to be elevated, taking care to keep the tube conſtantly in the vertical poſition. It is repreſented in this ſituation Fig. 2, ſecond poſition. The mercury deſcends in the tube, but not to the level of the external ſurface, and a column DE of mercury remains ſuſpended in the tube, the height of which is known by the ſcale. The interior air is therefore leſs compreſſed than before, the increaſe of its volume being equal to the whole capacity of the tube from C to D, which is indicated by the ſecond ſcale.

It is known therefore that the preſſures are in proportion to the barometrical column and to the ſame column diminiſhed by the ſubtraction of DE. And the bulks of the air in theſe two ſtates are inverſely in the ſame proportion, and again the difference between theſe bulks is the abſolute quantity left void in the tube by the fall of the mercury; from which data by an eaſy analytical proceſs the following rule is deduced: Multiply the number which expreſſes the leſs preſſure by that which denotes the augmentation of capacity, and divide the product by the number which denotes the difference of the preſſures. The quotient will be the bulk of the air when ſubject to the greater preſſure.

To render this more eaſy by an example, ſuppoſe the height of the mercury in the barometer to be 78 centimetres, and the inſtrument being empty to be plunged in the mercury to the point C. It is then covered, and raiſed until the ſmall column of mercury DE is ſuſpended, for example, at the height of ſix centimetres. The internal air, which was at firſt compreſſed by a force repreſented by 78 centimetres, is now compreſſed only by a force repreſented by 78 — 6, or 72 centimetres.

Suppoſe it to be obſerved at the ſame time, by means of the graduations of the ſecond ſcale, that the capacity of the part CD of the tube which the mercury has quitted is 2 cubic centimetres. Then by the rule $\frac{72}{6} \times 2$ give 24 cubical centimetres, which is the volume of the air included in the inſtrument when the mercury roſe as high as C in the tube.

The body of which the volume is to be aſcertained, muſt then be placed in the capſule, and the operation repeated. Suppoſe, in this caſe, the column of mercury ſuſpended to be eight centimetres, when the capacity of the part CD of the tube is equal to 2 centimetres cube. Then the greateſt preſſure being denoted by 78 centimetres, as before, the leaſt will be 70 centimetres, the difference of the preſſures being 8, and the difference of the volumes two cubical centimetres. Hence $\frac{70}{8} \times 2$ gives the bulk of the included air under the greateſt preſſure 17,5 cubic centimetres. If therefore 17,5 centimetres be taken from 24 centimetres, or the capacity of the inſtrument when empty, the difference 6,5 cubic centimetres will expreſs the volume of the body which was introduced. And if the abſolute weight of the body be multiplied by its bulk in centimetres, and divided by the abſolute weight of one cubic centimetre of diſtilled water, the quotient will expreſs the ſpecific gravity of the body in the common form of the tables where diſtilled water is taken as unity, or the term of compariſon.

After this deſcription and explanation of the uſe of his inſtrument, the author proceeds with the candour and acuteneſs of a philoſopher to aſcertain the limits of error in the reſults; an object ſeldom ſufficiently attended to in the inveſtigation of natural phenomena. From his reſults it appears, that with the dimenſions he has aſſumed, and the method preſcribed for operating, the errors may affect the ſecond figure. He likewiſe gives the formulæ by means of which the inſtrument itſelf may be made to ſupply the want of a barometer in aſcertaining the greateſt preſſure. He likewiſe adverts to the errors which may be produced by change of temperature. To prevent theſe as much as poſſible, the actual form of the inſtrument and arrangements of its auxiliary parts are ſettled as in Fig. 3; by which means the approach of the hand near the veſſel and its tube is avoided. In this figure the vertical poſition of the tube is ſecured by the ſuſpenſion of the veſſel, and a perforation in the table through which the tube paſſes. The table itſelf ſupports the capſule in its firſt poſition, namely, that at which the cover is required to be put on.

It ſeems probable, from the author's general reflections immediately preceding the explanation of the figures, that he had not entirely finiſhed his meditations on this ſubject. If he had, I think it likely that he would have determined his preſſures as well as the meaſures of bulks by weight. For it may be eaſily underſtood, that if the whole inſtrument were ſet to its poſitions by ſuſpending it to one arm of a balance at H Fig. 3, the quantity of counterpoiſe when in equilibrio might be applied to determine the preſſures to a degree of accuracy much greater than can be obtained by linear meaſurement.

VIII.

Uſeful Notices reſpecting various Objects.—Styrian Steel—Elaſtic Strings for Muſical Inſtruments and other Purpoſes—Wheels without Cogs.

1. *Styrian Steel.*

SOME time ago I was favoured with a viſit from Mr. Ramondini, an Italian gentleman on his travels. He aſſured me that the Styrian ſteel is made very well in Hungary, and can be obtained from any iron originally ſmelted with charcoal, and not with coke. He ſays that the ſecret of the proceſs conſiſts in the management upon the hearth of the blaſt or refining furnace. If the iron be ſtirred about while in fuſion, it will afford bar iron; but if the ſcoria be ſimply raked off as it riſes, and the metal be left for a due time undiſturbed in fuſion, and then taken to the hammer, it proves to be ſteel. He affirms, that the iron made with coke will not cement into ſteel, but forms a ſlag. Perhaps this may ariſe from a redundancy of plumbago.

2. *Elaſtic Strings for Muſical Inſtruments and other Purpoſes.*

THE elaſtic ſtring known by the name of catgut, and very uſeful for making the bands of lathes, when its diameter is large, or the ſtrings of muſical inſtruments, when ſmall, poſſeſſes peculiar properties which are well known. As it is compoſed of animal fibre, it is ſubject to putrefy by accidental moiſture, and on this account the ſtrength of ſuch as appears to be perfect is very uncertain. I underſtand that muſical ſtrings of catgut are not made in this country. An intelligent friend of mine made an experiment many years ago with ſilk to form this laſt article. He wound the ſingle thread or fibre of the ſilkworm, without twiſting, till the maſs of fibres formed a body equal to his wiſh. He ſmeared this maſs with white of egg, which he rendered immediately conſiſtent by paſſing it through heated oil. The ſtring was exquiſitely true, or uniform in its thickneſs. When tried upon an inſtrument, the tone was not ſatisfactory to an accuſtomed ear: the performer ſaid it was tubby, a word which I ſuppoſe has reference to the ſound emitted by a tub, and will, as I imagine, convey a correct though perhaps undefinable notion to a muſician. Muſical ſtrings produce a ſound more grave the greater their diameter or their length, and the leſs their tenſion. It is required that this ſound ſhould not only be intenſe and perfect, but alſo that it ſhould endure for a certain ſhort time without conſiderable diminution. The maximum of theſe qualities, when the length is limited, is aimed at by regulating the diameter and the tenſion. If the diameter be too great, the ſound will not laſt; if too little, it will be deficient in intenſity. There is an artifice by which the permanence of a grave ſound from a given length of ſtring is ſecured, without requiring the tenſion to be much diminiſhed. It conſiſts in adding to the weight of the ſtring, without adding to its ſtrength or elaſtic force. For this purpoſe a ſlender wire is wrapped round the ſtring, which with this load gives a very different and more laſting tone than a ſimple ſtring of the ſame length. I believe it is admitted that the tone of theſe ſtrings is leſs brilliant or clear, and I ſuppoſe the ſtrings made by my friend had the ſame quality, the brittle indurated white of egg adding to the weight but not to the elaſticity. The caoutchouc or elaſtic gum was at that time unknown. I think it worth trying with ſilk for muſical ſtrings, and ſtill more in the fabrication of ſtrong bands for lathe work, to be uſed inſtead of catgut.

3. *Wheels without Cogs.*

VERY much of the diſpoſal or tranſmiſſion of mechanic force is effected by means of wheels acting upon each other. The connection of the moving parts is performed either by the mutual action of teeth, or by ſtraps, or by the friction of one face of a wheel againſt another. I have ſeen a drawing of a ſpinning-wheel for children, at a charity-ſchool, in which a large horizontal wheel with a ſlip of buff leather glued on its upper ſurface, near the outer edge, drove twelve ſpindles at which the ſame number of children ſat. The ſpindles had each a ſmall roller likewiſe faced with leather, and were capable by an eaſy and inſtantaneous motion of being thrown into contact with the large wheel at pleaſure. Each child could therefore throw her own part of the apparatus into work, or cauſe it to ſtop, as often or as long as ſhe pleaſed. The winding bobbins for yarn at the cotton-mills operate on the ſame ſimple and elegant principle, which poſſeſſes the advantage of drawing the thread with an equal velocity, whatever may be the quantity on the bobbin, and cannot break it. I do not know that this principle has been applied in large work, except in a ſaw-mill by Mr. Taylor of Southampton. In this the wheels act on each other by the contact of the end grain of wood inſtead of cogs. It makes very little noiſe, and at the time it was mentioned to me had been in wear about fourteen years. I ſuppoſe, of courſe, that there is a contrivance for bearing the wheels firm againſt each other by wedges at the ſockets, or rather by levers.

IX.

An Economical Proceſs to obtain Pure Cauſtic Alkali in the Large Way, with Fuſed Pot-aſh, or the Lapis Cauſticus. By Citizen BOUILLON LE GRANGE *.

THE proceſſes for obtaining the cauſtic alkali and fuſed pot-aſh, called lapis cauſticus in the diſpenſatories, being either defective or tedious, the Citizen Welter and myſelf have endeavoured to abridge this operation, by uſing a proceſs which requires leſs time and expence, and is more certain and uſeful, particularly for obtaining the cauſtic alkali in the large way, which is ſo neceſſary in the arts and in chemical experiments.

We believe, therefore, that practical men will receive with pleaſure the deſcription of a method by which no part of the pot-aſh is loſt, and which affords it in a ſtate of purity, and very cauſtic, without much expence or apparatus.

Our apparatus conſiſts of ſeveral veſſels of white wood, or, which is better, calcareous ſtone, whoſe dimenſions may be varied according to the quantities intended to be prepared. Thoſe which we have eſtabliſhed for the Polytechnic School are of ſtone, of the internal capacity of one foot cube, (ſee Fig. 4, Plate XIV.) having their bottoms grooved by channels of an inch in depth and the ſame width, and ſo far diſtant from each other that there are five or ſix parallel, which terminate at an end in a ſimilar groove, which croſſes the whole, and ſerves as a gutter to collect the ſaline ſolution. In the middle of this laſt an hole is pierced, to inſert a tube of glaſs, which comes out under an angle of 45 degrees with the horizon.

---

* Annales de Chimie, XXII. 137.

The channels are covered with tubes of glaſs ranged acroſs, upon which is placed a cloth in ſuch a manner as to cover them completely. Upon this a thin layer of wood-aſhes is ſprinkled, and afterwards the mixture which we ſhall proceed to mention is put into the veſſels.

For want of veſſels of ſtone, we may operate by uſing ſmall tubs of white wood, as in Figure 5; and inſtead of the grooves, river ſand muſt be ſpread on the bottom, after being firſt carefully waſhed; over this a finer ſtratum muſt be ſpread, and the whole then covered with a cloth, and wood-aſhes ſtrewed thereon. Theſe veſſels, like the others, muſt each have a tube to convey the fluid which filters through.

It is not neceſſary to remark, that the firſt method is to be preferred. For the cauſtic alkali always ſeizes a portion of the colouring matter of the wood, and may even, according to its degree of concentration, take up a ſmall quantity of ſilex by ſolution. But theſe inconveniencies are of little conſequence, when merely the cauſtic alkali or lapis cauſticus is wanted.

For the arts, and for the delicate experiments of chemiſtry, the ciſterns of calcareous ſtone are to be preferred: with theſe the liquor is obtained perfectly limpid.

Things being thus diſpoſed, equal parts are to be taken of quicklime and of pot-aſh, more eſpecially when the lime is very cauſtic. In the contrary caſe, twenty parts of lime may be taken to fifteen of pot-aſh. Water is then to be heated in an iron pot till it nearly boils; in this ſtate the lime is to be added, which by its extinction brings it to the boiling heat. When the extinction is complete, the pot-aſh is mixed, and the whole formed into a thick maſs, which is left to cool a little.

The mixture is afterwards poured into the ciſterns, and immediately covered with water. In order to avoid making perforations in the mixture by pouring the water, a ſmall board is laid to receive the ſtream, which riſes with the fluid.

Care muſt be taken to place pitchers or other veſſels to receive the liquor which flows through the tube; and in order to prevent the lixivium from abſorbing carbonic acid from the atmoſphere, the veſſels muſt be ſlightly cloſed to prevent the circulation of the external air.

It is likewiſe neceſſary that a ſupply of water ſhould be kept up over the mixture. When it comes out of the tube in an inſipid ſtate, it is unneceſſary to draw off any more.

The ſolutions obtained are all to the laſt very nearly of the ſame ſtrength; for they become weak all at once: from this circumſtance the very aqueous ſolutions are avoided.

Iron pots may be uſed to evaporate the waters. The laſt run muſt be taken firſt, becauſe it is rather weakeſt; and by this means the ſtronger ſolutions are not ſo long expoſed to the contact of the air. The evaporation muſt be performed by a ſtrong ebullition.

When the concentration has arrived to a certain point, the ſulphate of pot-aſh cryſtalliſes and falls down. It may eaſily be taken out by placing at the bottom of the pot a perforated iron ladle, in which the ſalt is collected. Strong ebullition is neceſſary to keep the atmoſpheric air at a diſtance from the ſurface, and towards the end the motion of boiling ſerves to carry the ſulphate of pot-aſh into the ladle.

If the lapis cauſticus be wanted, the concentrated liquor is to be poured into a ſmaller pot, and the evaporation carried ſo far that the ſalt may congeal when poured out on a plate of iron or marble *.

If a purer alkali be required for the delicate experiments of chemiſtry, inſtead of pot-aſh, the tartareous acidule or cream of tartar may be calcined and made uſe of; or otherwiſe the fuſed pot-aſh before-mentioned may be purified by alcohol after the manner of Berthollet. Experiment has proved to us, that very pure alkali may by this means be obtained †.

In this caſe, the lixivium is to be evaporated to the conſiſtence of a thick ſyrup in a ſilver baſon ‡, or preferably in cloſe veſſels. This matter is then to be diſſolved in alcohol. The pot-aſh alone combines with the fluid: the ſulphate and muriate of pot-aſh, the portions of earth and even of carbonic acid which it obſtinately retains, or which it has reſumed from the air during the evaporation, remain at the bottom of the ſolution. If the alcohol has been poured on the matter whilſt ſtill hot, and a leſs quantity of this ſolvent has been employed than was requiſite for diſſolving the pot-aſh, it cryſtallizes by cooling in white blades, which are ſometimes ſeveral inches long. If it be required to ſeparate the pot-aſh from the alcohol, and to obtain it in the dry ſtate, the ſolution muſt be evaporated in a ſilver baſon, and not in a glaſs veſſel; for the pot-aſh often diſſolves a portion of ſilex, which alters its purity.

We ſee by this operation, of which the details deſerve to be conſulted, in the memoir of Berthollet, that the cauſtic pot-aſh is deprived of ſilex, of carbonic acid, of all the foreign ſalts, and of the ſmall portion of iron it may have taken up from the veſſel in which the evaporation was performed.

---

* It is proper to remark, that the ſolid cauſtic alkali thus prepared is much more active than that uſually made by chemiſts for ſale. For this reaſon it is neceſſary to be very prudent in its application. G.

† See, however, the remark of Lowitz on this ſubject. "Philoſ. Journ. I. 165. N."

‡ What follows is extracted from the memoir of the celebrated Berthollet, for the advantage of thoſe who may not poſſeſs the means of conſulting the original. G.

X.

A TABLE for reducing the Unities of the Metre, Litre, and Gramme into English Inches, Gallons, and Grains. (See page 199).

		LITRES IN						LITRES IN			
Fr. Measures	Metres in Inches.	Cubic Inches.	Ale Gallons of 282 Inches.	Wine Gallons of 231 Inches.	Grammes in Grains.	Fr. Measures	Metres in Inches.	Cubic Inches.	Ale Gallons of 282 Inches.	Wine Gallons of 231 Inches.	Grammes in Grains.
1	39.383	61.083	0.2166	0.2644	22.966	51	2008.5	3115.2	11.047	13.486	1171.3
2	78.766	122.17	0.4332	0.5289	45·932	52	2047.9	3176.3	11.264	13.750	1194.2
3	118.15	183.25	0.6498	0.7933	68.898	53	2087.3	3237.4	11.480	14.015	1217.2
4	157.53	244.33	0·8664	1.0577	91.864	54	2126·7	3298.5	11.697	14.279	1240.2
5	196.91	305.41	1.0830	1.3221	114.83	55	2166.1	3359.6	11.913	14.544	1263.1
6	236.30	366.50	1.2996	1.5866	137.79	56	2205.4	121.30	121.30	14.808	1286.1
7	275.68	427.58	1.5162	1.8510	160.76	57	2244.8	3481.7	12.347	15.072	1309.1
8	315.06	488.66	1.7328	2.1154	183.73	58	2284.2	3542.8	12·563	15.337	1332.0
9	354.45	549.75	1.9495	2.3799	206.69	59	2323.6	3603.9	12.780	15.601	1355.0
10	393.83	610.83	2.1661	2.6443	229.66	60	2363.0	3665.0	12.996	15.866	1378.0
11	433.21	671.91	2.3827	2.9087	252.63	61	2402.4	3726.1	13·213	16.130	1400.9
12	472.60	733.00	2·5993	3.1731	275.59	62	2441.7	3787.1	13·430	16.395	1423.9
13	511.98	794.08	2.8159	3.4376	298.56	63	2481.1	3848.2	13.646	16.659	1446.9
14	551.36	855.16	3.0325	3.7020	321.52	64	2520.5	3909.3	13.863	16.923	1469.8
15	590.74	916.24	3.2491	3·9664	344.49	65	2559.9	3970.4	14.079	17.188	1492.8
16	630.13	977.33	3.4657	4.2309	367.46	66	2599.3	4031.5	14.296	17.452	1515.8
17	669.51	1038.4	3.6823	4.4953	390.42	67	2638.7	4092.6	14.513	17.717	1538.7
18	708.89	1099.5	3·8989	4.7597	413.39	68	2678.0	4153.6	14.729	17.981	1561.7
19	748.28	1160.6	4.1155	5.0242	436.35	69	2717.4	4214.7	14.946	18.246	1584.7
20	787.66	1221.7	4.3321	5·2886	459.32	70	2756.8	4275.8	15.162	18.510	1607.6
21	827.04	1282.7	4.5487	5.5530	482.29	71	2796.2	4336.9	15.379	18.774	1630.6
22	866.43	1343.8	4.7653	5.8174	505.25	72	2835.6	4398.0	15.596	19.039	1653.6
23	905.81	1404.9	4.9819	6.0819	528.22	73	2875.0	4459.1	15.812	19.303	1676.5
24	945.19	1466.0	5.1985	6.3463	551.18	74	2914.3	4520.1	16.029	19.568	1699.5
25	984.57	1527.1	5.4151	6.6107	574.15	75	2953.7	4581.2	16.245	19.832	1722.4
26	1023.1	1588.2	5.6318	6.8752	597.12	76	2993.1	4642.3	16.462	20.097	1745.4
27	1063.3	1649.2	5·8484	7.1396	620.08	77	3032.5	4703.4	16.679	20.361	1768.4
28	1102.7	1710.3	6.0650	7.4040	643.05	78	3071.9	4764.5	16.895	20.625	1791.3
29	1142.1	1771.4	6·2816	7.6684	666.01	79	3111·3	4825.6	17.112	20.890	1814.3
30	1181.5	1832.5	6·4982	7.9329	688.98	80	3150.6	4886.6	17.328	21.154	1837.3
31	1220.9	1893.6	6.7148	8.1973	711.95	81	3190.0	4947·7	17.545	21.419	1860.2
32	1260.3	1954.7	6.9314	8.4617	734.91	82	3229.4	5008.8	17.762	21.683	1883.2
33	1299.6	2015.7	7.1480	8.7262	757.88	83	3268.8	5069.9	17.978	21·948	1906.2
34	1339.0	2076.8	7.3646	8.9906	780.84	84	3308.2	5131.0	18.195	22.212	1929.1
35	1378.4	2137.9	7.5812	9.2550	803.81	85	3347.6	5192.1	18.412	22.476	1952.1
36	1417.8	2199.0	7.7978	9.5194	826.78	86	3386.9	5253.1	18.628	22.741	1975.1
37	1457.2	2260.1	8.0144	9.7839	849.74	87	3426.3	5314.2	18.845	23.005	1998.0
38	1496.6	2321.2	8.2310	10.018	872.71	88	3465.7	5375.3	19.061	23.270	2021.0
39	1535.9	2382·2	8.4476	10.313	895.67	89	3505.1	5436.4	19.278	23.534	2044.0
40	1575.3	2443.3	8.6642	10.577	918.64	90	3544.5	5497.5	19·495	23.799	2066.9
41	1614.7	2504.4	8.8808	10.842	941.61	91	3583.9	5558.6	19.711	24.063	2089.9
42	1654.1	2565.5	9·0975	11.106	964.57	92	3623.2	5619.6	19.928	24.327	2112.9
43	1693.5	2626.6	9.3141	11.370	987.54	93	3662.6	5680.7	20.144	24.592	2135.8
44	1732.9	2687.7	9.5307	11.635	1010.5	94	3702.0	5741.8	20.361	24.856	2158.8
45	1772.2	2748.7	9.7473	11.899	1033.5	95	3741.4	5802.9	20.578	25.121	2181.8
46	1811.6	2809.8	9.9639	12.164	1056.4	96	3780.8	5864.0	20.794	25.385	2204.7
47	1851.0	2870.9	10.180	12.428	1079.4	97	3820.2	5925.1	21.011	25.650	2227.7
48	1890.4	2932.0	10.397	12.693	1102.4	98	3859.5	5986.1	21.227	25.914	2250.7
49	1929.8	2993.1	10.614	12.957	1125.3	99	3898.9	6047.2	21.444	26.178	2273.6
50	1969.1	3054·1	10.830	13.221	1148.3	100	3938.3	6108.3	21.661	26.443	2296.6

MATHEMATICAL AND *PHILOSOPHICAL CORRESPONDENCE.*

QUESTION VII. *Anſwered by J. F—:—:—:—R.*

THE order in which the firſt 51 cards are diſtributed being of no importance with reſpect to the chance for trumps, the ſolution of this queſtion will be ſomewhat ſimplified by conceiving the hands of the dealer's two adverſaries to conſiſt of the firſt 26 cards dealt off. The chance that theſe may be all of them of the 39, which are not trumps will evidently be, $\frac{39\cdot38\cdot37\cdot36\,..\,\&c.\,....\,14}{51\cdot50\cdot49\cdot48\,..\,\&c.\,....\,26} = \frac{25\cdot24\cdot23\cdot22\,..\,\&c.\,.....\,14}{51\cdot50\cdot49\cdot48\,..\,\&c.\,.....\,40} = \frac{19}{580027} = \frac{1}{30527\frac{14}{19}}$, ſo that the odds againſt it are $30526\frac{14}{19}$ to 1; and yet it is 5200300 times more probable that the dealer and his partner ſhould have the thirteen trumps between them, than that the dealer himſelf ſhould hold them all in his own hand.

De Moivre, in his "Doctrine of Chances;" (2d edit. cor. to prob. xix.) gives a theorem reſpecting the chance of drawing any certain number of black and white counters, which is applicable in ſuch an infinite variety of caſes, that it is perhaps worth mentioning. It may be thus generaliſed:—If a and b be the reſpective numbers of things of two different kinds, heaped promiſcuouſly together, the following formula will expreſs the value of the chance, that out of c of them, taken at hazard from amongſt the whole number $n\,(=a+b)$ there be found the exact number p, and no more, of that kind whoſe whole number is a:

$$\frac{c\,.\,c-1\,.\,c-2\,..\,\&c.\,(p \text{ terms})\times d\,.\,d-1\,.\,d-2\,...\,\&c.\,(a-p \text{ terms})\times\frac{a}{1}\,.\,\frac{a-1}{2}\,.\,\frac{a-2}{3}\,\&c.\,(p \text{ terms})}{n\,.\,n-1\,.\,n-2\,.\,n-3\,..\,\&c.\,(a \text{ terms}).}$$

wherein d is $= n-c$. A proper ſubſtitution in this expreſſion will alſo give us the ſame reſult as above.

QUESTION VIII. *Anſwered by J. F—:—:—:—R.*

LET a and b be the hyperbolic logarithms of a and b. Then will $\mathrm{a}x$ be $= \mathrm{h.l.}\,a^x$ and $1+\mathrm{a}\,.\,x+\frac{\mathrm{a}^2}{2}x^2+\frac{\mathrm{a}^3}{2\,.\,3}x^3+\frac{\mathrm{a}^4}{2\,.\,3\,.\,4}x^4\,....\,\&c. = a^x$; and, in like manner, $1+\mathrm{b}\,.\,x+\frac{\mathrm{b}^2}{2}x^2+\frac{\mathrm{b}^3}{2\,.\,3}x^3+\frac{\mathrm{b}^4}{2\,.\,3\,.\,4}x^4\,....\,\&c. = b^x$; the ſum of which $2+(\mathrm{a}+\mathrm{b})x+\frac{\mathrm{a}^2+\mathrm{b}^2}{2}x^2+\frac{\mathrm{a}^3+\mathrm{b}^3}{2\,.\,3}x^3+\frac{\mathrm{a}^4+\mathrm{b}^4}{2\,.\,3\,.\,4}x^4\,...\,\&c. = c$, by the queſtion. Hence, putting $d = c-2$, and $\alpha, \beta, \gamma, \delta, \varepsilon$, &c = the above co-efficients of the ſeveral powers of x in the latter ſeries, we get $x = \frac{1}{\alpha}d-\frac{\beta}{\alpha^3}d^2+\frac{2\beta^2-\alpha\gamma}{\alpha^5}d^3+\frac{5\,.\,\alpha\beta\gamma-5\beta^3-\alpha^2\delta}{\alpha^7}d^4\,....\,\&c.$ As, however, this ſeries does not, when d is great in reſpect of α, begin to converge till after a conſiderable number of terms, it ſeems better to uſe the common tentative proceſs, repeatedly aſſuming two values of x; and applying the following proportion:—Difference of reſults : difference of aſſumed values : : leaſt error : correction for the neareſt value.—This method, though an indirect one, has certainly great practical advantages in a variety of caſes wherein the expreſſions are ſo entangled with ſurds or unknown exponents as not to be otherwiſe reducible without a great deal of trouble.

Another method of obtaining the value of x is by means of a table of artificial ſines, as follows:—Let r be the logarithmic radius, a, b, and c the common logarithms of *a*, *b*, and *c*, and $d = r - \frac{1}{2}c$. Find an artificial ſine, and its correſpondent coſine, ſ and s, in the tables, ſo that $\frac{ſ - d}{\frac{1}{2}a}$ may be $= \frac{s - d}{\frac{1}{2}b}$, each of which quantities will then be $= x$. The demonſtration becomes obvious by conſidering, that if one of three quantities be equal to the ſum of the other two, their ſquare-roots are the ſides of a right-angled triangle.

Nearly a ſimilar anſwer was given to this queſtion by Analyticus.

NEW MATHEMATICAL QUESTIONS.

Question XI. *By W. Simpson.*

IF H, *b*, be the heights of any two ſignals above the horizon of an obſerver, A, the angle which they ſubtend, and *a*, the ſame angle reduced to the plane of the horizon, then will $\text{coſ. } A = \text{coſ. } a \times \text{coſ. } H \times \text{coſ. } b + \text{ſine } H \times \text{ſine } b$. Required the demonſtration.

Question XII. *By W. C. of Greenwich.*

ON the 1ſt of May 1797, the ſun's declination being 15° 9', it was obſerved that his altitude, azimuth, and the latitude of the place were all equal. Required the hour and place where the obſervation was made.

*** I SHOULD be glad to give a deſcription and drawing of Mr. Varley's machine for producing perpetual motion, as requeſted by Mr. Notlem of Wiſbech, if an attentive peruſal of the ſpecification enrolled in Chancery had ſhewn me any thing tending to improve the theory or practice of mechanics. The deſcription in the periodical work he mentions is not ſufficiently clear to ſhew the whole of what the writer meant to explain, and I found the original equally imperfect. Mr. Varley's notion, obſcured by ſome extraneous and unimportant circumſtances, appears to be, that if an exhauſted cylinder be fixed to one part of the periphery of a wheel, and a piſton fitted therein, the preſſure of the atmoſphere on this laſt, ſuppoſed alſo to be attached to the wheel by a ſpring and chain (parallel to a tangent), will tend to drive it into the vacuum, and, if prevented by the ſhortneſs of the chain, will draw the wheel round. It is obvious to any perſon moderately acquainted with ſtatics, that the preſſures on his wheel muſt counterbalance each other, and cannot produce motion.

It has always been eaſy to ſhew the fallacy of ſchemes for perpetual motion in the particular inſtances; but I have met with no clear enunciation of this project ſo general as to include every poſſible ſcheme, and evince its own abſurdity. The difficulty of performing this ſeems to ariſe from a want of direct and conciſe demonſtrations of the fundamental principle of the lever, and of the equal preſſure of fluids in all directions.

⁎ THE correſpondent who deſires an explanation whence the ſulphate of pot-aſh mentioned in the laſt paragraph of page 218, was derived, is reminded, that this ſalt is the moſt abundant impurity in the common vegetable alkali. Kirwan, in the Iriſh Memoirs for 1789, ſtates its amount in Dantzick pearl-aſh at 505 grains in the pound troy, which is upwards of one ſeventh of the weight of the pure alkali. As Profeſſor Siegling made his liquor of flints *by the ordinary proceſs*, it is not to be ſuppoſed that his alkali had undergone any very laborious purification.

PUBLICATIONS ON GALVANISM.

THE celebrated Aldini, Profeſſor of Natural Philoſophy at Bologna, has forwarded to the firſt claſs of the National Inſtitute a collection of works publiſhed in Italy, ſince the diſcovery made by Galvani, of the irritation produced in animal matters by the metals; and which treat on that ſubject. Theſe works having been hitherto little circulated, Citizen Guyton, from whoſe annonce in the 22d volume of the Annals of Chemiſtry, page 323, this extract is taken, naturally concluded that the liſt would be acceptable to the world. The paper of Mr. Humboldt, of which I have given a tranſlation[*]; another memoir by the ſame author, on the chemical proceſs of vitality, together with certain truly philoſophical remarks upon the ſame by Citizen Fourcroy, all which are inſerted in the 22d volume of the ſame Annals, have ſhewn the great importance of a phenomenon which has engaged the attention of all Europe. The firſt claſs of the Inſtitute of France has conſidered this as an object moſt worthy of inveſtigation, and has for that purpoſe nominated a commiſſion of ſeven members, who for five months paſt have been engaged in experiments proper to determine the circumſtances, and develop its nature.

The following is the liſt. The annexed remarks are made by Citizen Guyton.

Aloyſii Galvani in Bononienſi Archigymnaſio Profeſſoris, etc. de Viribus Electricitatis in Motu muſculari, Commentarius. Cum Joan. Aldini Diſſertatione et Notis. Mutinæ, 1792. Small folio, 80 pages, with three engraved plates.

This work contains a letter of Baſſani Carmineſi, which exhibits the opinion of Volta concerning the ſeat of animal electricity.

Joannis Aldini de Animali Electricitate Diſſertationes duæ. Bononiæ, 1794. Small folio, 41 pages, with figures.

Dell' Uſo e dell' Attività dell' Arco Conduttore nelle Contrazioni dei Muſcoli. In Bologna, 1794. Small quarto, 168 pages. With a Supplement of 23 pages.

Memoria intorno all' Electricità Animale, del Sig. Dott. Gio. Aldini, Prof. di Fiſica nell' Univerſità di Bologna, etc. 10 June, 1794. In quarto, 12 pages.

---

[*] Philoſ. Journ. I. 256.

Prima Brunonis Theoriæ Rudimenta. Bononiæ, 1797. Octavo, 16 pages.

This paper of Aloyſius Zanottus is written in the form of a letter addreſſed to Charles Mundini.

It contains a development of Dr. Brown's opinion, that irritability is one and indiviſible relative to the ſyſtem of the nerves; that is to ſay, that this force is diſtributed through every point of the ſyſtem, in the ſame manner as velocity, impreſſed on any body or ſyſtem of bodies, appertains equally to all the elements of this body or ſyſtem.

Irritants are diſtinguiſhed into extrinſic and intrinſic.

The extrinſic are heat, light, ſound, food, &c. The blood and other animal fluids, though appertaining to the ſyſtem of the animal, appear to act on its economy like extrinſic objects.

The intrinſic irritants are the functions of the ſyſtem, ſuch as the muſcular contraction, the exerciſe of the ſenſes, the action of thought, the movements of the brain, &c.

The common effect of theſe forces is motion. Each poſſeſſes a certain activity called ſtimulus. There cannot be ſuch an agent as a ſedative; for, when the irritants produce weakneſs, this weakneſs muſt be attributed to a defect of the ſtimulating power in degree, &c. &c.

* * *

Fig.5.

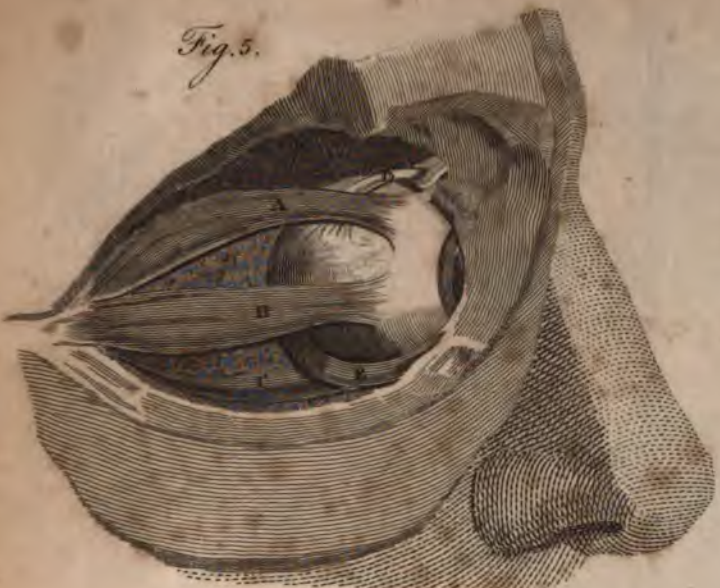


Fig.2.

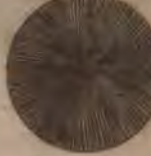
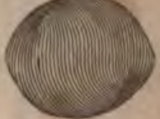
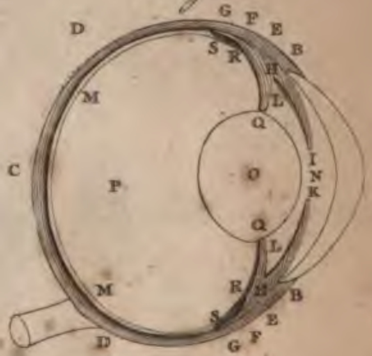


Fig.3.



Focal adjustment of the Eye.

Fig.1.



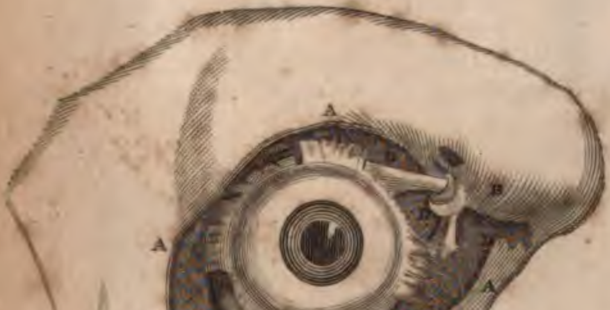


Fig.4.

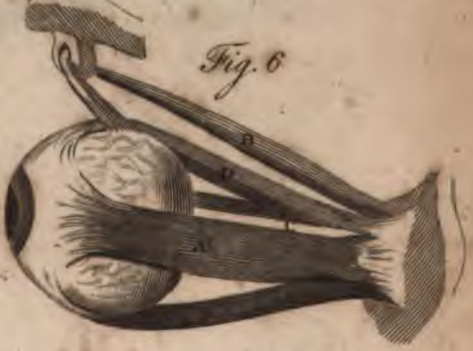
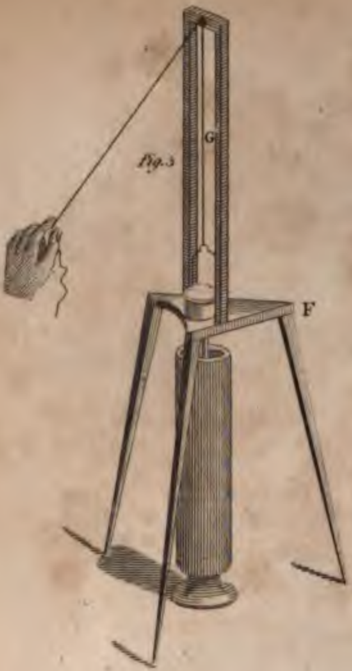
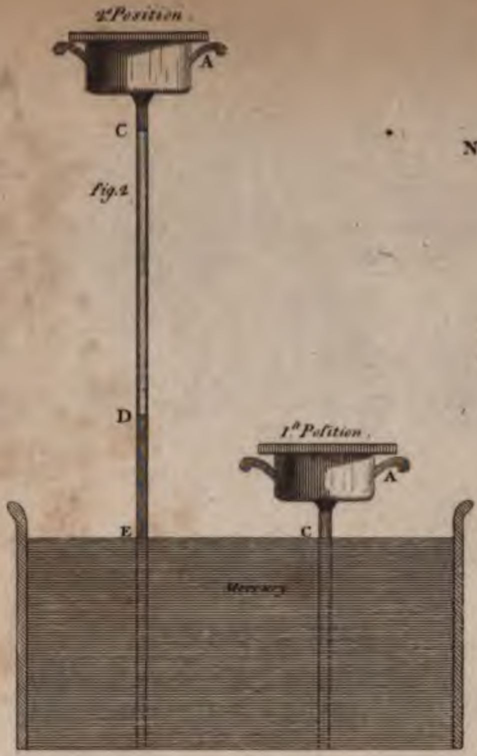
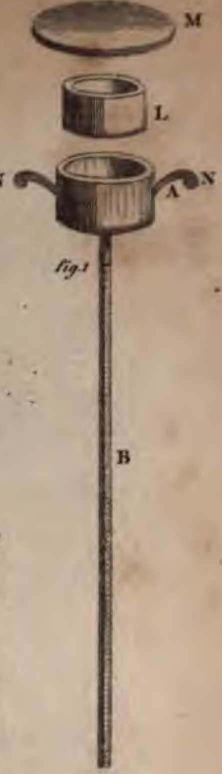


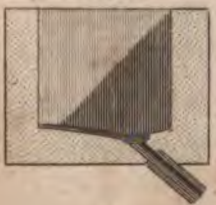
Fig.6

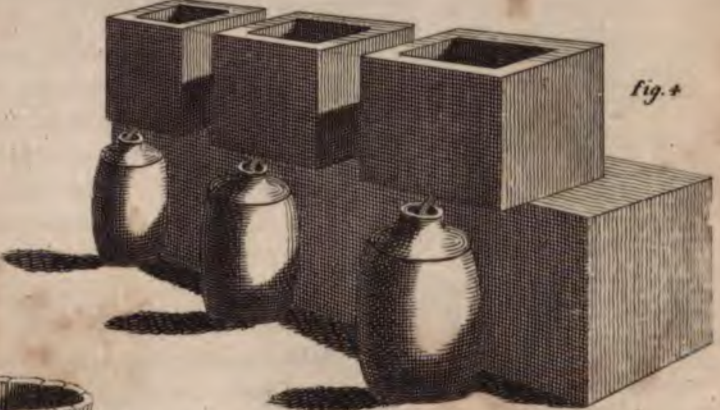


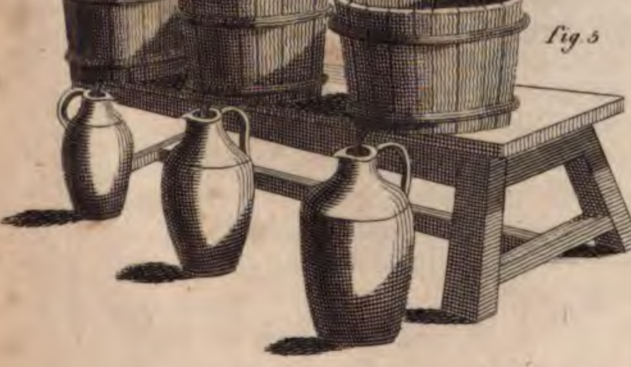




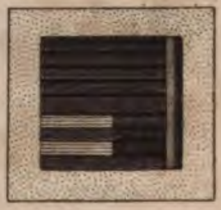
Section







Plan



THE NEW YORK
PUBLIC LIBRARY

ASTOR LENOX AND
TILDEN FOUNDATIONS
R L

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER 1797.

ARTICLE I.

Experiments and Obſervations on the Nature of Sugar. By WILLIAM CRUICKSHANK, *Chemiſt to the Ordnance, and Surgeon of Artillery.*

SUGAR has been ſuppoſed to be a ſubſtance intermediate between mucilages and vegetable acids, containing more oxygene than mucilage, and leſs than the acids. To aſcertain this and ſome other circumſtances, the following experiments were inſtituted:

Two ounces of refined ſugar were introduced into a retort, and expoſed to a heat gradually increaſed until its bottom became red-hot: there came over into the receiver ſeven drachms of a ſharply acid liquor, which required 132 grains of a ſolution of pot-aſh to ſaturate it: this liquor was mixed with a little empyreumatic oil: the charry reſiduum which remained in the retort weighed ſeven drachms; the quantity of gas which eſcaped during the operation muſt therefore have amounted to two drachms: ſome of this was examined, and found to conſiſt of a mixture of carbonic acid gas and hydro-carbonate.

Two ounces of gum-arabic were introduced into a retort at the ſame time, and expoſed to a heat in every reſpect ſimilar: the quantity of acid liquor which came over into the receiver amounted to ſeven drachms and 15 grains; this contained a little more empyreumatic oil, but was not ſo ſharp as that obtained from the ſugar, and required only 117 grains of the ſame ſolution of pot-aſh to ſaturate it: the charry reſiduum which remained in the retort weighed five drachms and 45 grains; the quantity of elaſtic fluid or gas which eſcaped during this proceſs muſt therefore have amounted to three drachms: it conſiſted, like the former, of a mixture of hydro-carbonate and carbonic acid gas; but towards the end of the operation the proportion of hydro-carbonate was more remarkable. From theſe experiments it would appear that ſugar yields by diſtillation more pyromucous acid than gum, in the proportion of 131 to 117. The reſiduary charcoal of the ſugar alſo exceeded that of the gum by 1-7th; but this may in ſome meaſure be accounted for from the greater quantity of the hydro-carbonate yielded by the latter. As oxygene is now allowed to be the univerſal acidifying principle, and as the acid yielded in both inſtances (viz. the pyro-mucous) was exactly of the ſame kind, it may be reaſonably inferred, that the ſugar which afforded the greateſt quantity of acid contained likewiſe the greateſt proportion of oxygene; for it is probable that both the carbonic acid and the hydro-carbonate were formed from the decompoſition of the water by the carbone of theſe ſubſtances, as neither was produced in any quantity until near the end of the operation: the oxygene, therefore, contained in the former ſhould not be conſidered as entering eſſentially into the compoſition of either the gum or ſugar.

It is well known that vegetable mucilages and fæcula are ſomehow converted into ſugar by malting: we conceived, therefore, that it would throw conſiderable light on this ſubject, to obſerve with more attention than had hitherto been done, the particular changes and decompoſitions which take place during this proceſs: it was with this view that the following experiments were made:

December 1, 1796. A quantity of barley, after being ſoaked in water for 24 hours, was put into a wine-glaſs, and introduced into a jar containing common air, and inverted over water: the temperature in this and the following experiments was preſerved between 60 and 70 as nearly as poſſible. At the end of five days it began to grow, and on the 28th the greateſt part had thrown out ſhoots at leaſt half an inch in length. On January 7, vegetation was ſtill going on, and the air in the jar had ſomewhat diminiſhed: the barley being now withdrawn was found to be very ſweet, and nearly converted into the ſtate of malt. The air in the jar was found to conſiſt of azotic and carbonic acid gas in the proportion of 20 to 6, the whole of the oxygene being either abſorbed, or converted into carbonic acid.

January 19, 1797. A quantity of barley, previouſly ſteeped in water for 48 hours, was introduced, as in the laſt experiment, into a jar containing oxygene gas, and inverted over water to which ſulphuric acid had been added. At the end of three days it began to grow, and this proceſs went on to the 29th. The water had now riſen conſiderably in the jar, the gas having ſuffered a diminution of about one-third. The barley being withdrawn ſmelled completely of malt, and taſted ſweet. The gas in the jar, on examination, was found to conſiſt of 64 parts carbonic acid, 32 azote, and 4 oxygene; from which it would appear that the air employed in this experiment had contained originally about 20 per cent. of azotic gas.

To be more certain of the nature of the change which the pure air undergoes in this proceſs, the experiment was repeated as follows:

January 23. A quantity of barley, ſoaked in water for two days, was introduced into a jar containing 46 meaſures of very pure oxygene gas, and inverted over mercury. At the end of three days the barley began to grow, and this proceſs continued for ten days, although very ſlowly: the column of gas remained exactly of the ſame height, ſo that it had undergone no apparent diminution or increaſe: the barley being withdrawn, the air in the jar was examined, and found to conſiſt of carbonic acid gas, mixed with only 1-50th of its bulk of oxygene gas. The barley was partly converted into malt, the quantity of oxygene being inſufficient to produce this change upon the whole.

Another experiment with common air was made at the ſame time, and exactly under ſimilar circumſtances. In this caſe the barley did not begin to grow until the end of the fourth day; and at the end of ten days had made much leſs progreſs than that in the oxygene gas. It was now withdrawn, and the air in the jar, which had increaſed a little, examined; when it was found to conſiſt of carbonic acid and azotic gas, in the proportion of one to two very nearly, mixed with a very ſmall quantity of oxygene gas. A little of the barley taſted ſweet.

Being now ſatisfied that during the evolution of the ſaccharine principle from vegetable mucilage, a quantity of oxygene was either abſorbed or converted into carbonic acid, we wiſhed to know if this proceſs could take place in any degree without the preſence of this gas.

In order to determine this point the following experiments were made:

January 20. A quantity of barley, ſoaked as in the former experiments, was introduced into a jar filled with, and inverted over, mercury. At the expiration of 12 days a very conſiderable quantity of gas was produced, at leaſt five or ſix times the bulk of the barley; but nothing like vegetation was perceivable. The gas, on examination, was found to conſiſt of carbonic acid, being entirely abſorbed by lime-water. The barley had not the leaſt ſweet taſte, nor did it appear to have undergone any ſenſible change.

On January 20th, another portion of the ſame ſoaked barley was introduced into a wine-glaſs, and placed in a jar containing nitrous gas inverted over water. At the expiration of ten days the gas had undergone a ſlight diminution, but there was not the ſmalleſt appearance of vegetation. The barley being withdrawn and examined was found to have undergone no apparent change. The gas contained about one-ninth of its bulk of carbonic acid, the remainder being pure nitrous gas, as was manifeſt from the diminution it underwent when mixed with pure air. The nitrous gas which diſappeared in this inſtance muſt have been abſorbed either by the barley or the water. The carbonic acid which was found mixed with it is accounted for by the laſt experiment.

Two other portions of ſoaked barley were introduced into jars, the one containing hydrogenous and the other azotic gas, and inverted over mercury. At the expiration of 12 or 14 days there was not the leaſt appearance of vegetation in either, but the gas in both had increaſed in bulk about one-fifth. The barley being withdrawn and examined, that in the hydrogenous gas taſted muſty, but not in the leaſt ſweet; the portion in the azote appeared to have undergone no change. The gas in both jars contained from one-third to one-fourth of its bulk of carbonic acid, the remainder being the original gaſes not ſenſibly changed.

From theſe experiments, therefore, it is manifeſt that oxygene is abſolutely neceſſary for the converſion of vegetable mucilage into ſugar; as in no one inſtance was ſaccharine matter formed where this was not preſent, and the quantity of the former was always in proportion to that of the latter; for we found in all the experiments, that when the oxygene was conſumed this proceſs immediately ceaſed.

It may ſtill remain doubtful, whether the oxygene is abſorbed by the barley, or merely converted into carbonic acid: we are inclined to think that it is chiefly abſorbed, although part may alſo be conſumed in the formation of this acid; for we have ſeen that carbonic acid is formed without the preſence of oxygene gas, and that in very conſiderable quantity;

which we conceive muſt proceed from the decompoſition of the water, whoſe oxygene unites with the carbonaceous principle of the barley, whilſt its hydrogene is fixed, and may be neceſſary to the production of the ſaccharine principle. We ſuppoſe, therefore, that vegetable mucilage is converted into ſugar by being deprived of part of its carbone, whilſt at the ſame time it is combined with a greater proportion of oxygene, and probably alſo with hydrogene from the decompoſition of the water. Thus, then, both from analyſis and ſyntheſis, it would appear that ſugar contains more oxygene than gum or mucilage. From this hypotheſis it ſhould follow, that if ſugar be deprived of part of its oxygene, it muſt loſe its ſweetneſs, and form ſomething like a gum. To ſee how far this might be accompliſhed was the object of the following experiments:

A quantity of ſyrup was introduced into a jar, filled with, and inverted over, mercury: to this was admitted about an equal quantity of the phoſphuret of lime: a conſiderable production of phoſphoric gas almoſt immediately took place, and the mercury deſcended in the jar. At the expiration of eight days the ſyrup was withdrawn and examined: it had no ſenſibly ſweet taſte, but rather a bitter aſtringent one: when filtered, alcohol produced a copious white precipitate in flakes, very much reſembling mucilage ſeparated from water by the ſame ſubſtance.

This experiment was ſomewhat varied, as follows: A little refined ſugar was diſſolved in alcohol, and to this ſolution a little phoſphuret of lime was added: no phoſphoric gas was diſengaged, nor was there any apparent action produced. More phoſphuret being added, the mixture was allowed to remain in an open phial for ſeveral days. The alcohol having now evaporated, ſome diſtilled water was added; but this produced no diſengagement of gas, as the phoſphuret had been decompoſed, and converted principally into phoſphate of lime. The mixture being filtered, and the clear liquor evaporated, there remained a ſubſtance extremely tenacious, and which had much the appearance of gum-arabic: its taſte was bitter, with a very ſlight degree of ſweetneſs: when ſqueezed between the teeth it had exactly the feel of gum, but more tenacious. It did not appear to be ſoluble in alcohol, or at leaſt in any conſiderable quantity: when thrown upon a red-hot iron it burned like gum, and left a bulky and inſipid charcoal.

It would appear that the ſaccharine principle had been deſtroyed in theſe experiments, and converted into ſomething reſembling a gum. That this was effected by the abſtraction of oxygene is rendered highly probable from the nature of the ſubſtance employed, and the change which it was found to have undergone; for there are few ſubſtances which have ſo ſtrong a tendency to combine with oxygene as the phoſphuret of lime.

Some other trials of a ſimilar nature were made by mixing ſolutions of ſugar with the different ſulphurets, and by agitating them with nitrous gas in cloſe veſſels. The ſulphurets, more eſpecially that of pot-aſh, manifeſtly deſtroyed the ſaccharine taſte; but on account of the ſolubility of the different products, the particular change produced could not be ſo eaſily and accurately aſcertained. The action of the nitrous gas was more doubtful.

In order to be ſatisfied how far the effects produced on the ſugar in the former experiments might be owing to the abſtraction of oxygene, I added to ſolutions of this ſubſtance in water both lime and pure pot-aſh, and boiled the mixtures for ſome time: the lime appeared manifeſtly to combine with the ſugar, to which it communicated a very bitter aſtringent taſte, but it was ſtill ſweet: a little alcohol, added to the filtered ſolutions, produced a precipitate in white flakes, ſomewhat ſimilar to that in the experiments with the ſulphuret of lime, and which appeared to be a combination of ſugar with lime. Some of the filtered ſolution being evaporated by a gentle heat, there remained a ſemi-tranſparent ſubſtance, much more tenacious than the thickeſt ſyrup, but not equal to that produced by the phoſphuret of lime; and it had a rough bitter taſte, mixed with a certain degree of ſweetneſs. The pot-aſh, likewiſe, appeared to combine with the ſugar, the ſweet taſte being more completely deſtroyed than by the lime: but on the addition of ſulphuric acid, ſulphat of pot-aſh was formed; and this being precipitated by alcohol, the ſweetneſs appeared to be completely reſtored. It may likewiſe be proper to obſerve, that when alcohol was added to a portion of the ſolution of ſugar and pure pot-aſh, after it had been boiled to the conſiſtence of a ſyrup, no union took place; but the alcohol, notwithſtanding the mixture was completely and repeatedly agitated, ſtill ſwam pure on the top: a circumſtance which would ſeem to prove that a new compound is formed by theſe ſubſtances, which is not ſoluble in this fluid, although they are both completely ſo in a ſeparate ſtate.

Having found that ſugar might be converted into a ſpecies of gum by depriving it of part of its oxygene, we conceived that gum might, by the addition of oxygene, be changed into a ſubſtance reſembling ſugar: but although ſeveral trials were made with a view of combinining oxygene in different proportions with gum-arabic, no remarkably ſweet taſte was ever perceived; on the contrary, in every experiment, it ſeemed to run very readily into the acid ſtate, particularly when it was expoſed to the action of the oxygenated muriatic acid gas.

Indeed, when we reflect on the change which vegetable mucilage muſt undergo in the proceſs of malting, the ſimple addition of oxygene does not appear to be ſufficient; for it is probable, from the decompoſition of the water, that ſome of its hydrogene is fixed, whilſt its oxygene diſengages and unites with a certain portion of charcoal, forming the carbonic acid. Although, therefore, ſugar and mucilage conſiſt of the ſame principles, viz. carbone, hydrogene, and oxygene; yet unleſs theſe are combined in certain determinate proportions, the former, which when pure is no doubt always a ſubſtance of exactly the ſame nature, cannot be produced: the hydrogene and carbone muſt be accurately proportioned, as well as the oxygene[*].

II.

An Account of the Manner in which Heat is propagated in Fluids, and its general Conſequences in the Economy of the Univerſe. By BENJAMIN *Count of* RUMFORD.

[Continued from page 296.]

FROM the internal motions of the particles of fluids during the propagation of heat, and from this propagation being impeded by every thing which obſtructs thoſe motions, reſults amounting to a theory of conſiderable perfection were obtained. But the ſubject in the hand of our author naturally produced other intereſting conſequences. It was by the motions of very fine particles of duſt, accidentally mixed with the ſpirits of wine in his large thermometer, and ſtrongly illuminated by the ſun, that he firſt diſcovered the internal motions which take place in that fluid during the time of its cooling. Reflecting on this fact, he immediately concluded that the internal motions of water might be rendered equally viſible if he could find any ſolid body of the ſame ſpecific gravity as water, and not liable to be diſſolved by it; but ſuch a ſubſtance was not to be found. On reflection, it occurred to him, that it is very fortunate that ſuch ſubſtances do not abound; for otherwiſe we ſhould find great difficulty in procuring water in a pure ſtate.

---

[*] This paper is, with the author's conſent, extracted from Dr. Rollo's account of two caſes of Diabetes Mellitus, of which an account is given in this Journal, p. 285.

As the ſolid could not be found, he determined to adapt the ſpecific gravity of the fluid to that of a ſolid very little heavier than the water in its pure ſtate. The ſolid he made choice of was tranſparent yellow amber, of the ſpecific gravity 1,078, and he increaſed the denſity of the water by the addition of a certain quantity of pure alkaline ſalt.

The amber was broken into pieces about the ſize of muſtard-ſeeds, and put into a glaſs veſſel with water, to the bottom of which it ſunk. Upon the gradual addition of an alkaline ſolution ſome of the pieces roſe to the top, and others ſubſided to the bottom. The former were removed, and the fluid was carefully adjuſted with regard to denſity, until the latter, at 60° of Fahrenheit, remained permanently ſuſpended in the different parts of the fluid.

A glaſs veſſel, conſiſting of a globe about two inches in diameter, with a cylindrical neck three quarters of an inch in diameter and twelve inches long, was filled with this prepared liquid. The firſt experiment made with this inſtrument was to plunge it into a tall glaſs jar, nearly filled with water almoſt boiling hot. Two currents, in oppoſite directions, began to move at the ſame inſtant with great celerity in the liquid in the cylindrical tube, the aſcending current occupying the ſides of the tube, while that which moved downwards occupied its axis.

As the ſaline liquor grew warm, the velocity of theſe currents gradually diminiſhed; and at length when the liquor had acquired the temperature of the ſurrounding water in the jar the motion ceaſed entirely. On taking the glaſs body out of the water, the internal motions of the liquor recommenced: but the currents had changed their directions; that which occupied the axis of the tube being now the aſcending current. When the cylindrical tube was inclined a little, the aſcending current occupied the upper ſide, and the deſcending current the lower. Both ceaſed when the inſtrument had acquired the common temperature of the room.

In all caſes when the inſtrument received heat, the current in the axis of the tube, or on its lower ſide if inclined, moved downwards; but when it parted with heat this motion was in the oppoſite direction. A change of a few degrees was ſufficient to ſet the contents of the inſtrument in motion. By the application of an heated body, the fluid neareſt the place of contact was made to aſcend; but the contrary effect took place if the body were colder than the inſtrument. And in either caſe the motion thus produced was attended with a contrary motion in ſome other part of the inſtrument.

Among the applications made by this enlightened philoſopher to an inſtrument ſo calculated to detect the ſecret operations of nature, one conſiſted in applying a lighted candle to the middle of the tube while it was inclined in an angle of about 45 degrees from the perpendicular. In this ſituation the motion of the fluid in the upper part of the tube became exceſſively rapid, while that in the lower end, where it was united to the globe, as well as that in the globe itſelf, remained almoſt perfectly at reſt. He even found that he could make the fluid in the upper part of the tube actually boil, without that in the lower part of it appearing to the hand to be ſenſibly warmed. This fact not only ſerved to ſhew that heat is propagated in fluids chiefly, if not altogether, by virtue of theſe motions which ariſe from change of denſity, but alſo that this tranſportation cannot be effected in a downward direction.

It is an opinion generally received among philoſophers, that water cannot be heated in contact with ice. Our author ſaw that this poſition muſt be in a great meaſure true with regard to water upon which ice floats; becauſe the hot water muſt aſcend and be cooled by cauſing part of the ice to melt, while the cold water from this laſt would deſcend to the bottom, and tend to preſerve the uniformity of temperature. But he ſaw likewiſe that the caſe would be very different if the ice were at the bottom. The inferences which this courſe of argument would point out are ſingular and ſtriking, and they are not leſs ſo for being announced to us under the incontrovertible ſanction of experiment. Natural as the induction is, from the valuable experiments of Count Rumford, it ſtill ſeems in ſome meaſure aſtoniſhing to aſſert, that water may be actually made to boil, and kept in that ſtate without melting a piece of ice, plunged in it, with more rapidity than could be effected by cold water at 40 degrees.

The direct experiments with ice and hot water are related at large in the Eſſay. A circular cake of ice, three inches and an half thick, weighing 10⅛ ounces, and nearly as large as the internal diameter of a cylindrical glaſs jar, which was 4,7 inches (its height being 14 inches), was gently put into the jar containing ſix pounds 1¼ ounce troy of boiling-hot water. It was entirely melted in two minutes and fifty-eight ſeconds.

The ſame experiment was repeated; but inſtead of the ice being ſuffered to ſwim at the ſurface, it was faſtened down in the bottom of the jar, and the hot water poured upon it. The ice was retained in the jar by means of two ſlender and elaſtic pieces of deal about 1-8th of an inch thick, and 1-4th of an inch wide, which, being a very little longer than the internal diameter of the jar, were of courſe ſlightly bended when they were introduced into it horizontally; and on being put down to the ice at right angles to each other, they confined it from riſing to the ſurface when the water was added. A circular piece of ſtrong writing-paper was laid upon the ice to protect it from the action of the boiling-hot water while poured, which was afterwards removed, as gently as poſſible, by means of a ſtring faſtened on one ſide; and to prevent the glaſs jar from being cracked by the ſudden application of the boiling-hot water, a ſmall quantity of cold water was firſt poured in, to cover the ice to the height of about a quarter of an inch. The hot water was poured againſt the middle of the circular piece of paper.

The jar with the ice and the hot water in it being placed on a table near a window, the paper which covered the ſurface of the ice was gently drawn away, and the Count prepared himſelf to obſerve at his eaſe the reſult of this moſt intereſting experiment.

A very few moments were ſufficient to ſhew that his expectation with regard to it would not be diſappointed. In the former experiment a ſimilar cake of ice had been entirely melted in leſs than three minutes; but in this, after more than twice that time had elapſed, the ice did not ſhew any apparent ſigns of beginning to melt. Its ſurface remained ſmooth and ſhining, and the water immediately in contact with it appeared to be perfectly at reſt;

though the internal motions of the hot water above it, which was giving off its heat to the ſides of the jar and to the air, were very rapid, as was diſtinctly perceived by means of ſome earthy particles or other impurities which this water happened to contain.

The ice was examined with a very good lens, but it was a long time before any ſigns of its melting could be perceived. The edges of the cake remained ſharp; and the minute particles of duſt, which by degrees were precipitated by the hot water as it grew colder, remained motionleſs as ſoon as they touched the ſurface of the ice.

As the hot water had been brought from the kitchen in a tea-kettle, it was not quite boiling-hot when it was poured into the jar. After it had been in the jar one minute, a thermometer was plunged into it, and its temperature found to be at 180.

After 12 minutes had elapſed, its temperature at the depth of one inch under the ſurface was 170°. At the depth of ſeven inches, or one inch above the ſurface of the ice, it was at $169\frac{1}{10}$; while at only 3-4ths of an inch lower, or 1-4th above the ſurface of the ice, its temperature was 40°.

When 20 minutes had elapſed, the heat in the water at different depths was found to be as follows:

	Degrees.
Immediately above the ſurface of the ice	40
At the diſtance of half an inch above it	46
At 1 Inch	130
At 3 Inches	159
At 7 Inches	160

When 35 minutes had elapſed, the heat was as follows:

At the ſurface of the ice	40
Half an Inch above it	76
1 Inch above it	110
2 Inches	144
3 Inches	148
5 Inches	$148\frac{1}{2}$
7 Inches	149

At the end of one hour the heat was as follows:

At the ſurface of the ice	40
1 Inch above it	80
2 Inches	118
3 Inches	128
4 Inches	130
7 Inches	131

After one hour and 15 minutes had elapſed, the heat was found to be as follows:

	Degrees.
At the ſurface of the ice	40
1 Inch above it	82
2 Inches	106
3 Inches	123

The heat of the water had hitherto been taken near the ſide of the jar:—in the two following trials it was meaſured in the middle or axis of the jar.

When one hour and 30 minutes (reckoning always from the time when the boiling water was poured into the jar) had elapſed, the heat of the water in the middle of the jar was found to be as follows:

	Degrees.
At the ſurface of the ice	40
1 Inch above it	84
2 Inches	115
3 Inches	116
7 Inches	117

When two hours had elapſed, the heat in the middle of the jar was found to be as follows;

	Degrees.
At the ſurface of the ice	40
1 Inch above it	76
2 Inches	94
3 Inches	106
4 Inches	108
6 Inches	108¼
7 Inches	108½

An end being now put to the experiment, the hot water was poured off from the ice; and on weighing that which remained, it was found that five ounces ſix grains troy (= 2406 grains) of ice had been melted.

Taking the mean temperature of the water at the end of the experiment at 106°, our author remarks that the maſs of hot water (which weighed 73¼ ounces) was cooled 78 degrees, or from the temperature of 184° to that of 106° during the experiment. Now as it is known that one ounce of ice abſorbs juſt as much heat in being changed to water as one ounce of water loſes in being cooled 140 degrees, it is evident that one ounce of water which is cooled 78 degrees, gives off as much heat as would be ſufficient to melt $\frac{78}{140}$ths of an ounce of ice; conſequently the 73¼ ounces of hot water which in this experiment were cooled 78 degrees, actually gave off as much heat as would have been ſufficient to have melted $\frac{73\frac{1}{4} \times 78}{40} = 40\frac{8}{10}$ ounces of ice.

But the quantity of ice actually melted was only about five ounces; and hence it appears that leſs than one-eighth part of the heat loſt by the water was communicated to the ice, the reſt being carried off by the air.

As the ſame quantity of hot water was uſed in this experiment and in that which immediately preceded it, and as this water was contained by the ſame veſſel, it appears that ice melts more than eighty times ſlower at the bottom of a maſs of boiling-hot water than when it is ſuffered to ſwim on its ſurface. For as in the one experiment 10⅛ ounces of ice were melted in two minutes and 58 ſeconds, five ounces at leaſt muſt have been melted in one minute and 29 ſeconds; but in the other experiments two hours or 120 minutes were employed in melting five ounces.

The ice however was melted, though very ſlowly, at the bottom of the hot water; and that circumſtance alone would have been ſufficient to have overturned the hypotheſis reſpecting the propagation of heat in fluids ſolely by means of the inteſtine motion. This conſideration demanded an attentive enquiry into the circumſtances of the experiment. One of the moſt ſtriking among theſe was, that at the end of the firſt half-hour, on examining the ſurface of the ice it was ſeen that it had been melted, excepting only where it had been covered, or as it were ſhadowed, by the flat ſlips of deal which ſecured the ice in its place. It was ſingular that the ice was defended, not only by the undermoſt piece of wood, but by the other, which, lying acroſs the under piece, did not touch the ice any where except at its ends. It was natural to imagine, from this event, that the ice had been melted by radiant heat or calorific rays from the water, and that a portion of theſe had been intercepted by the ſlips of deal. This ſuppoſition pointed out another courſe of experimental enquiry.

Into a cylindrical glaſs jar, 6½ inches in diameter, and eight inches high, was put a circular cake of ice, as long as could be made to enter the jar, and about 3½ inches thick; and on the flat and even ſurface of the ice was placed a circular plate of the thinneſt tin that could be procured, near 6½ inches in diameter, or ſufficiently large juſt to cover the ice. This plate of tin (which, to preſerve its form or keep it quite flat, was ſtrengthened by a ſtrong wire, which went round it at its circumference) had a circular hole in its centre juſt two inches in diameter; and it was firmly fixed down on the upper ſurface of the cake of ice, by means of ſeveral thin wooden wedges which paſſed between its circumference and the ſides of the jar.

A ſecond circular plate of tin, with a circular hole in its centre, two inches in diameter, and in all other reſpects exactly like that already deſcribed, was now placed over the firſt, and parallel to it, at the diſtance of juſt one inch, and, like the firſt, was firmly fixed in its place by wooden wedges.

Theſe perforated circular plates being fixed in their places, the jar was placed in a room where Fahrenheit's thermometer ſtood at 34°, and ice-cold water was poured into it, till the water juſt covered the upper plate; and then the jar was filled to within half an inch of its brim with boiling water, and, being covered over with a board, was ſuffered to remain quite two hours.

At the end of this time, the water, which was ſtill warm, was poured off, and, the circular plate being removed, the ice was examined.

A circular excavation, juſt as large as the hole in the tin plate which covered the ice (namely, two inches in diameter), and correſponding with it, perfectly well defined, and about 2-10ths of an inch deep in the centre, had been made in the ice.

This was what our author expected to find; but there was ſomething more which he did not expect, and which for ſome time he was quite at a loſs to account for. Every part of the ſurface of the ice which had been covered by the tin plate appeared to be perfect, level, and ſmooth, and ſhewed no ſigns of its having been melted or diminiſhed, excepting only in one place, where a channel about an inch wide, and a little more than 2-10ths of an inch deep, which ſhewed evident marks of having been formed by a ſtream of warm water, led from the excavation juſt mentioned in the centre of the upper part of the cake of ice to its circumference. As the edge or vertical ſide of the cake of ice was evidently worn away where this ſtream paſſed, there could be no doubt with reſpect to its direction.

It certainly ran out of the circular excavation in the middle of the ice; and though it might at firſt appear difficult to explain the fact, and to ſhew how this hot water could arrive at that place; yet it was quite evident that the immediate cauſe of the motion of this ſtream of water could be no other than its ſpecific gravity being greater than that of the reſt of the water at the ſame depth; and that this greater ſpecific gravity was at the ſame time accompanied by a higher degree of heat, is evident from the deep channel which this ſtream had melted in the ice, while other parts of the ſurface of the ice at the ſame level were not melted by the water which reſted on it.

This experiment, though attended with remarkable circumſtances, did not clearly ſhew that the fuſion of the uncovered part of the ice had been effected by radiant heat. It occurred to the author, that radiant heat, like light, might probably be reflected downwards in part from the internal ſurface of the water; and conſequently, that it might be expected that a light black body, namely, a circular piece of deal board, covered over with black ſilk, would abſorb a portion of the whole body of rays which were directed to the ice. This trial was made, but with no ſenſible effect.

As it is uncertain whether heat in the radiant ſtate can be reflected in this manner, the experiment may perhaps be conſidered as likely to afford a leſs equivocal concluſion than might have been obtained from the application of a poliſhed metallic ſurface to the ſuperior termination of the cylinder of water. But the appearance of a channel worn in the plate of ice in the preceding experiment gave riſe to meditations which clearly pointed out the manner in which the ice was melted, and rendered the attempts to detect the ſuppoſed operation of latent heat unneceſſary. The Count's explanation, in his own words, is as follows:

Though it is one of the moſt general laws of nature with which we are acquainted, that all bodies, ſolids as well as fluids, are condenſed by cold, yet, in regard to water, there appears to be a very remarkable exception to this law. Water, like all other known bodies, is indeed condenſed by cold at every degree of temperature which is conſiderably higher than that of freezing; but its condenſation, on parting with heat, does not go on till it is changed to ice; but when in cooling its temperature has reached to 40 degrees of Fahrenheit's ſcale, or eight degrees above freezing, it ceaſes to be farther condenſed; and on being cooled ſtill farther it actually expands, and continues to expand as it goes on to loſe more of its heat, till at laſt it freezes; and at the moment when it becomes ſolid, and even after it has become ſolid, it expands ſtill more on growing colder. This fact, which is[*] noticed by M. de Luc, in his excellent Treatiſe on the Modifications of the Atmoſphere, has ſince been farther inveſtigated and put beyond all doubt by Sir Charles Blagden. (See Philoſophical Tranſactions, vol. lxxviii.)

Now, as water in contact with melting ice is always at the temperature of 32°, it is evident that water at that temperature muſt be ſpecifically lighter than water which is eight degrees warmer, or at the temperature of 40°: conſequently, if two parcels of water at theſe two temperatures be contained in the ſame veſſel, that which is the coldeſt and lighteſt muſt neceſſarily give place to that which is warmer and heavier, and currents of the warmer water will deſcend in that which is colder.

---

[*] The ſimple fact was alſo obſerved by the celebrated Robert Boyle. N.

In the two laſt experiments, as the circular tin plate which covered the ſurface of the ice ſerved to confine the thin ſheet of water which was between the plate and the ice—as this water could not riſe upwards, being hindered by the plate—and as it had no tendency to deſcend—it is probable that it remained in its place; and as it was ice-cold, it was not capable of melting the ice on which it repoſed. But as the tin plate had a circular hole in its centre, the ſurface of the ice in that part was of courſe naked; and the ice-cold water in contact with it being diſplaced by the warmer and heavier water from above, an excavation in the form of a ſhallow baſin was formed in the ice by this deſcending warm current.

The warm water contained in this baſin overflowed its banks as ſoon as the baſin began to be formed; and iſſuing out on that ſide which happened to be the loweſt, opened itſelf a paſſage under the tin plate to the edge of the ice, over which it was precipitated, and fell down to the bottom of the jar. The water of this rivulet being warm, it ſoon formed for itſelf a deep channel in the ice; and at the end of the experiment it was found to be everywhere deeper than the bottom of the baſin where it took its riſe.

This manner of accounting for the appearances in queſtion ſeemed to the Count to be quite ſatisfactory; and the more he meditated on the ſubject, the more he was confirmed in his ſuſpicions that all liquids muſt neceſſarily be perfect non-conductors of heat.

On theſe principles he was now enabled to account for the melting of the ice at the bottom of the hot water, as alſo for the flowneſs with which that proceſs went on; and encouraged by this ſucceſs he proceeded with confidence to plan and to execute ſtill more deciſive experiments; from the reſults of which he conſiders the important facts in queſtion to have been put beyond all poſſibility of doubt.

If water be in fact a perfect non-conductor of heat, that is to ſay, if there be no communication whatever of heat between neighbouring particles or molecules of that fluid (which is what he ſuppoſes); then, as heat cannot be propagated in it, but only in conſequence of the motions occaſioned in the fluid by the changes in the ſpecific gravity of thoſe particles which are occaſioned by the changes of their temperature, it follows that heat cannot be propagated downwards in water, as long as that fluid continues to be condenſed with cold; and that it is only in that direction (downwards) that it can be propagated after the water has arrived at that temperature where it begins to be expanded by cold; which has been found to be at about the 40th degree of Fahrenheit's ſcale.

Reaſoning on theſe principles, he was led to this remarkable concluſion: namely, that *water which is only eight degrees above the freezing point, or at the temperature of* 40 *degrees, muſt be able to melt as much ice in any given time, when ſtanding on its ſurface, as an equal volume of water at any higher temperature, even though it were boiling-hot.*

The experiments by which this unexpected reſult was confirmed and eſtabliſhed muſt be deferred to the concluding part of this abſtract.

III.

Experiments and Obſervations made with the View of aſcertaining the Nature of the Gaz produced by paſſing Electric Diſcharges through Water; with a Deſcription of the Apparatus for theſe Experiments. By GEORGE PEARSON, *M. D. F. R. S.*

[Concluded from page 248.]

THE fire of the electric diſcharge, in a very condenſed ſtate, paſſes with inconceivable velocity through the whole length of the upper wire, in the caſe of the interrupted exploſion, p. 243—247, and of the ſingle wire in the caſe of the complete exploſion, p 247, 248; ſo that it neither exerts its energy on the wire, nor on the water, till it arrives at the extremity of the wire. There it is momentarily interrupted and accumulated; and, in the moment before its diffuſion through the water, it is ſo denſe and in ſuch quantity as to manifeſt itſelf by a ſpark at, or nearly in contact with, the extremity of the wire. In the moment of its diffuſion, a ſmall part of this condenſed fire interpoſes betwixt the conſtituent elements of the ultimate and inviſible particles of water, that is, betwixt the hydrogen and oxygen, of which water is compounded, ſo as to place them beyond the ſphere of their chemical attraction for one another; and each ultimate particle of hydrogen and of oxygen uniting with a determinate quantity of fire, new compound ultimate particles, conſiſting of hydrogen and caloric, and of oxygen and caloric, that is, hydrogen gaz and oxygen gaz, are compounded. This mode of action of electric fire on water is confirmed by the effect of electric fire and common fire, of a due degree of denſity, on oxide of quickſilver, which is by them reſolved into oxygen gaz, and quickſilver in the vapour ſtate. All calculation muſt needs be extremely vague; yet, perhaps, ſome elucidation will be obtained by conſidering that as it probably requires 70 or 80 thouſand diſcharges to produce a cubic inch of gaz from the ſuppoſed decompounded water, the gaz produced by each diſcharge cannot amount to one 200,000dth of a grain weight. The quantity of condenſed fire at the extremity of the wire muſt be immenſely great, comparatively with the quantity of it which enters into the compoſition of the gazes from decompounded water; otherwiſe it is not eaſy to conceive how even the minuteſt particle of water could be decompounded, the electric fire being in contact with a large body of water, and paſſing through it with a velocity which is incalculable.

The reaſon of the metal wire not uniting to the oxygen of the decompounded water, as in the experiment of paſſing water through red-hot iron tubes, might be aſſigned from the intenſity of the fire only; but it is alſo on account of the rapid motion of the diſcharge, as well as, partly, from the great quantity of light. In a very low temperature, light decompounds oxide of ſilver, and of ſeveral other metals; alſo oxy-muriatic acid, nitric acid, &c. Hence light both decompounds bodies, and prevents oxygen from coming within the ſphere of chemical attraction of the metal.

It is ſuppoſed that but a very minute proportion indeed, of the electric diſcharge, is conſumed at the end of the wire in the compoſition of gazes, during a momentary interruption, as above ſaid; for it diffuſes itſelf through the water, between the wires, yielding a volume of vivid light, till it arrives at the extremity of the under wire or point of the convexity of the metal cup, where it is again condenſed by the ſuperior conducting power of the metal to that of water, and where, if it be in due quantity, and of ſufficient denſity, it manifeſts itſelf by a ſpark, and infallibly again decompounds water: hence bubbles are ſeen to riſe from the end of the lower wire, and from the metal cup, as well as from the point of the upper wire.

From theſe interpretations it will not be difficult to explain the reaſon of a ſpark appearing in ſome caſes at the points of both the upper and under wire; why in other caſes it appears at the point of the upper wire only; why in others it appears at the point of the lower wire only; namely, according to the denſity of the fire of the diſcharge there accumulated.

Concerning the agency of electrical fire in cauſing the hydrogen and oxygen gaz of the ſuppoſed decompounded water to undergo combuſtion and produce water, it is well known that the ſmalleſt viſible ſpark, or particle of flame, or fire, can kindle as rapidly a very large quantity of hydrogen and oxygen gaz, as the greateſt quantity of flame, ſparks, or fire, can kindle the ſmalleſt quantity of theſe two gazes; while, on the other hand, the largeſt maſs of matter, heated moſt intenſely, but ſhort of ignition, cannot produce combuſtion of oxygen and hydrogen gaz.

Although, as hath been above explained, caloric and light, in a ſufficiently denſe ſtate, may decompound every compound ſubſtance in nature; it is alſo well aſcertained, that caloric, in certain ſtates, univerſally promotes chemical combinations. The mode of agency, in this latter caſe, I apprehend to be in a different way from that commonly accepted; for I think it is unneceſſary to ſuppoſe that it operates by increaſing the power of chemical attraction, and I conceive its agency to be merely diminiſhing or deſtroying the powers which counteract chemical union; eſpecially diminiſhing coheſive attraction, and exciting motion among the particles of the different ſubſtances: hence theſe ſubſtances are applied to one another, within their ſpheres of chemical attraction; or the chemical attraction acts between a greater number of points of the different bodies, as when caloric renders ſolids and inelaſtic fluids into the elaſtic fluid ſtate.

Accordingly, when an electric ſpark, or the ſmalleſt particle of flame, or of an ignited ſubſtance, is applied to the gaz produced in the above proceſs, or to the mixture of hydrogen and oxygen gaz, the ultimate particles of theſe gazes neareſt to the flame are driven from it in all directions, as from a centre, by the interpoſition of fire, or of caloric and light; ſo that they are brought within the ſphere of their chemical attraction for the ultimate particles of the gazes at a certain diſtance from the centre of application of fire; which therefore unite, and the caloric and light, diſengaged by that union, act in a ſimilar manner in producing union among the next ſet in order of proximity, of the ultimate particles of the gazes; the diſengaged caloric and light of which act in producing union of the next ſet of ultimate particles in order; and ſo on ſucceſſively, but with incalculable velocity, the greateſt bulk of ultimate particles of the two gazes unite with one another; the known products of which union are fire and water; or light, caloric, and water. According to this hypotheſis, if caloric or fire, or merely caloric of ſufficient intenſity and quantity, be applied to a given bulk of hydrogen and oxygen gaz, no combuſtion ſhould be produced; as the caloric will be interpoſed in ſuch quantity that all the ultimate particles muſt be at the ſame inſtant driven from one another in all directions, ſo that they are beyond their ſpheres of chemical attraction for one another: and in various inſtances common experience verifies this hypotheſis.

If light be conſidered as a different ſpecies of ſubſtance from caloric, then the theory of its agency, lately publiſhed by Dr. Parr, M. D. may be applied very happily to explain the exploſion from the combuſtion of oxygen with hydrogen gaz. From a very large induction of facts, Dr. Parr infers that, although light and caloric ſubſiſt together very commonly in the ſame compound ſubſtance, they ſimultaneouſly repel each other on the decompoſition of the ſubſtances with which they were united; and from this repulſion, and alſo the repulſion of oxygen and light of one another, he chiefly accounts for the combuſtion of oxygen gaz.

On my principles above ſtated, we can explain why quickſilver of the temperature of 1000°, or more, of Fahrenheit's ſcale, cannot unite to the oxygen of oxygen gaz; but why at this temperature caloric ſeparates oxygen in the gaz ſtate from oxide of quickſilver: and why at the temperature of between 600 and 1000° quickſilver does unite to the oxygen of oxygen gaz; but at which temperature oxygen gaz is not ſeparable from the oxide of this metal: and again, why at below 600° oxygen of oxygen gaz can neither unite to quickſilver, nor be ſeparated from its oxide.

Theſe principles may be applied variouſly to the interpretation of the phenomena of combuſtion, and other caſes of chemical combination, according to the ſtate of aggregation of the ſubſtances which have a chemical attraction for one another. For example; the oxygen of oxygen gaz cannot unite to the conſtituent ſubſtances of a wax or tallow candle, in a low temperature; becauſe the coheſive attraction of the wax or tallow, as well as the chemical attraction between their conſtituent ſubſtances—hydrogen and carbon—counteract the chemical attraction between the oxygen of oxygen gaz, and the ultimate particles of hydrogen and carbon of the candle. But at a pretty elevated temperature, when the wax and tallow are in the vapour ſtate, the coheſive attraction no longer ſubſiſting among the ultimate particles of theſe ſubſtances, and the motion excited by the ignited portion of wax or tallow bringing the ultimate particles of theſe ſubſtances within the ſphere of chemical attraction of the particles of oxygen gaz, theſe latter unite firſt to the carbon, by virtue of the ſtronger attraction between the oxygen and carbon, than that between the oxygen and hydrogen; but, in the inſtant of this diſengagement of the carbon from the hydrogen, a portion of this hydrogen unites alſo to oxygen, and thus not only carbonic acid gaz but water is produced: hence the *blue flame*, as appears on other occaſions, in which there is combuſtion of hydro-carbonate gaz. The remainder of the hydrogen of the decompounded wax or tallow, aſcending in the gaz ſtate, it unites to the oxygen of oxygen gaz; and, as in other caſes of combuſtion of oxygen gaz with hydrogen, a *white* or *ſtraw-coloured flame* is produced. The wick anſwers the purpoſe, by means of capillary attraction, of applying the wax and tallow in ſuch quantities as can be decompounded and combine with the oxygen of atmoſpherical air; hence the combuſtion is gradual and equal. The wick itſelf contains hydrogen and carbon; hence, in combining with oxygen of oxygen gaz, it alſo produces a *blue flame*. Hydro-carbonate gaz being ſpecifically heavier than hydrogen gaz, is another reaſon for the blue flame appearing diſtinct from the white, and at the inferior part of the *fruſtum* of a cone of flame from a burning candle. It is ſcarcely neceſſary to ſay that hydrogen and carbon are conſtituent ſubſtances of wax and tallow;

and that when they combine with oxygen, as above explained, the products are water and hydro-carbonate gaz.

If I did not uſe the term *demonſtration* in a more ſtrict and preciſe ſenſe than is uſual, except in mathematics, I would venture to affirm that this theory is almoſt demonſtrated by the agency of fire and carbon on black oxide of manganeſe. If colourleſs vitrifiable matter, and that oxide be melted together, by means of the *white* part of the flame of a candle, purple-coloured glaſs will be produced; for then the hydrogen of this part of the flame carries off little or none of the oxygen from the oxide: but if theſe ſubſtances be melted together by the *blue* part of the flame, colourleſs glaſs will be produced; for then the carbon of this part of the flame carries off oxygen from the black oxide, and produces white oxide. By combining oxygen anew with colourleſs glaſs, containing manganeſe, it will become purple; and this is effected by melting ſuch colourleſs glaſs with a little nitrate of pot-aſh, or by melting it in open veſſels by the yellow flame of a candle. By ſeparating oxygen from glaſs rendered purple by manganeſe, colourleſs glaſs is produced; and this is effected by melting ſuch coloured glaſs with a little carbon, or by the blue flame of a candle. I have not thought it neceſſary to diſtinguiſh between the indigo and violet rays; or to notice the extremely thin film of violet flame, which an attentive obſerver may perceive ſurrounding the inferior part of the white, extending as high but ſcarcely higher than the wick; becauſe the explanation is perfectly obvious from the preceding diſtinction of the two kinds of gazes afforded by the candle. If I have explained more ſatisfactorily than former members of this ſociety the above phenomena of combuſtion, I owe this advantage to ſome experiments of combuſtion of inflammable gazes which I have made for ſome years paſt, in my chemical lectures; by which the colours are ſhewn to be very different, and correſpond to the above theory. I apprehend the theory of an ingenious member of this ſociety cannot explain adequately the phenomena, and does not appear ſupported by any facts: for there is no evidence that the white flame is not equally the effect of immediate decompoſition as well as the blue; or that the blue becomes white flame by ignition; and I preſume that the experiments which I have mentioned ſhew that theſe differences depend upon different decompounding ſubſtances contained in the candle.

With regard to the evidence afforded by the foregoing experiments concerning the compoſition of water and of hydrogen and oxygen gaz. Theſe ſubſtances are now accounted for in two ways only; namely, 1. By ſaying that theſe two gazes conſiſt of water and imponderable matter; and that during combuſtion the water is precipitated. 2. By ſaying that the two gazes conſiſt of a peculiar baſis, one of which is named oxygen and the other is hydrogen, each of which is rendered into the gaz ſtate by uniting to caloric, and perhaps alſo to light; and that during combuſtion theſe baſes unite with one another, thus compounding water and diſcharging caloric and light. If complete demonſtration could be given, there would not be two opinions; for its proofs, if underſtood, command univerſal aſſent: but the caſe being otherwiſe, that opinion muſt be adopted on the ſide of which the evidence preponderates according to the laws of reaſoning in phyſical ſcience. Now with regard to the former of theſe opinions, I can perceive but two facts in ſupport of it. The firſt of theſe is ſuppoſed to afford a ſort of ſynthetical proof: it is the inſtance of water being required to obtain, by fire, the whole of the carbonic acid from carbonate of baryt. Here the fact, if admitted, is true only of carbonic acid; but even in this caſe it has not been ſhewn that the water enters into the compoſition of carbonic acid; and until that is proved, it is warrantable to ſuppoſe that the water ſerves to diſengage the carbonic acid, by attracting and uniting with the baryt; eſpecially as this acid gaz may be wholly ſeparated from other ſubſtances without the intermediation of water, and as it can be compounded by uniting the drieſt carbon to the drieſt oxygen gaz. Further, although carbonic acid gaz be very apt to contain water, it may be obtained in ſo dry a ſtate as not to indicate moiſture to the moſt delicate teſts.

The next fact affords a ſort of analytical proof; which is the exiſtence of water in air or gaz in general, as ſhewn by muriate of lime, acetite of pot-aſh, ſulphuric acid, quicklime, pot-aſh, &c. But the abſorption of water by theſe ſubſtances only ſhews that it may be ſuſpended or diſſolved in gazes; but not that it enters into their compoſition. Gaz may be obtained or rendered quite free from water, as juſt ſaid; and as the compounds by the combination of the gazes with various different ſubſtances are quite different from the compounds by the combination of water with the ſame various different ſubſtances, there does not appear to be in this caſe any admiſſible evidence of water being an eſſential conſtituent ſubſtance in the compoſition of gazes.

The experiments of ſeparating oxygen and nitrogen gaz from water by boiling, and by expoſing it in vacuo and applying caloric, only ſhew how much more tenaciouſly, and in how much greater quantity, theſe gazes are contained in water than is commonly ſuppoſed; for however long a given quantity of water yielded gaz, ſo that there ſeemed no limit to the quantity, the water which afforded it was ſtill very conſiderably greater than the weight or quantity of gaz.

With reſpect to the other opinion, that hydrogen and oxygen gaz are compounded of a peculiar baſis and caloric, and that water is compounded of theſe two baſes—1. It has been ſhewn by an experiment, the accuracy of which is not queſtionable, that when theſe two gazes produce water by combuſtion, the amount of the weight of the water is exactly the weight of the gazes conſumed, and that no ponderable matter but water is produced when nothing but the baſes of theſe two gazes unite; but that when other ſubſtances, ſuch as nitrous acid and carbonic acid, were produced, as well as water, then nitrogen and carbon were preſent. 2. It has been ſhewn, that when oxygen in a concrete ſtate, as in metallic oxides, unites to the hydrogen of hydrogen gaz, the water thus compounded is equal in weight to that of the hydrogen gaz conſumed, and the deficient weight of the oxide. 3. It appears that when hydrogen in a concrete ſtate, as in alcohol, is united to the oxygen of oxygen gaz, the water compounded is equal in weight to the weight of the alcohol and oxygen gaz conſumed, provided the addition be made of the carbonic acid alſo produced by the carbon of the alcohol. The carbon of alcohol can be ſhewn by analyſis; and its exiſtence in carbonic acid can be ſhewn both by analyſis and ſyntheſis. 4. When water is applied to certain ſubſtances, nothing but hydrogen gaz is obtained, and new compounds, the conſtituent parts of which are oxygen and the ſubſtance applied; for theſe new compounds have the ſame properties as the compounds of the ſame ſubſtances ſo applied to water, and of the oxygen of oxygen gaz. And in theſe caſes the additional weight derived from the water, together with the weight of the hydrogen gaz ſeparated from the water, is equal to the water deſtroyed. 5. It has been rendered at leaſt very probable, that when oxygen and hydrogen are rendered into the gaz ſtate, they abſorb or unite with a large quantity of caloric, or of both caloric and light. I might add, that beſides the poſitive evidence juſt ſtated, there does not appear to be any evidence to contravene the concluſion that water is compounded of hydrogen and oxygen; and that theſe gazes are compounded of a peculiar baſis, called hydrogen and oxygen, united to caloric: for it would be eaſy to ſhew, but it would be digreſſing too far, that the remaining partizans of the phlogiſtic ſect have advanced erroneous evidence in ſome caſes, and in others have neglected the conſideration of oppoſing but well authenticated evidence.

In the experiments contained in this paper no ſupport can be found for the former opinion concerning water, and hydrogen and oxygen gaz; but they confirm the latter opinion. For, 1. the combuſtion of theſe gazes, rendered perfectly dry, afforded water, p. 303, Exp. IV. 2. The evidence from the proceſs above deſcribed is peculiar; for no *one* other proceſs affords oxygen and hydrogen gaz from water. In all other proceſſes for decompounding water, either the decompounding concrete ſubſtance receives ſomething from the water applied, and at the ſame time hydrogen gaz is produced; as in the inſtance of paſſing water through a red-hot iron tube: or the decompounding concrete ſubſtance receives ſomething from the water; and this compound unites to that in water, which produces hydrogen gaz with caloric; as in the inſtance of applying water to red-hot carbon: or two decompounding ſubſtances being applied, each of them receives a different thing from the water, namely, hydrogen and oxygen. Hence, by the proceſs above deſcribed, the objection is removed, that the hydrogen gaz might be ſeparated from the decompounding ſubſtance itſelf, by water taking its place. 3. The production of hydrogen and oxygen gaz, and the production of water by the combuſtion of them, in the above proceſs, afford an additional evidence of the decompoſition and compoſition of water, and of hydrogen and oxygen gaz; as the mode of their production by this proceſs is perfectly conſiſtent with the rationale of their production in all other caſes.

Thus it appears that the grandeſt diſcovery ever made in chemical philoſophy, by an illuſtrious member of this Society, in 1781, has been confirmed by a number of ſubſequent experiments. The body of evidence is indeed ſo numerous, and of ſuch a nature, that, in the minds of thoſe who underſtand its import, and who rely on the accuracy of the weights and meaſures employed, it produces as much conviction concerning the compoſition of water as can be obtained by the evidence of almoſt any other caſe of compoſition. I muſt, however, beg leave to proteſt againſt thoſe able philoſophers, who have maintained, that the compoſition of water, and ſeveral gazes, has received full and complete demonſtration; by ſuch unwarrantable pretenſions their adverſaries have obtained over them ſome advantages. For in the chain of cauſes and effects there are ſome links which cannot be explained by the direct evidence of ſenſe; and there, in ſo far as we admit hypotheſes, although conſiſtent with the phenomena, we may be ſaid to quit day-light, notwithſtanding we at the next ſtep emerge into light derived from the perceptions of ſenſe. For inſtance, if I combine a certain known weight of hydrogen and oxygen by combuſtion of their gazes, and produce water equal in weight to that of theſe gazes; and if I again reſolve this water, by means of the electric fire, or red-hot iron, into the original quantity of hydrogen gaz and oxygen, I cannot give the full and complete demonſtration of the compoſition of water and theſe gazes: for, as I proceed in the interpretation, I at length come to demonſtrate the mode of agency of the particles of the hydrogen and oxygen gaz on one another when they produce water, caloric, and light. But here I muſt call in the aid of the imagination: accordingly I imagine that the gazes conſiſt of hydrogen and oxygen, which are ponderable—united to caloric, and perhaps light, which are imponderable; that theſe ponderable particles unite with one another, and their caloric and light are ſet at liberty. Now here I have not any evidence of ſenſe; for I cannot perceive, by the ſenſes, the exiſtence of the compoſition of the gazes juſt ſtated, nor of their decompoſition, and union of their ponderable parts. This being the caſe, other kinds of imponderable matter may unite, or eſcape, beſides caloric and light; conſequently I cannot give the full and complete demonſtration in theſe inſtances. But the ſame objections may be made to the pretenſions to demonſtrate fully and completely the compoſition of ſulphate of ſoda, ſulphate of pot-aſh, or any double ſalt, perhaps, whatever; for caloric is ſeparated, and poſſibly other imponderable matter, when theſe ſubſtances are compounded; and caloric, and perhaps other matter, may unite when the ſulphuric acid and alkali are diſunited.

Hence chemiſtry, in its preſent ſtate, ought not to pretend to vie with mathematical philoſophy in its demonſtrations. But it does not appear improbable, that the ſame certainty as in mathematics may hereafter be attained in chemiſtry. We are encouraged to entertain this hope, from finding that the art of obſervation, and the invention of artifices for rendering the properties of matter evident to the ſenſes, have been proportionate to the advances of knowledge of facts; as was predicted by Chancellor Bacon.

IV.

Obſervations on the Electrophore, tending to explain the Means by which the Torpedo and other Fiſh communicate the Electric Shock.

THOSE who are converſant with electrical experiments know that the electrophore conſiſts of a flat metallic plate with an inſulating handle, and another ſeparate plate of non-conducting matter, either varniſhed glaſs, or ſome reſinous ſubſtance, coated with metal beneath, and placed with its uncoated ſide uppermoſt. When this uncoated face is electrified by friction or otherwiſe, and the plate of metal placed upon it, this laſt is found to give a ſmall ſpark to the finger, which is of the ſame nature as the electricity of the non-conducting ſurface; but when the plate is lifted by its inſulating handle, it will emit a ſpark to the finger which is much ſtronger, and of the oppoſite kind to that of the non-conducting plate. This phenomenon, which was at firſt conſidered as very difficult to be explained, was afterwards found to be produced throughout by the ſame energies, whatever they may be, which govern the electric charges. For, as I have elſewhere obſerved *, the charge conſiſts of two electricities, uſually called plus and minus, which compenſate each other; and alſo of a portion of electricity on the inſulated ſide, which in equal charges is greater, the greater the diſtance between the two electrified ſurfaces, and in unequal moderate charges is nearly in proportion to the charge itſelf. When the non-conducting ſurface of the electrophore is rubbed, it acquires both theſe electricities, namely, the charge by virtue of the compenſating power of the uninſulated coating beneath, and alſo the portion of ſimple electricity requiſite to maintain the charge. If the metallic plate be then placed upon it, and touched by the ſinger, a portion of the electricity may really paſs to the metal at the few places of actual contact; and another part, if the intenſity be ſtrong, may ſtrike through the thin plate of air interſperſed between the metal and the reſinous face. What remains on the reſinous ſurface will loſe by far the greateſt part of its intenſity, in conſequence of its becoming a charge to that ſmall plate of air, while the metallic plate, poſſeſſing the oppoſite part of the charge, will have its intenſity equally low. During theſe charges, the ſpark which paſſes from the metal muſt therefore be of the ſame character as that of the reſinous plate; in the ſame manner as the outer coating of the jar emits poſitive ſparks during its tranſition to the negative ſtate. The commencing intenſity of this ſpark cannot exceed that which belonged to the charge between the reſinous ſurface and the lower coating: but as the intenſity requiſite to maintain a charge on the thin plate of air is extremely minute, by far the greater part of the electricity will be employed in conſtituting that charge, and the ſpark given off by the plate will be much ſhorter and more gradual than if the ſame quantity had paſſed at once from the plate in a ſtate of ſimple electrization; that is to ſay, the ſpark will be apparently very ſmall. But when the plate is raiſed up by its inſulating handle, part of the charge on the reſinous ſurface will begin to acquire its former ſtate with regard to the lower coating; and that which continues to be compenſated by the upper moveable plate, will require a greater portion of uncompenſated electricity to maintain it. The intenſity of the plate will rapidly increaſe as it riſes. It will throw out ſparks and ramifications to the lower plate, and to the ſurrounding bodies; and at a certain diſtance the whole remaining portion of its former charge may be conſidered as ſimple electricity, which will ſtrike the finger at a greater diſtance, and with more ſuddenneſs and brilliancy. This ſpark, though in fact conſiſting of leſs electricity than the former, will nevertheleſs be more perceptible both to the eye and the ear, and conſequently will be thought larger: the firſt is the gradual exploſion of a charge; the ſecond conſiſts of the ſudden eſcape of a portion of ſimple electricity.

---

* Philoſophical Tranſactions, 1789.

If we ſuppoſe the non-conducting matter of the electrophore to be extremely thin, and the ſpark when in contact to be made to paſs from the upper to the lower metallic plate, the effect will be nearly the ſame as the tranſition of the ſhock from ſuch a coated electric with a charge of the quantity of electricity which paſſes; and when the moveable plate is raiſed, the effect will be ſimilar to that of a ſpark from the prime conductor. Or rather, perhaps, by a compariſon familiar to electricians, the firſt may be conſidered as the ſhock from a large battery charged by only one turn of the handle of the machine, and the latter as the ſhock from a ſmall jar capable of being charged high by the very ſame quantity of electric matter. It is certain that the ſhock, as well as the ſpark from the battery, would in theſe circumſtances be inconſiderable, though the effects of the jar might be very ſtriking.

When the electrophore is thus compared with the jar, its charge will be found to be wonderfully ſmall, as it conſiſts of no more electricity than, when uncompenſated, would paſs off in the ſimple ſpark which was obtained.

I found that two ſquare inches of Muſcovy talc, about one-hundredth part of an inch thick, which is an exceedingly good non-conductor, required, when coated with tinfoil, one turn of a ſmall cylinder to diſcharge through one-tenth of an inch; and one turn of the ſame cylinder charged a ſimple conductor of about ſix ſquare feet ſurface, ſo as to give a ſpark about nine inches long. Now if we aſſume the quantities of electricity in conductors to be as the length of the ſpark, which, though a doubtful poſition in great intenſities, may ſerve the preſent occaſion, and that a talc electrophore of two ſquare inches ſurface on each ſide have the intenſity to give a ſpark of one-tenth of an inch, its whole electricity will be expreſſed by $4 \times 0{,}1 = 0{,}4$. But the whole electricity of the conductor which gave the nine inch ſpark will be expreſſed by $6 \times 144 \times 9 = 7776$. And this number doubled, or 15552, for a double electrophore, alſo expreſſes ſuch a charge of the talc as gives a ſpark of exploſion or diſcharge of 0,1 inch. The length of the exploſive ſpark, on applying ſuch a ſimple electrophore to its plate, will therefore be ſhorter than 0,1 in the proportion of 0,4 to 15552, or 1 to 38880; the ſpark would therefore be the ,000002 of an inch long. And as Mr. Cavendiſh has found that electric ſhocks nearly equal are produced when the quantities of electricity are inverſely as the lengths of the ſpark of diſcharge, and alſo that the quantities of electricity in charges are as the ſurfaces, it will follow, that the ſhock of two ſquare inches of the talc with a ſpark of 0,1 inch, will be equal to the ſhock of 77760 ſquare inches with a ſpark of 0,000002 inches. This ſurface is equivalent to an electrophore of 279 inches ſquare, or meaſuring 23 feet by the ſide; or 19440 of the double electrophores of two inches ſurface.

To elucidate this, I conſtructed a ſmall electrophore adapted to perform the experiments which Beccaria called vindicating electricity. It conſiſted of two metallic plates with inſulating handles. Each was a ſquare whoſe ſide meaſured two inches, and the face of each was coated with the thin talc. When the two uncovered ſurfaces of the talc were applied together, and the whole charged as one plate, the diſcharge gave a denſe ſhock of conſiderable ſeverity. The ſparks, on ſeparation, were about one-eighth of an inch long, and very weak. No perceptible ſpark was exhibited on bringing them together. The operation appeared capable of being repeated almoſt inceſſantly, as in the electrophore.

It may perhaps be ſimpler to conſider the electrophore as a compound jar or plate variable in its thickneſs. When the plates of the inſtrument juſt deſcribed are together, the two electricities on the contiguous ſurface compenſate each other, and the external coatings may be ſuppoſed nearly in the natural ſtate. But when they are ſeparated, the portion of compenſated electricity will be leſs, and the intenſities would riſe if it were not for the external coatings, which in a great meaſure prevent it; each plate becoming charged like a ſimple jar, and the equilibrium being poſſible to be reſtored by communication between the external coatings. This ſecond charge, intenſity, and exploſion will be greater the further the plates are removed. It appears, therefore, that in a deduction of the manner in which electric fiſh may communicate the ſhock, we may ſafely avoid the more complicated conſideration of the electrophore, and compare the action immediately to that of a ſimple jar.

Mr. Hunter, in the 63d volume of the Philoſophical Tranſactions, page 434, deſcribes the electric organ of the torpedo to conſiſt of a number of columns varying in their length from an inch and an half to a quarter of an inch, and their diameters about 2-10ths of an inch. The number of columns in each organ of the torpedo he preſented to the Royal Society was about 470; but in a very large torpedo the number of columns in one organ was 1182. Theſe columns were compoſed of films parallel to the baſe of each, and the diſtance between each partition of the columns was 1-150th of an inch. If we ſuppoſe theſe films to be charged with electricity, and to be 1-300th of an inch thick; and a middling ſize torpedo to contain in both organs on the whole 1000 columns of an inch long, and 0,03 ſquare inches area at the baſe; then 1000 × 150 × 0,03 = 4500 ſquare inches. Now I found that Muſcovy talc, of 0,01 inch thick, has twelve times the capacity of the glaſs of a jar of 421 ſquare inches, which, from former experience, I know to be as thin as ſuch jars can be without danger of breaking by exploſion; and the torpedinal membrane, being leſs than one-third the thickneſs of the talc, will have three times the capacity; that is to ſay, its capacity will be thirty-ſix times that of ſtout glaſs; or both organs will be equivalent to 4500 × 36 = 162000 ſquare inches, or 1125 ſquare feet.

My large jar, with Lane's electrometer, meaſuring a ſpark about 1-200th of an inch long, gave very ſenſible and rather unpleaſant ſhocks acroſs the hand, and alſo the tremulous ſenſation cauſed as I ſuppoſe by the imperfect conducting power of the ſkin with ſo low an intenſity. When the ſpark was 1-100th of an inch long, the ſhock was ſtrong enough to convulſe the hand, and at leſs than 1-50th it was painfully ſtrong. This laſt was probably ſtronger than the ſhock of the torpedo. If we therefore avail ourſelves of Mr. Cavendiſh's deduction, that, the quantity of electricity being increaſed in proportion as the length of ſpark is diminiſhed, the ſhock will be rather greater than before, we may compute the length of the ſpark in the torpedinal ſhock of the magnitude laſt mentioned. For the organs of the torpedo, compared as to their capacity with that of the jar, will be equivalent to $\frac{162000}{421} = 375$ times as great, and they will give ſuch a ſhock when charged as to afford a ſpark $\frac{1}{375}$ of $\frac{1}{50}$ of an inch $= \frac{1}{18750}$ inch. No wonder, therefore, that the ſhock will not paſs through an interrupted circuit, and that the ſpark is not exhibited.

Reſpecting the manner of operation, there are no facts which ſhew how this charge is actually produced, maintained, and communicated. Whether electricity be actually collected, compoſed, or decompoſed in the organs of the fiſh, or whether it ſimply exiſts in thoſe organs, as perhaps it may in all bodies, in the ſtate of what is called compenſation, are queſtions concerning which we in fact know nothing. It has appeared to me, from the obſervation of the high electric ſtate which talc naturally poſſeſſes, and from the innumerable ſhocks the electrophore is capable of giving by mere change of arrangement, that a machine might be conſtructed alſo capable of giving numberleſs ſhocks at pleaſure, and of retaining its power for months, years, or to an extent of time of which the limits can be determined only by experiment. I will not here deſcribe the mechanical combinations which have occurred to me in meditating on this ſubject, but ſhall ſimply ſhew that the dimenſions of the organs of the torpedo are ſuch as by certain very poſſible motions, and the allowable ſuppoſition of conducting and non-conducting powers, may produce the effects we obſerve. How far it may be probable muſt undoubtedly be left to future experimental reſearch.

In new talc, which had never been excited nor electrified, and exhibited no ſigns of electricity when applied to Bennet's electrometer, I found that the laminæ were naturally in ſtrong, oppoſite, electric ſtates, counterbalancing each other. When they were torn aſunder in the dark, they gave flaſhes at leaſt 1-10th of an inch long to each other. This is 1875 times the intenſity of the torpedinal electricity, as before deduced. If, therefore, one or more columns of talc, or other thin electric plates 1-300th of an inch thick, and making up the ſurface of the electric organs of the torpedo, were ſo conſtructed as that the plates might touch each other by pairs only, naturally in oppoſite ſtates, and coated on the outſide; if, moreover, there were one common conductor communicating with the upper plate of every pair, and another in the ſame manner with the lower;—then a ſeparation of all the pairs to the diſtance of only $\frac{1}{18750}$ inch would produce the torpedinal intenſity; the equilibrium would be reſtored by the two conductors if made to communicate, and whatever living creature was in the circuit would receive a ſhock: and on reſtoring the original ſituation of the apparatus, a ſhock might alſo be given. The force of theſe ſhocks would differ according to the quantity of the apparatus made uſe of at the time, or the diſtance to which the plates were drawn aſunder. If different columns were exploded in rapid ſucceſſion, the quick repetition of ſmall ſhocks would produce the tremulous ſenſation.

If we were to conjecture that the torpedo actually operates like a machine of this kind, we ſhould find our ſuppoſition to include the following ſubordinate parts:—1. The membranes may be non-conductors, and the fluid between them a conductor. 2. They may act as electrophores. 3. The white reticular matter between the columns may conſiſt of conductors ſeparately leading to the two oppoſite ſurfaces. 6. Theſe ſeparate conductors, in all their ſubdiviſions, may be well kept aſunder by a covering of non-electric matter. If this be of the ſame kind as the membranes, and $\frac{1}{10800}$th of an inch thick, it would be ſufficient for the purpoſe, becauſe the intenſity of the electric ſtate is deduced from its power of breaking through a much more permeable electric, namely, air, at nearly twice that interval. 7. The effects may alſo be produced by the motion of conducting plates in a non-conducting fluid.

V.

A Letter from Mr. VON HUMBOLDT to M. H. VAN MONS on the Chemical Proceſs of Vitality; together with the Extract of a Letter from Citizen FOURCROY to Citizen VAN MONS on the ſame Subject.*

I HAVE lately addreſſed ſeveral letters to Meſſrs. Dolomieu and Fourcroy at Paris, and perceive by thoſe I have received from the former that mine have miſcarried. Permit me, Sir, to addreſs myſelf to you. By your means I may perhaps ſucceed in forwarding to Paris ſome explanations reſpecting facts which, as I underſtand, employ part of the time of the National Inſtitute. Be pleaſed to accept my aſſurances of the great reſpect which your zeal and your chemical diſcoveries have inſpired me with. The natural philoſophers of Europe ought to form a ſingle family. They are in purſuit of the ſame intereſting objects; and this is a ſufficient motive to produce that uſeful degree of intimacy which is calculated to promote their reſearches.

You are probably acquainted with my Eſſays on the Vegetable Philoſophy, ſuch as my *Aphoriſmi ex doctrinâ phyſiologiæ chimicæ plantarum*, annexed to my *Flora ſubterranea Fribergenſis*, and ſeveral memoirs which I have preſented to the National Inſtitute.——The memoir on the action of oxygenated muriatic acid upon the vegetable and animal fibre, which is printed in the *Magazin Encyclopédique* of Millin, Noël, and Warens, ſeems to have had more ſucceſs. I am happy to hear that Meſſrs. Vauquelin and my friend Dolomieu have begun to repeat my experiments. As the memoir which was read to the National Inſtitute related principally to the germination of vegetables, I have thought it my duty to announce to you certain facts more ſtriking reſpecting the animal fibre. The ſtrongeſt ſtimulus of the nervous fibre is that of the alkalis. It appears that theſe ſalts affect the irritable and ſenſible ſyſtem by means of their azote. Let the thigh of a frog be thrown into the oxygenated muriatic acid, or the nitric acid, and it will remain motionleſs. Let it be put into a ſolution of pot-aſh, or of ſoda, and it will undergo contractions no leſs ſtrong than when irritated by the metals. Theſe motions always commence at the lower extremities. The toes move firſt, afterwards the *muſculus gaſtrocnemius*, and then the thigh. If the nerve be very ſenſible (for nothing more is required than ſimply to immerſe the extremity of the crural nerve in the oleum tartari per deliquium), the contractions will end in an univerſal tenſion or rigidity. The leg riſes up perpendicularly, the membrane of the feet extends itſelf, and the tetanus appears. In this ſituation all the irritability of the fibre appears to be extinguiſhed; and if an electric ſtroke be paſſed through the limb, the exhauſtion becomes real. It is a ſtriking phenomenon to ſee the laſt remaining ſigns of tetanus diſappear in an inſtant. But there is another method by which the tenſion diſappears, and by which I am able to reſtore the irritability to the organs. It ſeems that the acidifiable baſes of the alkali, principally the azote, have conſumed all the oxygene contained in the fibre. The chemical proceſs of vitality ceaſes. If I pour an acid, for example the nitric acid, upon the nerve, an effervеſcence will take place; part of the alkali becomes latent, and the reſt will have a proper proportion with reſpect to its oxygene. From this moment the contraction with zinc and ſilver is again produced. Increaſe the quantity of acid, and the movements are again weakened. In this manner it is, that by forming an equilibrium between the azote of the alkali and the oxygene of the acid applied to the animal fibre, the irritability of the organs may be taken away or reſtored three or four times in ſucceſſion. You may eaſily perceive, Sir, that theſe experiments require ſteady attention. The degree of inſenſibility to which the nerve is reduced by repeating them may be very different. It is poſſible to determine exactly the quality of the chemical agents, their weight and temperature; notwithſtanding which, many experiments do not ſucceed. The reaſon is, that there are conditions which depend on the individuality of the organization, and concerning which we muſt ſtill confeſs our total ignorance. The influences of the oxygenated muriatic acid upon the animal fibre are leſs marked than thoſe of the alkalis; but they are nevertheleſs of much importance. I ſteeped the feet of a frog (I mention this animal by preference, though I have made the ſame experiments on other ſpecies) in a ſolution of opium in alcohol. The metals, or galvaniſm, excited no motion. I threw one leg into pure water, and the other into the oxygenated muriatic acid; the firſt remained motionleſs, the ſecond gave very ſtrong contractions, and ſhewed that its irritability was reſtored. The common acids depreſs the irritability of the nervous fibre. A crural nerve, rendered inſenſible by the ordinary muriatic acid, remains ſo though it has been ſteeped in the ſolution of pot-aſh: but the mineral acids exhauſt the forces of the muſcles, by condenſing the elements of the muſcular fibre. Theſe acids act in the ſame manner as cold, which depreſſes the nerves, and is beneficial to the muſcles. The muſcles and the nerves have ſpecific ſtimuli, agreeable to the diverſity of the elements. The terrible action which the alkalis exerciſe on the nerves appears to explain the effect of the ſecretion of the ſeminal liquor on the blood. It is this alkali which, diſtributed throughout the ſyſtem, anſwers the purpoſe of a ſtimulus beneficial to the animal fibre. By this action I account for the ferocity of the ichthyophagi.

---

* Annales de Chimie, XXII. 64.

My eldeſt brother, who is very ſkilful in the ſtudy of anatomy, applied zinc and ſilver to the mouth and the brain of a dead fiſh; it afforded no motion. I poured oxygenated muriatic acid on the nerves, and at that inſtant the contractions became very ſtrong. Mr. Herz and ſeveral learned men of Berlin were preſent at theſe and many other experiments. The heart of the ſame fiſh, which had entirely ceaſed to palpitate, began to perform this movement with regularity when I threw it into the oxygenated muriatic acid. The ſame experiment ſucceeded very often with the hearts of frogs: when a heart is immerſed in a ſolution of pot-aſh, it loſes its irritability for ever; ſo that azote is not the ſpecific ſtimulus of the heart.

Mr. Pfaff, while employed in my experiments reſpecting germination in the oxygenated muriatic acid, has diſcovered that frogs ſuffocated in the oxygenated muriatic acid gas exhibit a very high degree of irritability after their death. I beg you will fix the attention of Mr. Vauquelin on the action of ſulphate of pot-aſh upon the nerves. I have been aſtoniſhed at every thing I beheld. Two legs of frogs in a very lively ſtate were ſteeped in the ſolution of the ſulphate of pot-aſh. I tried them three or four minutes afterwards with the metals. The contractions had increaſed in force, and were even convulſive. It appeared that the three acidifiable baſes contained in the ſolution hydrogene, azote, and ſulphur, acted ſtrongly on the oxygene conveyed by the arterial blood. This action revives the proceſs of vitality. After fourteen or ſixteen minutes the whole thigh became of a blackiſh brown. All the oxygene of the blood was abſorbed, aud the carburet of hydrogene appeared in a diſengaged ſtate. The zinc and the ſilver are not then capable of exciting the ſmalleſt motion.

Yet it would be a great miſtake to conclude that all irritability is exhauſted in this caſe. I have ſeen the contractions re-appear ſeveral times on reſtoring oxygene to the fibre by means of a ſolution of the oxide of arſenic. The flame is thus renewed which ſeemed ready to expire. The oxide of arſenic produces a tetanus and perfect inſenſibility if the nerve remains long immerſed. It ſeems then that the too great quantity of oxygene abſorbs as it were the acidifiable baſes which ſupport the chemical proceſs of vitality. I have thrown the whole thigh into the ſolution of pot-aſh, and I obſerved that galvaniſm afterwards had the power of exciting motion.

You ſee, Sir, what an immenſe number of experiments remain to be made on theſe objects of vital chemiſtry. It is enough that a method has been pointed out of meaſuring the degree of irritability of the organic parts by means of galvaniſm. I ſhall have the honour to ſend you my work on the nervous and muſcular fibre, and on the chemical proceſs of vitality. I collect facts, and miſtruſt my own hypothetical ideas. You will perceive with me how miſtaken the notion is that oxygene performs the principal part in this proceſs. My experiments prove that the irritability or tone of the fibre depends only on the mutual equilibrium between all the elements of the fibre, azote, hydrogene, carbone, oxygene, ſulphur, phoſphorus, &c. The chemical combinations of phoſphorus and of azote, for example, appear to be in no reſpect leſs important than thoſe of oxygene with the acidifiable baſes. How much light may we not expect from the advances of yourſelf, Fourcroy, and Vauquelin on theſe objects!

Von Humboldt.

ADDITION to the foregoing LETTER.

HAVING preſerved ſome frogs for the winter, I have this morning repeated ſome experiments, of which I venture to ſend you an account. In the preceding letter I have remarked that, as we are only ſuperficially acquainted with the principles of vital chemiſtry, we ought not to be ſurpriſed if we do not always obtain the ſame reſults. A negative experiment proves nothing againſt another of an affirmative nature.

I am very ſure that a nerve rendered inſenſible by alcohol will not recover its irritability by ſulphate of pot-aſh. But it may very well happen that a thigh, of which the tetanus has been cauſed by the oxide of arſenic, ſhould remain in a ſtate of tenſion notwithſtanding the action of the ſolution of pot-aſh.

I have ſeen the following facts within this quarter of an hour. I took the four extremities of a very lively frog. The right arm and the right leg leaped on zinc and ſilver. I ſteeped them for four minutes in alcohol. The hydrogene acted ſtrongly on the fibre. The toes of the foot trembled during the firſt minute. Soon afterwards a total rigidity came on; the muſcle became white, the blood having apparently loſt its oxygene. I replaced the arm and the leg on the zinc and ſilver, but there was not the ſlighteſt contraction. I then quickly threw them into the oxygenated muriatic acid, which I had ſhaken ſtrongly before it was poured out; the limbs remained in it for three minutes. A ſlight tremulous motion ſhewed, even in the cup, that the vital forces were reſtored. I replaced the arm and the leg on the metals; the contractions were again produced, not only with zinc and ſilver, but with zinc and iron.

Here I think is a very ſimple and deciſive experiment. I then changed the method in order to obſerve the effect. I took the left thigh, and immerſed it for nine minutes in alcohol. It loſt all irritability, and the oxygenated muriatic acid was no longer capable of reſtoring the vital force. The left arm had remained untouched for fifteen or eighteen minutes. I prepared its nerve, but it ſhewed only very weak and ſlow contractions with zinc and ſilver. I threw it into alcohol. After the firſt minute its irritability was increaſed, the galvaniſm acted more ſtrongly; but after three minutes all the irritability was exhauſted, and I applied in vain the remedy of oxygenated muriatic acid. I ſteeped the arm in the ſolution of the oxide of arſenic, and it then afforded contractions, though very weak.

Here are four experiments, two of which ſucceeded, and in the two others the vital forces were not reſtored. I think, nevertheleſs, that in good logic we ought to admit the affirmative experiments. Examine the conditions, and you will ſee they are very different. The left leg remained too long, nearly nine minutes, in the alcohol. The right arm was already very weak when the experiment began. Who can boaſt of reviving the dead?—If of two chemiſts the one ſhould obtain oxygene gas by heating the red oxide of mercury, while the other did not obtain it, we ſhould always believe that the apparatus of the latter was not hermetically cloſed. I never ſaw an organ rendered inſenſible by alcohol which recovered its irritability by being left to itſelf. It neceſſarily follows, therefore, that in the experiments I have ventured to relate, and of which my work contains a very great number, the oxygene of the muriatic acid muſt have been a principal agent. The art of medicine will be infinitely benefited if we ſhould ſucceed in obſerving the phenomena which the ſeveral elements produce in contact with the irritable fibre. It is proper to begin with ſimple combinations, and afterwards proceed to combinations of two, three, and four principles.

I have ſent to the National Inſtitute a memoir on the nature of light, and its chemical combinations. Mr. Wedgwood pretends that the phoſphoreſcence of calcined bodies is not altered in hydrogene and azotic gas. I think he did not purify theſe gaſes by means of phoſphorus, as I did. I have ſeen luminous wood extinguiſhed in the azotic and hydrogene gaſes. A ſmall quantity of oxygene, being admitted into the veſſel, cauſes the whole of the phoſphoreſcence[*] to revive. I have alſo converted morilles (Phallus Eſculentus) into a ſubſtance which reſembles tallow, by means of the ſulphureous acid. I have made ſoap of it.

Bayreuth, December 29, 1796. VON HUMBOLDT.

Extract of a Letter from Citizen FOURCROY *to Citizen* VAN MONS *on the Subject of that of Mr.* HUMBOLDT.

I THINK Mr. Humboldt proceeds with rather too much expedition in his ſolutions. It is to be feared that he may find it neceſſary to retract. I am apprehenſive that he admits too many hypotheſes[†]: that he does not repeat each experiment ſufficiently before he forms a concluſion. This in particular is much more important with regard to the philoſophy of animal bodies than any other branch of ſcience, becauſe it is ſurrounded by numberleſs difficulties and multiplied ſources of error and illuſion. I fear that if certain chemiſts continue to advance with ſuch rapidity, the phyſicians will ſoon have reaſon to exclaim againſt this encroachment. If applications be too ſuddenly made, and arbitrary ſuppoſitions accumulated, it may perhaps come to paſs that this ſcience may a ſecond time be rejected from the healing art, as it was formerly by Stahl and Boerhaave, in conſequence of the exceſſive abuſe of hypotheſes committed before their time in this reſpect by Tachenius, Willis, and others. Too much earneſtneſs may be equally pernicious to chemiſtry as well as medicine, and impede the progreſs which the firſt is capable of producing, and ought to produce, in the ſecond.

Meſſrs. Girtanner and Valli appear to me to make an ill uſe of their abilities and knowledge in this reſpect. They ſuffer themſelves to be carried away by the ingenious notions they derive from modern chemiſtry.

All this, however, does not prevent my being of opinion that the experiments of Mr. Humboldt are extremely intereſting, and that he ought to continue them with aſſiduity; but I ſhould wiſh that he would vary them more, repeat each in particular more frequently, and be moderate in his concluſions. I cannot, for example, repeat to you how many new ideas and chemical explanations, very probable in themſelves, have occurred to me during ſeveral years paſt, in conſequence of my reſearches into the animal analyſis. They would be ſufficient to change the aſpect of phyſiology and medicine; but I have been careful not to publiſh them until well matured and proved by experience, leſt I ſhould otherwiſe embarraſs two ſciences at once. I wiſh to riſk nothing of this kind, but proceed gently, and hope to arrive at ſolid concluſions in the courſe of time. I am earneſtly deſirous of ſeeing the work of Mr. Hildebrandt, as well as that of M. Humboldt. Notwithſtanding the ſpeed they appear to ſhew in their chemical explanations of vegetable and animal life, I know not why I am perſuaded that they are leſs advanced than we in the analyſis and true intimate knowledge of the materials of theſe two kingdoms. I very much commend their zeal, and admire their bold advances; but they cannot blame our well-grounded caution and prudent ſlowneſs. It is admirable to proceed with expedition, and make great advances in the paths of nature; but it is ſtill better to obſerve well, to ſee clearly, and to communicate with accuracy what we obſerve in our progreſs. I am ſtill on my journey, and confeſs that I am very far from having arrived at the place I am deſirous of reaching.

---

[*] Spallanzani obſerved the ſame phenomena; and, what is more remarkable, he obſerved the phoſphoric or ſhining animals ceaſed to emit light in the azote, hydrogene and carbonic gaſes, and that they emitted a light infinitely more vivid in oxygene gas than in the atmoſpheric air. Chimico Eſame degli Eſperimenti de Goettling. Modena, 1796.

[†] For example, in the preceding memoir he ſpeaks of the azote of alkalis as if it were demonſtrated that azote is one of the principles of theſe bodies. I firſt announced or ſuſpected this eight years ago, but it is not yet proved. F.

The young men have attended with ardour my courſe of Animal Chemiſtry at the School of Medicine. Nothing can equal their wiſh to learn. The twenty lectures I give on this part of chemiſtry, which is ſo new, produce, as I ſee, a great movement in this branch of the ſtudy of nature. But I moderate their enthuſiaſm as much as I can. I fear leſt by too much precipitation this beautiful machine ſhould be broken in my hands. It would be much to be regretted if expectations ſo rich and ſo happy were to be diſſipated in ſmoke; and this will not fail to happen if the edifice be built on hypotheſes, or too much haſte be made in conſtruction before the materials are ready. I collect them by degrees, but they are ſtill too ſcanty to riſk the formation of a ſyſtem. Yet, to ſpeak plainly, I think there are few chemiſts who poſſeſs more facts than myſelf on the animal analyſis: but they are not yet ſufficiently connected in their relations to each other to form an entire work. If the attempt were made, it certainly would not be *Ære perennius*, &c. &c.

VI.

Concerning the Properties of the Sulphureous Acid, and its Combinations with Earthy and Alkaline Baſes. By Citizens FOURCROY *and* VAUQUELIN.

Concluded from page 318.

Sulphite of Soda.] THIS ſalt is white, and perfectly tranſparent. Its figure a four-ſided priſm, two of the ſides being very broad, and two narrow, terminating in dihedral pyramids. Its taſte is cool, and afterwards ſulphureous.

Its habitudes in the fire are abſolutely the ſame as thoſe of the ſulphite of pot-aſh, excepting only that it commences its alterations by the aqueous fuſion.

By expoſure to the air it effloreſces, and is afterwards converted into ſulphat, but leſs ſpeedily than the ſulphite of pot-aſh.

It requires four parts of water for its ſolution; is more abundantly ſoluble in hot water, and readily cryſtallizes by cooling.

Barytes, lime, and pot-aſh decompoſe the ſulphite of ſoda.

It is not ſoluble in alcohol.

Salts with baſes of pot-aſh, except the carbonate, do not decompoſe this ſalt; the other genera decompoſe it like the ſulphate of pot-aſh.

The metallic oxides, and their ſolutions in acids, have the ſame effects with the ſulphite of ſoda as with that of pot-aſh.

It contains per quintal—1. Soda 18,8;—2. Sulphuric acid 31,2;—3. Water 50.

Sulphite of Ammoniac.] This ſalt has the form of a priſm of ſix ſides, terminated by ſix-ſided pyramids. It ſometimes aſſumes the figure of a ſquare tablet, the borders of which are ſloped ſo as to form a ſolid of ſix irregular faces. Its taſte is cool and penetrating, like that of the ammoniacal ſalts; but it leaves a ſulphureous impreſſion in the mouth.

By expoſure to the air it attracts moiſture, and ſoon afterwards paſſes to the ſtate of ſulphat.

It is very ſoluble, and requires at moſt its own weight of water for its ſolution.

Heat increaſes its ſolubility, and it cryſtallizes by cooling.

On the fire it is volatilized without decompoſition.

Barytes, lime, pot-aſh, and ſoda decompoſe it in the cold; magneſia produces the ſame effect by the aſſiſtance of heat.

The acids act on the ſulphite of ammoniac in the ſame manner as on thoſe of pot-aſh and ſoda; but the reſults are different.

Charcoal does not convert it into ſulphuret, becauſe it riſes too ſpeedily by heat.

It does not decompoſe ſalts with baſes of pot-aſh or of ſoda, but it decompoſes thoſe of lime, magneſia, barytes, and alumine, with which it forms inſoluble precipitates.

Its habitudes with the metallic oxides and ſalts are nearly the ſame as thoſe of the ſulphites of pot-aſh and of ſoda, excepting that it forms with ſeveral of them triple ſalts, as we ſhall more amply explain in another memoir on the metallic ſulphites.

Its component parts in the quintal are—1. Ammoniac 29,07;—2. Sulphuric acid 60,06;—Water 10,87.

Sulphite of Lime.] The form of the ſulphite of lime is that of a ſix-ſided priſm, terminating in a very long pyramid. Its taſte at firſt is ſcarcely perceptible; but when it has been kept for ſome time in the mouth, it communicates to the tongue a taſte which is manifeſtly ſulphureous.

This ſalt, when well neutraliſed, is very ſparingly ſoluble in water; but it becomes ſoluble by an exceſs of acid. In this way it may be obtained in cryſtals; that is to ſay, by expoſing its ſolution in the ſulphureous acid to the air. The acid is diſſipated, and leaves the ſalt in a ſtate of purity. Barytes alone, among the earths, is capable of decompoſing it. This may be aſcertained by mixing a ſolution of that earth with a ſolution of the neutral ſulphite of lime, when a light precipitate is formed.

Heat converts it into ſulphate, by depriving it of a portion of ſulphur.

The mineral acids decompoſe it, like the other ſulphites.

The alkalis produce no change in this ſalt; for the alkaline ſulphites are decompoſed by lime.

Among the neutral ſalts, the alkaline carbonates, as well as the alkaline phoſphates and fluates, alone decompoſe it.

It does not acquire the ſtate of ſulphat by contact of the air, but very ſlowly.

Metallic ſolutions decompoſe it, at leaſt for the moſt part.

The quintal contains—1. Lime 47;—2. Sulphuric acid 48;—3. Water 5.

Sulphite of Magneſia.] This ſalt is white and tranſparent. Its form a depreſſed tetrahedron; its taſte is mild and earthy at firſt, and afterwards ſulphureous.

By expoſure to heat, it ſoftens, ſwells up, and becomes ductile, like gum; it loſes about 0,45 of its weight by the deſiccation.

If the heat be kept up after it has loſt its water of cryſtallization, the ſulphureous acid flies off, and the reſidue in the retort is magneſia, nearly pure. In this way we found that a quintal of this ſalt is compoſed of 16 parts magneſia, 39 ſulphureous acid, and 45 water.

This ſalt is ſparingly ſoluble in water, but very ſoluble in an exceſs of acid; and this ſolution, expoſed to the air, cryſtallizes very readily, by loſing its exceſs of acid.

It becomes opake in the air, and changes by degrees into ſulphate; but for this purpoſe much time is required.

The fixed alkalis, lime and barytes, decompoſe it completely; ammoniac decompoſes it in part only, and a triple ſalt is formed.

The alkaline and earthy ſalts, except thoſe of alumine, decompoſe it likewiſe.

The mineral acids and metallic ſolutions produce the ſame effects on this ſalt as on the other ſulphites.

Sulphite of Barytes.] This ſalt does not cryſtallize, has no perceptible taſte, and is perfectly inſoluble in water. For this reaſon the ſulphureous acid carries down a precipitate from the aqueous ſolution of barytes. It is not rendered ſoluble by an exceſs of acid, like the other earthy ſulphites.

Heat changes it into ſulphate; but it does not paſs to this ſtate by expoſure to the air, but with extreme difficulty.

No earth nor alkali decompoſes it.

The acids decompoſe it; whence the ſulphureous acid does not precipitate the barytic ſalts, as the ſulphuric acid does.

Among the neutral ſalts there are none but the alkaline carbonates which decompoſe it.

The other properties of this ſalt are ſuch as are common to the ſulphites.

It is compoſed of—1. Barytes 59;—2. Sulphureous acid 39;—3. Water 2.

Sulphite of Alumine.] It is inſoluble in water, but becomes abundantly ſo by exceſs of acid. Its diſſolution does not cryſtallize by the contact of air, and becomes converted into a ſoftiſh ductile maſs.

Fire diſengages the ſulphuric acid without alteration.

All the alkalis and earths decompoſe it, as do alſo the mineral acids.

Its component parts are—44 alumine, 32 ſulphureous acid, and 24 water.

From the facts deſcribed in this memoir it is evident that the ſulphites poſſeſs very different properties from thoſe of the ſulphates; and that they follow peculiar laws of ſolution, cryſtallization, affinity, and decompoſition.

In fact they poſſeſs—1. A ſulphureous taſte, ſimilar to that of their acid. 2. They are decompoſable by fire, either by the eſcape of their acid without alteration, or by loſing a portion of ſulphur and becoming converted into ſulphates. 3. They are converted into ſulphates by the contact of air, or of any other ſubſtance capable of affording oxygene; and their weight is increaſed by this converſion. 4. They are decompoſed by moſt acids; which expel the ſulphureous acid with effervеſcence, and the production of a ſtrong penetrating odour. 5. They burn rapidly, and with flame, when heated with ſuper-oxygenated muriate of potaſh, or with ſaltpetre, and become ſulphates. 6. The alkaline ſulphites are more ſoluble than the ſulphates, and the earthy ſulphites are much leſs ſo. 7. Laſtly, the ſulphite of lime is not decompoſed by the alkalis, like the ſulphate.

VII.

An Account of the Great Copper Works in the Iſle of Angleſey. By Mr. ARTHUR AIKIN [*].

Auguſt 13, 1796.

THIS has been a moſt intereſting and entertaining day, being ſpent in viſiting the vaſt copper works connected with the Parys Mountain. We breakfaſted at Amlwch, a conſiderable town on the coaſt, about two miles from the mine, and almoſt entirely peopled by the miners and their families.

We had no difficulty in diſtinguiſhing this celebrated mountain, for it is perfectly barren from the ſummit to the plain below; not a ſingle ſhrub, and hardly a blade of graſs, being able to live in this ſulphureous atmoſphere.

"No graſſy mantle hides the ſable hills,
No flowery chaplet crowns the trickling rills;
Nor tufted moſs nor leathery lichen creeps
In ruſſet tapeſtry o'er the crumbling ſteeps."

DARWIN.

The nearer we approached the ſcene of buſineſs, the more penetrating was the fume of the ſulphur; but we had very ſoon too many objects of attention to regard this inconvenience. The mountain is about a mile in length, and is the property of Lord Uxbridge and the Rev. Mr. Hughes, and the fortunate diſcovery of the copper took place a little more than thirty years ago; thus converting a piece of ground, originally of very little value, into one of the moſt profitable eſtates in the kingdom.

The ſubſtance of the mountain being ore, the work is carried on in a very different manner from the cuſtom of other mines: here are, comparatively, few ſhafts or levels, the greater part being quarried out, ſo as to leave a vaſt excavation open to the day. There are two of theſe quarries or mines, which are worked by two different companies: the firſt goes by the name of the Mona Mine, and is the ſole property of Lord Uxbridge; the other, called the Parys Mine, is ſhared between the Earl and Mr. Hughes. The view down this ſteep and extenſive hollow is ſingularly ſtriking. The ſides are chiefly of a deep yellow or duſky ſlate colour, ſtreaked, however, here and there, by fine veins of blue or green ſhooting acroſs the cavern, mingled with ſeams of greyiſh yellow. The bottom of the pit is by no means regular, but exhibits large and deep burrows in various parts, where a richer vein has been followed in preference to the reſt. Every corner of this vaſt excavation reſounds with the noiſe of pickaxes and hammers: the edges are lined with workmen drawing up the ore from below; and at ſhort intervals is heard, from different quarters, the loud exploſion of the gunpowder by which the rock is blaſted, reverberated in pealing echoes from every ſide.

---

[*] Tour through North Wales.

The exterior covering of the mountain is an aluminous ſlate; the matrix black-grey chartz; the ore *copper*, chiefly

I. *The yellow ſulphurated:* of which the richeſt contains, according to miners' computation, that is, in the proportions of the ounce troy,

Sulphur	5 dwt. (25 per cent.)
Copper	Ditto.
Refuſe	10 dwt. (50 per cent.)

The worſt ore yields nearly the ſame quantity of ſulphur, but of metal no more than ſix grains (1¼ per cent.); this inferior kind, however, is chiefly worked for the ſulphur. The other ſpecies and varieties of ore that the mine produces are,

II. Black ore, containing copper, mixed with galena, calamine, and a little ſilver.

III. Malachite, or green and blue carbonate of copper.

IV. Native copper, but in very ſmall quantity.

V. Sulphate of copper, cryſtallized and in ſolution.

VI. Sulphate of lead in conſiderable quantity, containing a pretty large proportion of ſilver.

VII. Native ſulphur.

Proceſs.—The ore is got from the mine by blaſting; after which it is broken into ſmaller pieces by the hammer (this being chiefly done by women and children), and piled into a kiln, to which is attached by flues a long ſulphur chamber. It is now covered cloſe; a little fire is applied in different places, and the whole maſs becomes gradually kindled; the ſulphur ſublimes to the top of the kiln, whence the flues convey it to the chamber appointed for its reception. This ſmouldering heat is kept up for ſix months, during which the ſulphur chamber is cleared four times, at the expiration of which period the ore is ſufficiently roaſted. The pooreſt of this, that is, ſuch as contains from 1¼ to 2 per cent. of metal, is then conveyed to the ſmelting-houſes at Amlwch-port; the reſt is ſent to the company's furnaces at Swanſea and Stanley near Liverpool. The greater part of the kilns are very long, about ſix feet high; and the ſulphur chambers are of the ſame length and height, connected by three flues, and on the ſame level with the kilns; four new ones, however, have been built at Amlwch-port, by which much ſulphur is preſerved that would have been diſſipated in the old kilns. The new ones are made like lime-kilns, with a contrivance to take out the roaſted ore at the bottom, and thus keep up a perpetual fire. From the neck of the kiln branches off a ſingle flue, which conveys the ſulphur into a receiving chamber, built on the rock, ſo as to be on a level with the neck of the kiln, that is, above the ore.

The two ſmelting-houſes, of which one belongs to each company, contain 31 reverberatory furnaces, the chimnies of which are 41 feet high; they are charged every five hours with 12 cwt. of ore, which yields ½ cwt. of rough copper, containing 50 per cent. of pure metal; the price of rough copper is about 2l. 10s. per cwt. The coals are procured from Swanſea and Liverpool, a great part of which is Wigan ſlack. From experiment it appears, that though a ton of coals will reduce more ore than the ſame quantity of ſlack, yet, owing to the difference of price, the latter is, upon the whole, preferable; the prices of the two at Liverpool being—coals 8s. 6d. per ton—ſlack 5s. per ditto.

The ſulphate of copper, however, is the richeſt ore that the mine yields, containing about 50 per cent. of pure metal. This is found in ſolution at the bottom of the mine, whence it is pumped up into ciſterns, like tanners' pits, about two feet deep: of theſe pits there are many ranges, each range communicating with a ſhallow pool of conſiderable extent. Into theſe ciſterns are put caſt-iron plates, and other damaged iron veſſels procured from Coalbrook Dale; when the ſulphuric acid enters into combination with the iron, letting fall the copper in the form of a red ſediment very ſlightly oxided. The ciſterns are cleared once in a quarter of a year, when the ſulphate of iron in ſolution is let off into the ſhallow pool, and the copper is taken to a kiln, well dried, and is then ready for exportation. The ſulphate of iron remaining in the pool partly decompoſes by ſpontaneous evaporation, and lets fall a yellow ochre, which is dried and ſent to Liverpool and London.

The ſulphur produced in the roaſting, after being melted and refined, is caſt into rolls and large cones, and ſent to London. The cones are uſed chiefly for the manufactory of gunpowder and ſulphuric acid.

Green vitriol and alum are alſo made in ſmall quantities by a ſeparate company; but to theſe works ſtrangers are not admitted.

The number of men employed by the two companies is 1200 miners, and about 90 ſmelters; the miners are paid by the piece, and earn in general from a ſhilling to twentypence per day.

The depth of the mine in the loweſt part is 50 fathoms, and the ore continues as plentiful as ever, and of a quality rather ſuperior to that which lay nearer the ſurface.

With regard to the annual quantity of ore raiſed, little certain can be mentioned. The Parys Mine has furniſhed from 5000 to 10,000 tons per quarter, excluſive of what is procured from the ſulphate of copper in ſolution; and as the two mines employ nearly equal numbers of workmen, they probably afford about the ſame quantity of ore.

Adjoining to the ſmelting-houſes is a rolling-mill, upon the ſame conſtruction as malt-mills, for grinding the materials for fire-bricks; theſe conſiſt of fragments of old fire-bricks, with clunch (a kind of magneſian clay found in coal-pits) procured from near Bangor-ferry.

The port of Amlwch is chiefly artificial, being cut out of the rock with much labour and expence, and is capable of containing 30 veſſels of two hundred tons burthen; it is greatly expoſed, and dangerous of acceſs during high northerly winds, which drive a heavy ſea up the neck of the harbour. The two companies employ 15 brigs from 100 to 150 tons burthen, beſides ſloops and other craft, all of which lie dry at low water.

The various articles, the produce of the mines, which are exported, are the following:

I. Coarſe regulus of copper from the ſmelting-houſes.
II. The richer copper ore roaſted.
III. The dried precipitate of copper from the vitriol pits.
IV. Refined ſulphur.
V. Ochre.
VI. Alum.
VII. Green vitriol.

The town of Amlwch, which about 30 years ago had no more than half a dozen houſes in the whole pariſh, now ſupports a population of four or five thouſand inhabitants; and was at preſent, being market-day, thronged with miners and country-people. After dinner we walked along the ſea ſhore, climbing the ſteep ſlate rocks, whence the water below appeared of a beautiful green, and ſo tranſparent as to ſhew the ſhelving rocks to a great depth beneath.

Having heard that at Camlyn Bay, about eight miles weſt of Amlwch, there were ſome marble quarries, and that it furniſhed aſbeſtos, we reſolved to ſpend this day in viſiting it: the road lay in general about half a mile from the coaſt; the ſubſtratum was waved green magneſian ſlate. When we arrived at Camlyn Bay, we looked in vain for marble or aſbeſtos, and proceeded homewards along the coaſt. The ſhore of Camlyn Bay conſiſts entirely of green and purple waved magneſian ſlate rock, with large veins of quartz. Having arrived at a promontory that ſeparates Cemmaes Bay from the former, we found it to conſiſt of a fine blue-veined limeſtone, or common marble. Some way on, near the village of Cemmaes, this limeſtone is cut through by a ſtratum deſcending to the water, about 40 yards wide, of black ſlate, containing iron pyrites; and in the caverns dug in this, probably in a fruitleſs ſearch after metals, are effloreſcences of ſulphate of iron and chalybeate ſprings. To this ſucceeds a beautiful water-grey ſand, mixed with lime but of little coherence, on expoſure to the air, taking an ochrey ſtain. Adjoining to this are a few yards of calcareous free-ſtone, and then a cliff of very hard white and water-grey marble; a range of ſand and looſe free-ſtone ſucceeds, and the bay terminates with a marble promontory. The ſoil of the land ſurrounding the bay is for the moſt part, eſpecially near the village, a deep ſand. The limeſtone terminates ſhortly after, and the green-waved magneſian ſlate continues the boundary of the iſland. This ridge of lime is in general higher than the ſlate, deſcribing an irregularly indented line of coaſt, about four miles long: its breadth varies from a quarter to half a mile; and a narrow valley, forming its outline towards the land, ſeparates it entirely from the aſbeſtine ſlate, thus preventing any intermediate ſtrata.

The whole of this coaſt is cut out into bays or receſſes of various forms and dimenſions, with lofty projecting promontories, which are for the moſt part fine ſheep-walks. A number of iſlands alſo are formed by ledges of rock, many of them a good way out at ſea, and at high water juſt appearing like black ſpots in the midſt of the waves: many of theſe creeks are ſecure havens for ſmall veſſels, which are protected from weſt and ſouth-weſt winds by the rocks. The village of Cemmaes ſtands upon a little creek opening into a moſt beautiful bay about a mile acroſs; its entrance into the main ſea is guarded on each ſide by a craggy promontory, the one of grey, the other of ſnow-white marble, gliſtening above the green ſea, ſmooth as the ſurface of a mirror, and whoſe ſparkling tranſparency baffles deſcription. In the interior receſs of the bay, the bank of black ſlate, mentioned above, was finely contraſted with a lofty irregular projecting arch of white marble, pierced by the conſtant daſhing of the waves; while the ſounds of laughter and merriment, proceeding from two boats' crews of young people, that had juſt puſhed out of the creek on a party of pleaſure, added double life and intereſt to this lovely ſcene. The land adjoining the cliffs, that overlook the ſea, produces a great deal of corn, chiefly oats and barley. A golden tinge already begins to appear, that will uſher in the harveſt, as ſoon as the crop of hay with which the farmers are now buſied is ſafely houſed.

As we approached Amlwch, we were much pleaſed with ſeeing the ſcars of rock between the town and ſea occupied by numerous groupes of men, women and children, all neat and in their beſt clothes, it being Sunday, who were enjoying the mild temperature of a ſummer evening rendered refreſhing by the neighbourhood of the ſea. In one place we obſerved a circle of men gathered round a point of rock, in which was ſeated the orator of the party reading a newſpaper aloud, and commenting upon it; on other little eminences were ſeen family parties, the elder ones converſing, and the younger children gamboling about them, or running races with each other: in a new-mown meadow cloſe to the town, we paſſed by a large company of lads and laſſes ſeated on a green bank, chatting, laughing, and full of mirth and frolic. To one who had been a ſpectator of the groſs and riotous delight too frequent on holyday-evenings in the outſkirts of the metropolis, or any large town in England, the contraſt could not fail of being very ſtriking, and much to the advantage of the inhabitants of Amlwch: out of the whole number we did not ſee one drinking party; the pleaſures of ſociety and mutual converſe needed not the aid of intoxication to heighten their reliſh.

> Mean time the ſong went round, and dance and ſport,
> Wiſdom and friendly talk, ſucceſſive, ſtole
> Their hours away: while in the roſy vale
> Love breath'd his infant ſighs, from anguiſh free,
> And full replete with bliſs; ſave the ſweet pain
> That inly thrilling but exalts it more.
>
> Harmonious Nature too look'd ſmiling on:
> Clear ſhone the ſkies, cool'd with eternal gales,
> And balmy ſpirit all. THOMSON.

I am acquainted with no place the manners of whoſe inhabitants are ſo unexceptionable (as far at leaſt as a ſtranger is enabled to judge of them) as Amlwch; and the favourable opinion which I was led to entertain of them, on viſiting the town laſt year, is confirmed by what I have obſerved at preſent. Not a ſingle inſtance have I known of drunkenneſs; not one quarrel have I witneſſed during two very crowded market days, and one of them a day of unuſual indulgence, that I paſſed at this place; and I believe no gaol or bridewell, or houſe of confinement, exiſts in the town or neighbourhood. Moſt of the miners are methodiſts, and to the prevalence of this religious ſect is chiefly to be attributed the good order that is ſo conſpicuous. Men who have been long confirmed in habits of vice and irregularity, need arguments the moſt potent that can be offered to counterbalance the aſſociated power of habit and inclination: were it poſſible forcibly to tear them from their connections, and to place them in an entirely different ſituation, reaſon might then be called in gradually to perfect the cure; but where this cannot be done, (and in moſt caſes it is impracticable,) what argument can be urged of ſuch overbearing force as to combat with and overthrow the moſt rooted propenſities, even upon their own territory, unaſſiſted by external coercion, except a ſtrong and impreſſive appeal to their hopes and fears; and, by pre-

ſenting

ſenting both exaggerated and in full contraſt, to overwhelm the mind by ſurpriſe and alarm?

After ſupper we ſtrolled up to the mountain, which now no longer reſounded with the confuſed noiſe of pickaxes and hammers; all was huſhed in profound ſilence; and the moon-beams, which were reflected bright from the ſides of the vaſt excavations, could ſcarcely penetrate the deep abyſs below. As we returned we were ſtruck with the clear red vivid flames iſſuing in a large body from the long range of ſmelting-houſes on the coaſt, and caſting their rays to a great diſtance.

VIII.

A Method of diſpoſing GUNTER'*s Line of Numbers, by which the Diviſions are enlarged, and other Advantages obtained.*

OF the many ingenious inſtruments for computation which were in uſe during the laſt century, among mathematicians, ſcarcely any are to be found at preſent except the ſector and the logarithmic line of Gunter. The eaſe and accuracy of computation by thoſe admirable numbers have rendered the others of little importance; but the ſector has maintained its ſtation from its utility in graphical operations, and the Gunter's line is not only of great value in nautical and other proportions which do not exceed three places of figures, but alſo as a check to aſſure the truth of the leading figures in more extended calculations. About ten years ago I communicated to the Royal Society a method of extending the range of this laſt inſtrument, which I ſtill conſider as leſs generally known than its utility may perhaps claim. The principles depend on the following conſiderations:

1. If two geometrical ſeries of numbers, having the ſame common ratio, be placed in order with the terms oppoſite each other, the ratio between any term in one ſeries and its oppoſite in the other will be conſtant[*].

2. And the ratio of a term in one ſeries to any term in the other, will be the ſame as obtains between any other two terms having the ſame relative poſition and diſtance[†].

3. In all ſuch pairs of geometrical ſeries as have the ſame common ratio, the laſt-mentioned property obtains, though the firſt antecedent and conſequent be taken in one pair, and the ſecond in any other pair[‡].

4. If the differences of the logarithms of numbers be laid in order upon an arrangement of equi-diſtant parallel right lines, in ſuch a manner as that a right line drawn acroſs the

---

[*] Geom. ſeries $\begin{cases} a \; an \; an^2 \; an^3 \; an^4 \\ b \; bn \; bn^2 \; bn^3 \; bn^4 \end{cases}$

Then $a : b :: an : bn :: an^2 : bn^2$, &c.

[†] In the foregoing ſeries $a : bn^2 :: an^2 : bn^4 :: an : bn^3$, &c.

[‡] Geom. ſeries $\begin{cases} a \; an \; an^2 \; an^3 \; an^4 \\ b \; bn \; bn^2 \; bn^3 \; bn^4 \end{cases}$

Geom. ſeries $\begin{cases} d \quad dn \quad dn^2 \quad dn^3 \quad dn^4 \\ \frac{bd}{a} \quad \frac{bdn}{a} \quad \frac{bdn^2}{a} \quad \frac{bdn^3}{a} \quad \frac{bdn^4}{a} \end{cases}$

Then $a : bn^3 :: dn : \frac{bdn^3}{a}$, &c.

whole ſhall interſect it at diviſions which denote numbers in geometrical progreſſion; then, from the condition of the arrangement, and the property of this logarithmic line, it follows, firſt, that every right line ſo drawn will, by its interſections, indicate a geometrical ſeries of numbers *; ſecondly, that ſuch ſeries as are ſo indicated by parallel right lines, will have the ſame common ratio †; and thirdly, that the ſeries thus indicated by two parallel right lines, ſuppoſed to move laterally without changing either their mutual diſtance, or paralleliſm to themſelves, will have each the ſame common ratio; and, in all pairs of ſeries indicated by ſuch two lines, the ratio between an antecedent on one parallel and the oppoſite term on the other, taken as a conſequent, will be conſtant ‡.

5. In the foregoing paragraphs the logarithmic line has been conſidered as unlimited. On ſuch a line, therefore, any antecedent and conſequent being given, it would be poſſible to find both on the arrangement, and to draw two parallel lines, one over each number: and if the lines be then ſuppoſed to move without changing either their diſtance or abſolute direction, ſo that the line, which before marked an antecedent, may in the ſecond ſtation mark

---

* Let AB, CD, EF (Plate XVI. Fig. 1.) be portions of the logarithmic line arranged according to the condition; let GH be a right line drawn acroſs, ſo as to paſs through points of diviſion *e*, *c*, *a*, denoting numbers in geometrical progreſſion; then will any other line IK, drawn acroſs the arrangement, alſo paſs through points *f*, *d*, *b*, denoting numbers in geometrical progreſſion.

DEMONSTRATION. From one of the extreme points of interſection *f*, in the laſt named line IK, draw the right line *fg* parallel to GH, and interſecting the arrangement in the points *i*, *b*; and the ratios of the numbers *e* : *f*, *c* : *i*, and *a* : *b*, will be equal, becauſe the intervals on the logarithmic line, or differences of the logarithms of theſe numbers, are equal:

$$\text{Or } \frac{e}{c} = \frac{f}{i} \text{ and } \frac{e}{a} = \frac{i}{b}.$$

$$\text{But } \frac{e}{c} = \frac{c}{a} \text{ by the condition.}$$

Therefore $\frac{f}{i} = \frac{i}{b}$; or the numbers *f*, *i*, *b*, are in the ſame continued ratio as the numbers *e*, *c*, *a*.

Again, the point *f*, the line *id*, and the line *bb*, are in arithmetical progreſſion, and denote the differences of the logarithms of the numbers *f* and *f*, *i* and *d*, *b* and *b*.

The quotients of the numbers themſelves are therefore in geometrical progreſſion, that is,

$$\frac{f}{f} : \frac{d}{i} : \frac{b}{b}, \text{ or } \frac{i}{d} = \frac{db}{bi}.$$

$$\text{Or } \frac{i}{d} = \frac{di}{bf}, \text{ by ſubſtituting } \frac{i}{f} \text{ for its equal } \frac{b}{i}.$$

$$\text{Whence } \frac{f}{d} = \frac{d}{b} \text{ or } f : d : b. \qquad \text{Q. E. D.}$$

† In the ſame manner, as it was proved that the line *fg* parallel to GH paſſes through points of diviſion denoting numbers in the ſame continued ratio as thoſe indicated by the line GH, it may alſo be ſhewn, that the line LM, parallel to any other line IK, will paſs through a ſeries of numeral points having the ſame continued ratio as the ſeries indicated by that line IK to which it is parallel.

‡ Becauſe the lines preſerve their paralleliſm to their former ſituation, they will indicate geometrical ſeries having the ſame common ratio as before; and, becauſe their diſtance meaſured on the logarithmic line remains unchanged, the differences of the logarithms of oppoſite numbers, and conſequently their ratio, will be conſtant.

mark a new antecedent, the other (by 2. and 3.) will mark a number at the ſame relative poſition and diſtance, which will be the conſequent to this laſt antecedent, after the ſame ratio.

6. Suppoſe a logarithmic line to contain no more than a ſingle range of numbers from 1 to 10, it will not be neceſſary, for the purpoſes of computation, to repeat it; for, if a ſlider or beam have two fixed points at the diſtance of the interval between 1 and 10, and a moveable point be made to range between theſe (always to indicate the antecedent), in this caſe, if the conſequent fixed point fall without the rule, the other fixed point will ſhew the diviſion it would have fallen on if the rule had been prolonged. This may be eaſily applied to the arrangement deſcribed, N° 4.

7. If the arrangement conſiſt only of the logarithms from 1 to 10, and the parallel croſs lines interſect that geometrical ſeries whoſe ſucceſſive ratios altogether, with that of the laſt to the firſt, make by compoſition the ratio $\frac{1}{10}$, the contrivance N°. 6. may be applied to ſhew ſuch conſequents as fall, laterally, without the rule.

8. It will be convenient that the arrangement of the lines ſhould be diſpoſed ſo as to occupy a rectangular parallelogram; or, in other words, that the croſs line, cutting the ſeries laſt mentioned, may be at right angles to the length of the rule.

The conſtruction of an inſtrument on the principles here explained will admit of various diſpoſitions of the graduated lines and apparatus for meaſuring intervals upon them. In the Tranſactions I gave a figure of a rule conſiſting of ten parallel lines, equivalent to a double line of numbers, upwards of twenty feet in length, with a beam compaſs for meaſuring intervals.

Fig. 2. Plate XVI. repreſents a Gunter's ſcale, equivalent to that of $29\frac{1}{2}$ inches in length, publiſhed by the late Mr. Robertſon. It is, however, but one eighth part of the length, and contains only one-fourth of the quantity of diviſion. In the ſlider GH is a moveable piece AB, acroſs which a fine line is drawn; and there are alſo lines CD, EF, drawn acroſs the ſlider, at a diſtance from each other equal to the length of the rule. The ſketch No. 1. repreſents one face or ſide of the inſtrument, and No. 2. repreſents the oppoſite face. Each contains one-fourth part of the line of numbers. When it is uſed, the ſlider muſt be ſet ſo that the line on the piece AB may be placed at the antecedent, and one of the end marks CD, or EF, may be oppoſite the conſequent. After this adjuſtment of the ſlider, the whole may be moved at pleaſure, till the piece AB is ſet at any other required antecedent; and then the ſame line CD or EF, as before, will indicate the conſequent at the ſame diſtance or poſition as before. But if the conſequent mark of the ſlider ſhould fall without the rule, the other line will indicate the required conſequent upon the rule, though at the diſtance of one line on the rule farther off in poſition than the other conſequent mark would elſe have ſhewn it. The operations are obvious and familiar upon the rule itſelf.

Another inſtrument was deſcribed in the Tranſactions, which was equivalent to the ſame rule, but of a circular figure, one inch and a-half diameter. The graduations were made upon three concentric circles, and a ſector was applied inſtead of the ſlider. On account of the larger figure I have here given, it becomes unneceſſary to deſcribe this.

I approve of this conſtruction as ſuperior to every other which has yet occurred to me, not only in point of convenience, but likewiſe in the probability of being better executed, becauſe ſmall arcs may be graduated with very great accuracy, by diviſions transferred from a larger original.

The circular inſtrument is a combination of the Gunter's line and the ſector, with the improvements here pointed out. The property of the ſector may be uſeful in magnifying the differences of the logarithms in the upper part of the line of ſines, the middle of the tangents, or the beginning of the verſed ſines. It is even poſſible, as mathematicians will eaſily conceive, to draw ſpirals, on which graduations of parts, every where equal to each other, will ſhew the ratios of thoſe lines by means of moveable radii ſimilar to thoſe on ſuch an inſtrument.

After the publication of the account in the Tranſactions of the Royal Society, the mention of circular and ſpiral inſtruments brought to the recollection of the late Mr. George Adams, of Fleet-ſtreet, (who had ſeen the ſtraight inſtrument ſome years before it was communicated to the Society,) that he had a ſpiral engraved on a braſs plate by his father. He made me a preſent of the plate, and ſhewed me, by a manuſcript, that it was conſtructed in the year 1748. The ſpiral has ten turns, and its external diameter is twelve inches. It contains the numbers, ſines and tangents, the latter being twice repeated, and may with eaſe be uſed to compute to four places and an eſtimate figure; which is to the full as much as could be done with a common Gunter's ſcale of ſixty feet in length.

Fig. 3. Plate XVI. repreſents the line of numbers drawn according to this ſyſtem. The ſector ACB is uſed to meaſure the ratios. One thread muſt be ſet to the antecedent, and the other to the conſequent. If the antecedent thread be then removed without altering the angle to any other antecedent, the other thread will mark a conſequent at the diſtance of the ſame number of turns of the ſpiral in the ſame direction. In caſe the number of turns ſhould proceed without the ſyſtem, it will be neceſſary to return and reckon onward from the oppoſite extremity of the ſpiral to complete the number.

IX.

On the Mechanical Projects for affording a Perpetual Motion.

IN conſequence of the notice * taken of Mr. Varley's attempt to produce a perpetual motion, I have been requeſted by ſeveral correſpondents to ſtate how far the mechanical ſcheme for which Dr. Conrad Shiviers took out a patent in the year 1790, for the ſame object, may be worthy of attention. I have, on that occaſion, mentioned the difficulties which have prevented any clear general demonſtration of the abſurdity of this purſuit from being produced, though it has not been difficult to ſhew the fallacy of the individual plans. It does not indeed ſeem eaſy to enunciate the ſcheme itſelf. What in univerſal terms is the thing propoſed to be done? Is it to cauſe a body, or ſyſtem of bodies, to act in ſuch a manner that the re-action ſhall be greater than the action itſelf, and by that means generate force by the accumulation of the ſurplus? Or, can the motion communicated be greater than that loſt by the agent? Since theſe poſitions are evidently contrary to the phyſical axioms called the Laws of Nature, and frictions and reſiſtances would ſpeedily deſtroy all motions of ſimple uniformity, it may be preſumed that s'Graveſande, who thought that all the demonſtrations of the abſurdity of ſchemes for perpetual motion contained paralogiſm, would have ſtated the propoſition under different terms. But without entering into this apparently unprofitable diſquiſition, it may be uſeful, as well as entertaining, to make a few obſervations on the mechanical contrivances which depend on a miſtaken deduction from the general theorem reſpecting the balance, among which that of Dr. Shiviers muſt be claſſed.

---

* Philoſ. Journ. I. 334.

There is no doubt but numerous arrangements have been made, and ſtill are laboured at by various individuals, to produce a machine which ſhall poſſeſs the power of moving itſelf perpetually, notwithſtanding the inevitable loſs of force by friction and reſiſtance of the air. Little, however, of theſe abortive exertions has been entered upon record. The plans of Biſhop Wilkins, the Marquis of Worceſter, and M. Orfyreus, are all which at this time occur to my recollection.

There is no doubt but the celebrated Wilkins was a man of learning and ability. His Eſſay towards a real character and a philoſophical language is ſufficient to render his name immortal. Twenty years before the appearance of that work he publiſhed his Mathematical Magic, namely in the year 1648, containing 295 pages ſmall octavo, which, from the number of copies ſtill in being, I ſuppoſe to have been a very popular treatiſe. It is in this work that I find, among other contrivances for the ſame purpoſe, a wheel carrying ſixteen loaded arms, ſimilar to that delineated in fig. 4, plate xv. in which, however, for the ſake of ſimplicity, I have drawn but ſix. Each lever A B C D E F is moveable through an angle of 45°, by a joint near the circumference of the wheel, and the inner end or tail of each is confined by two ſtuds or pins, ſo that it muſt either lie in the direction of a radius, or elſe in the required poſition of obliquity. If the wheel be now ſuppoſed to move in the direction EF, it is evident that the levers A B C D, by hanging in the oblique poſition againſt the antecedent pins, will deſcribe a leſs circle in their aſcent, than when on the other ſide they come to deſcend in the poſitions EF. Hence it was expected that the deſcending weights, having the advantage of a longer lever, would always predominate. Dr. Wilkins, by referring the weights to an horizontal diameter, has ſhewn that in his machine they will not. A popular notion of this reſult may alſo be gathered from the figure, where there are three weights on the aſcending, and only two on the deſcending ſide; the obliquity of poſition giving an advantage in point of number, equal to what the other ſide may poſſeſs in intenſity. Or if this contrivance were to be ſtrictly examined, on the ſuppoſition that the levers and weights were indefinitely numerous, the queſtion would be determined by ſhewing that the circular arcs AK, HI, are in equilibrio with the arcs AG, GL.

The ſimpleſt method of examining any ſcheme of this kind with weights, conſiſts in enquiring whether the perpendicular aſcents and deſcents would be performed with equal maſſes in equal times. If ſo, there will be no preponderance, and conſequently no motion. This is clearly the caſe with the contrivance before us.

The Marquis of Worceſter, who will ever be remembered as the inventor of the ſteam engine, has deſcribed a perpetual motion in the 56th number of his Century of Inventions, publiſhed in the year 1655, and ſince reprinted in 1767 by the Foulis's at Glaſgow. His words were as follow:

"To provide and make, that all the weights of the deſcending ſide of a wheel ſhall be perpetually further from the centre than thoſe of the mounting ſide, and yet equal in number, and heft to the one ſide as the other. A moſt incredible thing if not ſeen, but tried before the late King (of bleſſed memory) in the Tower by my directions, two extraordinary ambaſſadors accompanying his Majeſty, and the Duke of Richmond and Duke Hamilton, with moſt of the Court attending him. The wheel was 14 feet over, and 40 weights of 50 pounds a piece. Sir William Balfour, then Lieutenant of the Tower, can juſtify it with ſeveral others. They all ſaw, that no ſooner theſe great weights paſſed the diameter line of the lower ſide, but they hung a foot further from the centre; nor no ſooner paſſed the diameter line of the upper ſide, but they hung a foot nearer. Be pleaſed to judge the conſequence."

Deſaguliers, in his courſe of Experimental Philoſophy, vol. I. page 185, has quoted this paſſage, and given a ſketch of a pretended ſelf-moving wheel, ſimilar to that of fig. 5, plate xv. as reſembling the contrivance mentioned by the Marquis of Worceſter. The deſcription of this laſt engineer agrees, however, ſomewhat better with the contrivance fig. 4. It muſt of courſe be a miſtake in terms, when he ſays the weight receded from the centre at the lower diameter, and approached towards it at the upper: the contrary being in fact neceſſary to afford any hope of ſucceſs; and accordingly in the quotation it is ſo ſtated. I am therefore diſpoſed to think that fig. 5 repreſents the wheel of Orſyreus at Heſſe Caſſel, much talked of about the year 1720, and which probably was made to revolve, during the time of exhibition, by ſome concealed apparatus. It conſiſts of a number of cells or partitions diſtinguiſhed by the letters of the alphabet, which are made between the interior and exterior ſurfaces of two concentric cylinders. The partitions being placed obliquely with reſpect to the radius, and a cylindrical or ſpherical weight placed on each, it is ſeen from the figure, that theſe weights will lie againſt the inner ſurface of the large cylinder, whenever the outer end of the bottom partition of any cell is loweſt; and on the contrary, when that extremity is higheſt, the weight will reſt on the ſurface of the interior cylinder. Let the wheel be made to revolve in the direction A B C; the weights in C D E F G H I being cloſe to the external circle, and the weights K L M A B cloſe to the inner, for the reaſons laſt mentioned. As the cell B deſcends, its weight will likewiſe run out, at the ſame time that the weight in the cell I will run in, in conſequence of its partition being elevated. By the continuation of this proceſs, ſince all the weights on the deſcending ſide paſs down at a greater diſtance from the centre, while thoſe on the aſcending ſide riſe for a conſiderable part of their aſcent at a leſs diſtance from the ſame point, it is concluded that the wheel will continue to maintain its motion. On this, however, it is to be remarked, that the perpendicular aſcent and deſcent are alike, both in meaſure, and in time of performance; and that the familiar examination, even to thoſe who know little of ſuch ſubjects, is ſufficient to ſhow that the preponderance is not quite ſo palpable as at firſt it appears. For the weights G and F, H and E, I and D, are evidently in equilibrio, becauſe at the ſame horizontal diſtance from the centre; and if the favourable ſuppoſition that the weight B hath already run out be admitted, it will then remain a queſtion whether theſe two exterior weights, B and C, can preponderate over the four inner weights K L M A. The more accurate examination of this particular contrivance will lead to the following theorem. In two concentric circles, if tangents be drawn at the extreme points of a diameter of the ſmaller, and continued till they interſect the larger, the common centre of gravity of the arc of the greater circle included between the tangents, and of the half periphery of the ſmaller circle on the oppoſite ſide of the diameter, will be the common centre of the circles. If, therefore, the balls were indefinitely numerous and ſmall, the ſuppoſed effective parts of the wheel, fig 5, would be in equilibrio, as well as the parts beneath the horizontal tangent of the inner circle.

Fig. 6 repreſents the contrivance of Dr. Shiviers, which, in a periodical publication, in other particulars reſpectable, has been ſaid to continue in motion for weeks, and even months together. There is not the ſmalleſt probability that it ſhould continue in motion for half a minute, or nearly as long as a ſimple wheel would retain part of its firſt impulſe. The external circle denotes a wheel carrying a number of buckets, A B I L, &c. C repreſents a toothed wheel, on the ſame axis, which drives a pinion D; and this laſt drives another pinion E upon the axis of a lanthorn, or wheel intended to work a chain pump with the ſame number of buckets as in the large wheel A B I. The lanthorn G is made of ſuch a ſize as to raiſe the buckets a b i l with a due velocity. K repreſents a gutter, through which a metallic ball, contained in the bucket m may run and lodge itſelf in the bucket A of the wheel. Each of the buckets of the wheel B I L M, which are below the gutter, is ſupplied with a metallic ball, and ſo likewiſe are the aſcending buckets a b i l m of the chain-pump. As the pump ſupplies the wheel, it is itſelf again ſupplied at M, where the balls fall into its aſcending buckets. Now it is preſumed that the balls in the wheel, I ſuppoſe on account of their diſtance from the centre of motion, will deſcend with more than ſufficient force to raiſe thoſe on the chain, and conſequently that the motion will be perpetual.

The deception in this contrivance has much leſs ſeduction than in the two foregoing, becauſe it is more eaſily referred to the ſimple lever. This, like the others, exhibits no proſpect of ſucceſs, when tried by the ſimple conſideration of the equality of the aſcent and deſcent in the whole time of the rotation of a ſingle ball. It may alſo be ſhewn from the principles of wheel work, which are familiar to artiſans, that whatever is gained by the exceſs of the diameter of the great wheel beyond that of the wheel C, is again loſt by the exceſs of the lanthorn A beyond the pinion E.

The fundamental propoſition of the ſimple lever or balance, that equal bodies at an equal diſtance from the fulcrum will equiponderate, but that at unequal diſtances the moſt remote will deſcend, has in theſe and numberleſs other inſtances led mechanical workmen and ſpeculators to purſue this fruitleſs enquiry with labour and expence often ill-afforded, and with a degree of anxiety and infatuation which can hardly be conceived by thoſe who have never ſuffered the pain of hope long deferred. For this reaſon chiefly, it has appeared deſirable and uſeful to treat the ſubject in a familiar way, without deſcending to thoſe expreſſions of contempt, which ignorance, harmleſs to all but itſelf, is ſurely not entitled to. If ſuch reaſoners were well convinced that the power of a machine is to be eſtimated by the exceſs of motion referred to the perpendicular, without any regard to the apparent centre of the machine, and that in machines very little compounded it is poſſible to produce effects directly contrary to the rule which is true of the ſimple lever, they would probably renounce many flattering projects, grounded only on the ſuppoſition of its univerſality. Deſaguliers contrived an apparatus in which two equal weights may be placed at any diſtance whatever from the centre of motion, and ſtill continue in equilibrio. Fig. 3 repreſents this inſtrument, AD denotes a balance with equal arms, and EF another of the ſame dimenſions. Theſe move on the centres B and C, and are connected by the inflexible rods AE and DF; the motion being left free by means of joints at the corners. Acroſs the rods AD, EF, are fixed two bars IK, LM. Now it is unneceſſary to ſhew that the weight G will deſcribe exactly the ſame line or circular arc, when the levers are moved into the poſition a d f e, or any other poſition, as it would have deſcribed in caſe it had been ſuſpended at A, or K, or E; and that it is of no conſequence in this reſpect at what part of the line AE, or IK, it be fixed. The ſame obſervations are true of the weight H on the other ſide. And accordingly it is found, that theſe equal weights may be ſuſpended any where on the lines IK and LM without altering their equilibrium.

By this contrivance it is moſt evidently proved, to thoſe who are totally unacquainted with the theory, that weights do not preponderate in compound engines, on account of their diſtance from the centre. Several other contrivances may be made to the ſame effect. The following combination of wheel-work preſented itſelf to me as one which would moſt probably be miſtaken for a perpetual motion. Fig. 2. Plate XV. The five circles repreſent the ſame number of wheels, of equal diameter and number of teeth, acting together. The middle wheel A is fixed between two upright pillars, ſo that it cannot revolve. The other four wheels are pinned in a frame HI, in which they can revolve, and through which the axis of A likewiſe paſſes. From the extremity of the axis of D, and alſo of d, proceed the horizontal levers HK and IL, which are equal, and point in the ſame direction parallel to the plane of the wheels. At the extremity of theſe arms hang the equal weights P and p. Let it now be imagined that the end I of the frame is depreſſed; the wheel B will turn round by the re-action of the fixed wheel A in the ſame direction as HI, and it will make one revolution, in the ſame time, relative to the frame, or two with regard to abſolute ſpace by reaſon of its being carried round. The action of B upon D will produce a rotation relative to the frame in the oppoſite direction during the ſame time. Inſtead therefore of two revolutions, like the wheel B, this wheel D, with regard to abſolute ſpace, will not revolve at all, and in every poſition of the apparatus the arm IL will continue horizontal, and point the ſame way. For ſimilar reaſons the arm HK will retain its poſition. Conſequently, it is ſeen that the deſcending weight will move at a great horizontal diſtance from the centre N, while the aſcending weight riſes very near that centre. But there will not on this account be a perpetual motion: for the actions of the levers HK and IL upon the frame HI, by means of the toothed wheels, will in the detail be found preciſely alike, and in the general conſideration of the motions of P and p, the oppoſite motions in the circle E F G will be accurately the ſame.

It has always been conſidered as eſſential to a perpetual motion, that it ſhould be derived from ſome energy which is not ſuppoſed to vary in its intenſity. Such are the inertia, the gravity or magnetiſm of bodies. For an occaſional or periodical variation of intenſity in any force is evidently productive of motion, which requires only to be accumulated or applied, and the apparatus for applying it cannot be conſidered as a machine for perpetual motion. Neither in ſtrictneſs can any machine, whoſe motion is derived from the motion or rotation of the earth, and the conſequent change of ſeaſons and rotation of events, be ſo conſidered, becauſe it does not generate, but only communicates. The perpetual flow of rivers, the viciſſitudes of the tides, the conſtant, periodical and variable winds, the expanſions and contractions of air, mercury, or other fluids, by daily or other changes of temperature, the differences of expanſion in metals by the ſame change, the riſe and fall of the mercury in the barometer, the hygrometric changes in the remains of organized beings, and every other mutation which continually happens around us, may be applied to give motion to mills, clocks, and other engines, which may be contrived to endure as long as the apparatus retains its figure.

X.

Uſeful Notices reſpecting various Objects.—Silver alloyed with Crude Platina.—Tempering of Steel.—Rifled Shot.

1. *Silver alloyed with Crude Platina.*

BERGMAN, in his Treatiſe on the Blow-Pipe, in one of the earlier editions, directed that the ſpoon for blow-pipe experiments ſhould be made of ſilver alloyed with one tenth of platina, of which the purification was at that time little known. Dr. Lewis, in his Philoſophical Commerce of Arts, mentions the fuſion of ſilver with crude platina. The metals united but imperfectly, with a remarkable projection of particles of the metal, as if by a kind of ebullition over the inſide of the crucible. Several years ago, being deſirous of making ſuch a ſpoon, I ſelected ten grains of crude platina, in particles poſſeſſing very little magnetiſm, and fuſed the ſame with one hundred grains of pure ſilver in a blaſt furnace, uſing a large proportion of nitre, with the intention of ſcorifying any of the baſer metals. The effect which Lewis mentions took place; and the compound, when poured out, had a ſcabrous or unſound appearance. I thought the grains of platina might have been merely ſurrounded by the adherent ſilver; but this did not ſeem to be the caſe, for it bore laminating between two ſteel rollers very well. After this laſt proceſs, I ſubjected it to fuſion again with a ſtronger heat, and again laminated it into a thin plate. This operation of fuſing and rolling was repeatedly performed, but ſtill the metal appeared rough in certain parts of the ſurface. As a laſt effort, I therefore expoſed it to the moſt violent heat I could urge, and determined to leave it to cool in the crucible beneath the nitre which flowed above it. The crucible in the ignited ſtate was taken, from ſome motive I cannot now recollect, to a window, and ſet down upon a tile. As I ſtood attentively obſerving the appearance of the metallic globule through the tranſparent and tranquil bath of nitre, the ignition gradually went off, ſo as to be ſcarcely viſible in that clear light. But on a ſudden it recovered itſelf in an inſtant, and the nitre boiled up ſo as to fill the vacant ſpace of the crucible. The button of metal, when cooled, was ſcabrous; but I nevertheleſs formed it into a ſpoon, by rolling, hammering, and afterwards poliſhing it. It was then expoſed to a low red heat, in a common fire, and became bliſtered all over.

The above facts poſſeſs the utility which attends unſucceſsful experiments, namely, that the narration may ſave others from a repetition of the labour. But, philoſophically ſpeaking, there are no new experiments which would be unſucceſsful if we thoroughly underſtood them. I think the decay and recovery of ignition in this is a curious inſtance of what ſeems to be a general law of the congelation of fluids. It is probable that all fluids, as well as water, are capable of being cooled below their freezing point, and afterwards become hotter by the eſcape of latent heat when they congeal. Thus water cooled below 32 degrees is ſuddenly raiſed to that temperature the inſtant ice is formed, becauſe the ice gives out the difference of the heat which was latent in the fluid ſtate. And ſo it appears to have happened with the fluid metallic compound in theſe experiments. As its temperature was diminiſhed, it became leſs luminous or ignited; but, at the inſtant of congelation, that portion of heat which had been employed in maintaining the fluid ſtate, was extricated, and became employed in raiſing the temperature. The effect of this increaſe was ſeen in the greater emiſſion of light, and the boiling of the nitre.

2. *Tempering of Steel.*

INSTRUMENTS of ſteel are required to be hard, in order that they may penetrate and divide the ſubſtances intended to be cut; and tenacious, that they may not break during the operation. The hardeſt ſteel is the moſt brittle; for which reaſon, though hardneſs would in every caſe be a deſirable quality, yet, for the ſake of tenacity, it is in many inſtances neceſſary to diminiſh it. We ſee, therefore, that there muſt be a preciſe mean between too ſoft and too brittle, which will be beſt ſuited for the reſpective purpoſes to be accompliſhed. A ſpring muſt be tenacious, and need not be very hard. A knife for cutting leather, and ſoft ſubſtances, muſt be ſomewhat harder than a ſpring. Pen-knives and razors muſt be ſtill harder; and files and tools for working metal muſt be hardeſt of all, though even in theſe care muſt be taken not to deſtroy their tenacity by making them too hard. Steel is hardened by ignition, and ſubſequent plunging in water. The chief art of this proceſs conſiſts in covering the ſteel with ſome mixture, which ſhall prevent its being degraded in the fire to the ſtate of iron. The file-makers uſe the grounds of beer mixed with common ſalt. Others uſe the cementing mixture. No greater heat is to be uſed with any ſteel, than by experience is found ſufficient to produce hardneſs at leaſt equal to that of a file. More heat would render the grain coarſe and open. Urine is thought to be better for quenching the ſteel than water, probably becauſe it may be a better conductor of heat, and perhaps on account of its phoſphoric ingredient, which is now with juſtice ſuppoſed [*] to be an eſſential part of ſteel. When ſteel is not intended to poſſeſs the utmoſt hardneſs, it is afterwards ſoftened by the application of a lower degree of heat. This operation is called tempering. The greateſt difficulty conſiſts in applying the proper degree of heat uniformly over the whole maſs. The common method is to judge by the colour aſſumed by the clean ſurface of the ſteel when thus heated. The heat may be applied by the fire, or a pan of charcoal, or the flame of a candle or lamp, or by laying the piece upon ſand to be gradually heated, or upon melted lead. The ſaw-makers, and ſome makers of ſprings, heat the article, rub it with greaſe, and then heat it ſtill farther till the fumes take fire; this is called blazing, and affords a temper nearly the ſame as when the ſteel by heat has acquired a deep blue colour. When the temper is given from the colour, the firſt tinge which appears is a faint ſtraw colour, which is ſuitable to pen-knives and hard cutting-tools. The next colour, which is purple, is rather too ſoft for a knife, and too brittle for a ſpring. After this follows the blue, of which there are ſeveral ſhades. The deepeſt is very ſoft, and is ſucceeded by a whitiſh yellow, which indicates too great a degree of ſoftneſs for any cutting-tool.

---

[*] Philoſophical Journal.

Mr. Hartley, in the year 1789, took out a patent for a method of tempering ſteel. His ſpecification, which is ſo general as perhaps to include no method at all in the way of monopoly, indicates that the heat is to be meaſured by a pyrometer or thermometer applied near the article. The actual practice of this method appears to conſiſt in uſing oil and a mercurial thermometer. In this way many dozens of razors or tools may be tempered at once with the utmoſt facility. The different degrees of heat for various kinds of ſteel, and their ſeveral uſes, may ſpeedily be determined by experiment. For want of a thermometer graduated to the higher degrees, I have not yet made any experiments. The only fact I have at preſent to communicate is, that Mr. Stodart, who uſes this method, ſtates that the requiſite temper on ſteel for a pen-knife is 450 degrees of Fahrenheit's ſcale *. I find, on trial, that a good pen-knife is as hard as any tool which can admit of tempering. Hard gravers, for turners' uſe, muſt not be ſoftened at all.

3. *Rifled Shot.*

IN the latter end of the year 1789, I was, by various conſiderations, induced to think, that the effect which is produced by rifling muſquetry might be produced in artillery by giving a ſuitable figure to the ſhot. It is almoſt needleſs to explain this effect. When a bullet is driven along the bore of a piece, it muſt be acted upon by the internal ſurface ſo as to cauſe a rotation, the axis of which motion will lie acroſs the line of direction. In conſequence of this, the re-action of the air will be ſtronger on one ſide of the bullet than on the other, and it will deviate from the intended courſe according to no certain rule. The method of rifling conſiſts in cutting one or more ſpiral grooves in the hollow ſurface of the muſquet, into which the ball is either forcibly rammed down, or elſe conveyed to its place by an aperture at the breach, or near the chamber. The lead is thus made to fit the internal ſcrew, and uſually takes about half a turn during its courſe through the barrel. The axis of this rotation being parallel to the line of direction, it muſt follow that the reſiſtance of the air will be equal on all ſides of the bullet, and it will fly with more certainty to the object of aim. It ſeemed to me, that if a cylindrical ſhot, with hemiſpherical ends, were thrown out of a common barrel, it might be poſſible, by means of certain ſpirals cut on the end ſurface, to cauſe the blaſt of the powder and the reſiſtance of the air to concur in producing the ſame rotation.

For this purpoſe I took a wooden pattern, and cut the ſpherical ſurface into twelve ſpiral planes, by dividing the equator into the like number of equal parts, and drawing ſpirals from

---

[*] Philoſophical Tranſactions 1795, p. 326.

the points of diviſion obliquely towards the poles. The wood between every pair of contiguous ſpirals was then taken away, by cutting from the one line parallel to the axis, and from the other perpendicular to a plane paſſing through it. By this proceſs, when the axis was ſet upright, there appeared, as it were, twelve roads ſloping upwards from the equator towards the pole, bounded on the ſide next the wood by upright walls; and the ſhot, when ſuſpended on an axis or centre point, could be blown round very ſwiftly by the breath directed towards the pole.

Shot of this kind were made and tried at a foundery in North Wales. By an experiment with a braſs gun newly bored, it was aſcertained that the ſhot did really revolve in its courſe along the bore; but the trials with ſhot of different weight and dimenſions did not promiſe more accuracy of effect than was obtained by common ſpherical ſhot uſed at the ſame time. Particular notice was taken of the manner in which the ſhot ſtruck the butt: the greateſt number of times, it ſtruck with the anterior end; ſometimes the ſtroke was made with the broad ſide, and, in a few inſtances, the end which came laſt out of the gun arrived firſt at the mark. Hence it appears, that the very ſlight angular deviation at the mouth of the piece is more than ſufficient to counteract any effect which might elſe have been derived from the ſubſequent action of the air upon a projectile duly figured.

It ſeems, nevertheleſs, that this principle might be applied to advantage in bar ſhot. If the ends of this projectile were chamfered or ſloped with reſpect to the axis, it would paſs through the air with a revolution of its extremities, inſtead of one end following the path of the other, as may ſometimes be ſuppoſed to happen.

With regard to the execrable practice of war, I think it a decided queſtion, that increaſe of power is, on the whole, in favour of rectitude and virtue; and that wars are likely to be fewer, leſs durable, and leſs pernicious, the more ſcientifically they are conducted.

MATHEMATICAL CORRESPONDENCE.

QUESTION IX. *Anſwered by J. F—:—:—:—R.*

IN order to a ſolution of this problem, we have only to find the point in the plane of the horizon, which is perpendicularly under the elevated object; the diſtances of which point from the three ſtations are as the cotangents of the angles of elevation taken at each reſpectively. This will be effected by the following

CONSTRUCTION.

LET A, B, and C be the three ſtations (Plate XV. Fig. 1.) and a, b, and c the cotangents of the reſpective angles of elevation taken at each. Produce AB and CB towards E and F, and make BF = BA, and BE = BC. Take FG to FB as c to b, and EG to EB as a to b, and about the centres F and E with the reſpective radii FG and EG deſcribe arcs interſecting in G. Draw FG, BG, EG, and in GB produced take BD to AB as BE to BG. I ſay the point D is the required point in the horizontal plane.

For, draw AD and CD.—Then the triangle GBE being ſimilar to ABD, and GBF to CBD, we have AB (= FB) : $\frac{c}{b}$ AB (= FG) :: b : c :: BD : CD and BC (= BE) : $\frac{a}{b}$ BC (= EG) :: b : a :: BD : AD. Therefore AD, BD and CD being as a, b, and c reſpectively, the point D is rightly found. Q. E. D.

Hence alſo, if with AD (: AB :: EG : BG) and CD (: CB :: FG : BG) as radii, arcs be deſcribed about A and C, they will interſect each other, and each of them will interſect GB produced in the required point.

CALCULATION.

FROM the values of FB, BE, FE, FG, and EG, find BG trigonometrically, and from thence AD, BD, or CD.

THIS queſtion was anſwered in nearly the ſame manner by **Analyticus**, who obſerves, that if the three ſtations are at unequal diſtances from the object, its apparent altitude will vary at each of them, according to the effect of refraction, and of courſe occaſion a threefold error in the reſult; which may be avoided by taking the ſtations ſo that the angles of elevation ſhall be all equal, in which caſe a ſingle refraction only will be concerned, which indeed is ſufficient to render this, or any other ſimilar method of determining the heights of terreſtrial objects, very inaccurate. If, beſides the angles of elevation, thoſe formed by the baſe and hypothenuſes drawn from each extremity of it, to the top of the object, be taken, there is no occaſion for more than two ſtations; which being choſen in ſets, in the way laſt mentioned, will enable us to aſcertain the difference of refraction at different diſtances.

◆

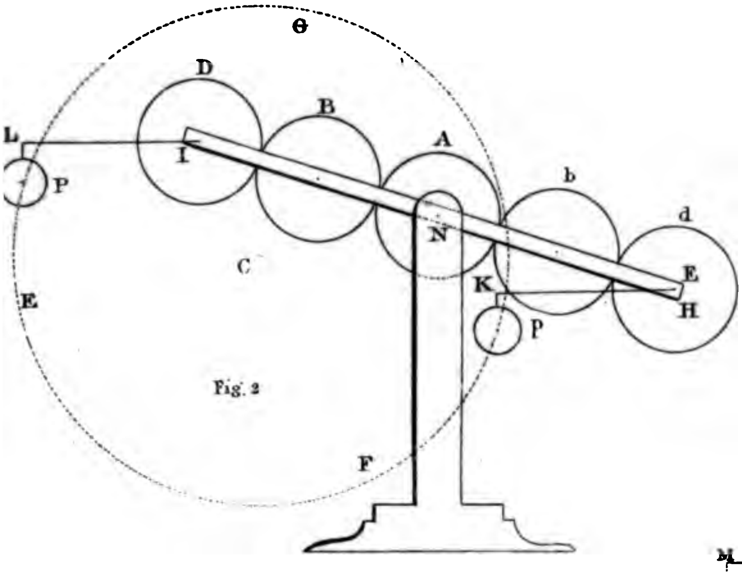


Fig. 2

By Bishop Wilkins

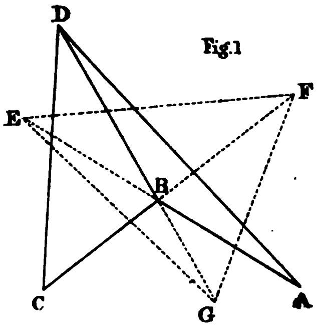


Fig. 1

Compound Ballance

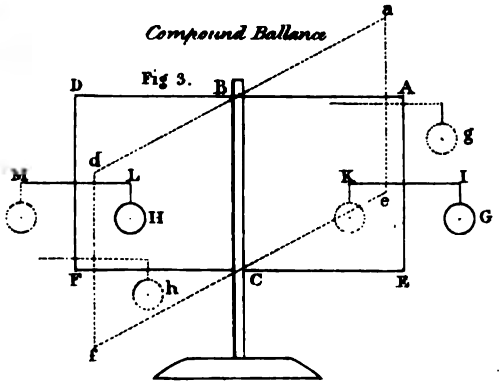


Fig 3.

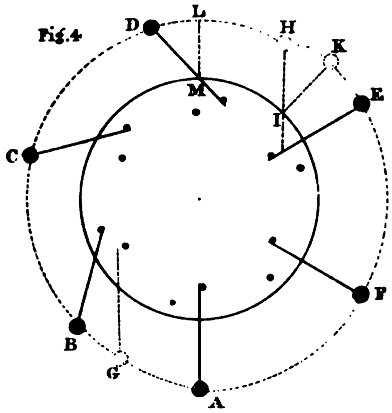


Fig. 4

By the Marquis of Worcester

Schemes for

Perpetual Motion

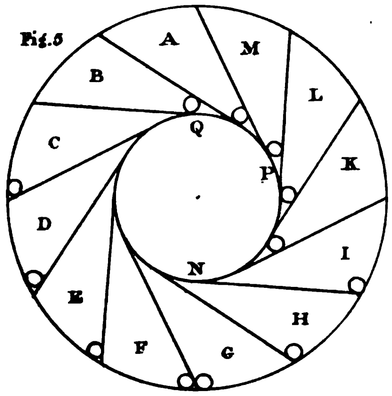


Fig. 5

By Dr. Shiviers

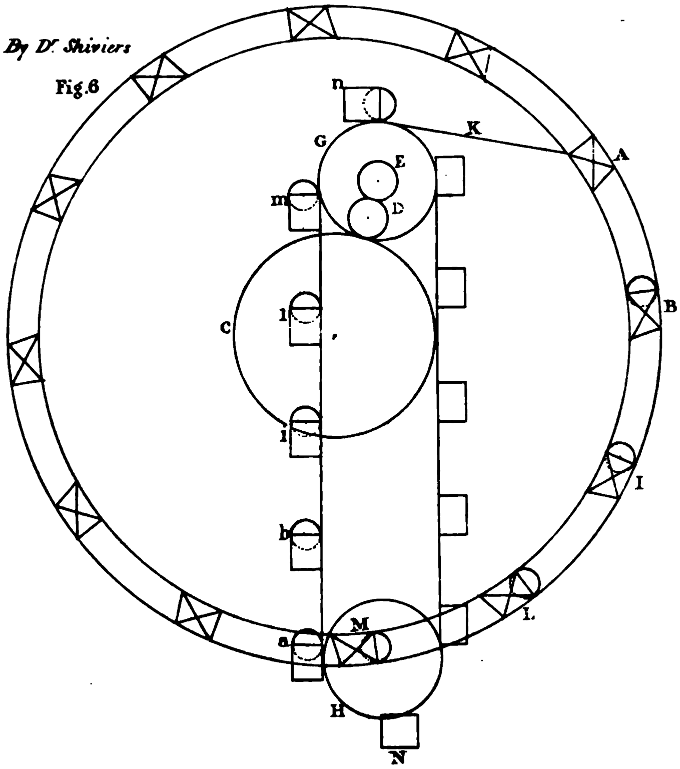
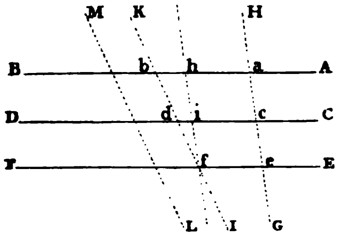


Fig. 6

Philos. Journal Vol. I. Plate XVI. facing p. 384.

Fig. 1.



Scale equivalent to Gunter's Rule of 29 ½ Inches long.

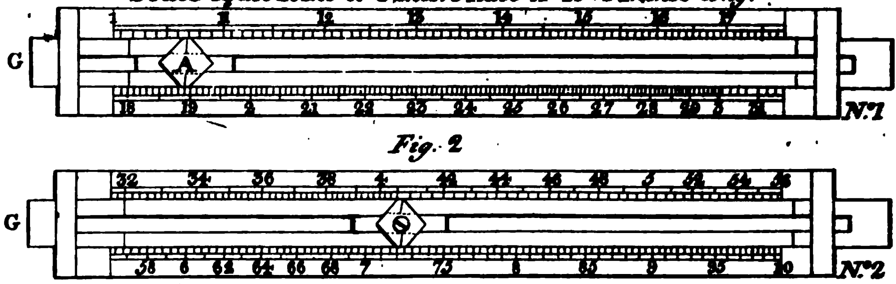
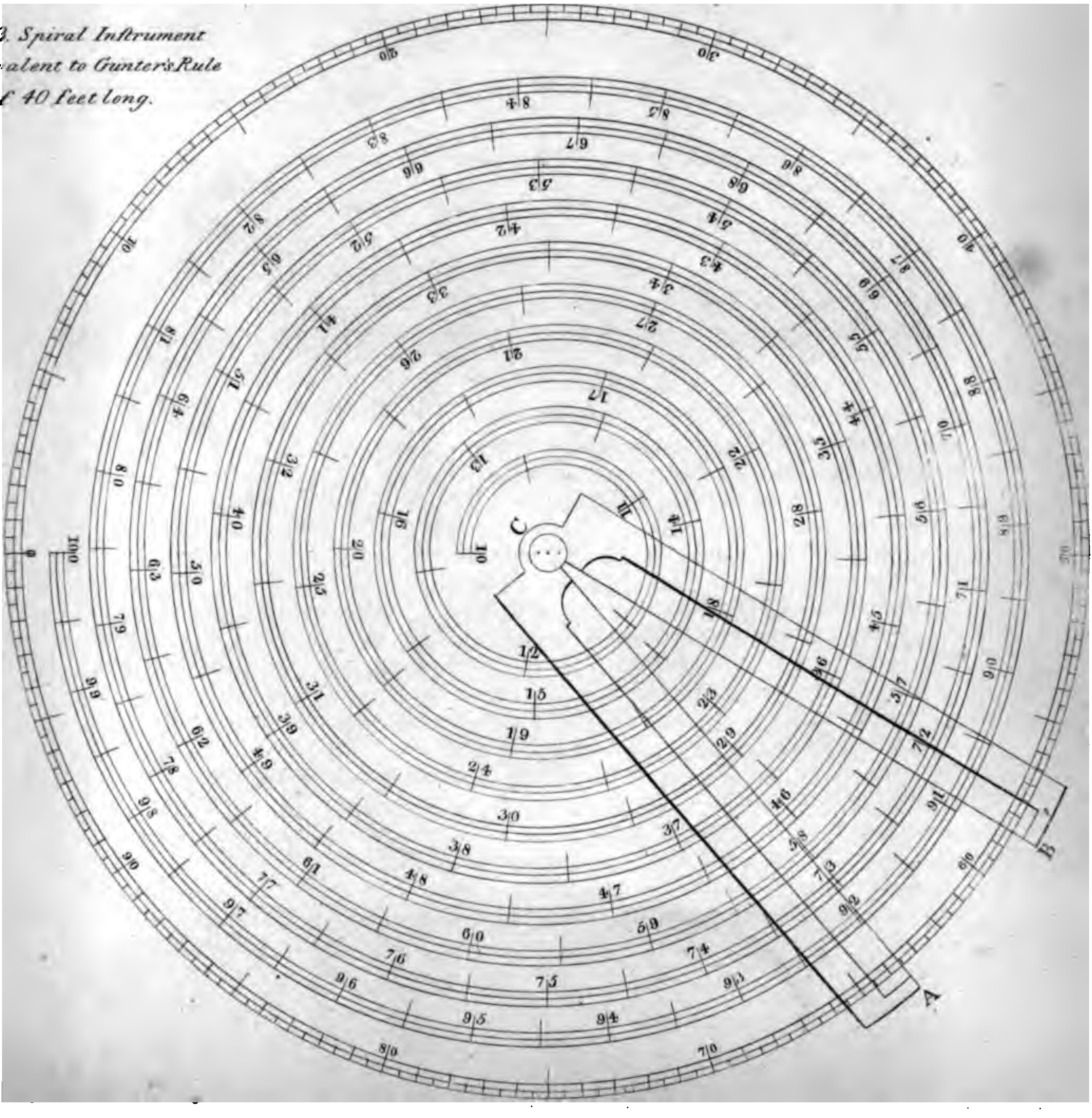


Fig. 2

g. 3. Spiral Inſtrument
uivalent to Gunter's Rule
of 40 feet long.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER 1797.

ARTICLE I.

Concerning the Spontaneous Action of Concentrated Sulphuric Acid on Vegetable and Animal Subſtances; its Action upon Alcohol, and the Formation of Ether. By Citizens FOURCROY *and* VAUQUELIN.

IT has been long known that the concentrated ſulphuric acid deſtroys the texture of organic matters, by converting them into coal, in conſequence of an action which reſembles that of an elevated temperature; but this action has not yet been properly explained, ſo as to determine in what it may conſiſt, or the products it affords. The ancient theory of chemiſtry attributed this effect, in a looſe manner, to the affinity of phlogiſton for the ſulphuric acid, and was content to compare it to combuſtion. The authors of the pneumatic theory diſcerned ſome of the data of this phenomenon, and arrived ſomewhat nearer the truth: but they did not comprehend the whole. They have even committed ſome miſtakes, doubtleſs becauſe they did not ſtudy all the details of this remarkable action, nor obſerve with ſufficient care every thing that paſſes in the ſubjects of the experiments. My aſſociate Vauquelin has long been employed, in conjunction with myſelf, upon various methods of analyſing vegetables; and having ſubmitted a great number to the energy of the principal re-agents among which the acids occupy the firſt rank, a long ſtudy of the action and curious effects of the concentrated ſulphuric acid has led us to diſcover what had hitherto eſcaped the obſervation of chemiſts reſpecting its manner of action upon organic ſubſtances. Though the reſult of our enquiries in this reſpect belongs directly to the vegetable analyſis, which we ſhall offer to the ſcientific world as ſoon as our labour ſhall have acquired all the maturity we are deſirous it ſhall poſſeſs, we have nevertheleſs thought proper to detach this ſmall part, becauſe it appears worthy to intereſt the cultivators of chemiſtry, and at the ſame time proper in ſome reſpects to explain and ameliorate their daily proceſſes, particularly thoſe which relate to ſeveral pharmaceutical operations.

When a dry vegetable ſubſtance, ſuch as wood, ſtraw, or gum, is plunged in the concentrated ſulphuric acid, this matter becomes coloured and ſoft, appears to diſſolve in the acid, and forms with it a magma or kind of brown or black imperfectly fluid matter, well known to every chemiſt as the product of their experiments. If, after the mutual action of theſe bodies is exhauſted, the mixture be diluted with a ſufficient quantity of water, two phenomena equally intereſting are obſerved. The one is the precipitation of a black powder, poſſeſſing all the characters of carbone nearly pure; the other is the ſlight degree of heat which the mixture produces with the water. It is far beneath the temperature which an equal quantity of the ſulphuric acid, originally employed, would have exhibited with a like quantity of water.

The vegetable matter is certainly much altered, and has undergone a great decompoſition in this experiment; ſince the carbone which entered into its compoſition is ſeparated, nearly pure, from the other principles with which it was combined. It is well proved at preſent, that vegetable ſubſtances are compoſed of carbone, hydrogene, and oxygene united in various proportions, and remaining in a conſtant equilibrium of compoſition, ſo long as new attractions do not come to deſtroy it. In the preſent caſe the equilibrium is manifeſtly broken, becauſe the carbone is ſeparated from the two other principles. It was at firſt imagined, and in this manner it is that Citizen Berthollet has particularly determined the cauſe of this phenomenon in ſeveral of his works; namely, that the hydrogene of the vegetable ſubſtance united with the oxygene of the ſulphuric acid, and, by thus forming water and ſulphureous acid, the carbonic principle was ſeparated and thrown down. We have ourſelves adopted and admitted the ſame explanation, until repeated experiments ſhewed us that this opinion is erroneous. In fact, in the caſe here mentioned, and obſerved for a great number of times with the utmoſt attention and care, we have not perceived that an atom of ſulphureous acid was produced, but that the ſulphuric acid remains entire without any alteration or change in the proportion of its conſtituent parts. Since, therefore, this acid is not decompoſed in the cold by vegetable matters, it is neceſſary to conclude, that the changes produced in thoſe bodies are the conſequence of a re-action between their proper principles; a re-action of which the ſulphuric acid is only the occaſional or ſubſidiary cauſe.

But in order to determine by what energy this acid produces ſuch an alteration in vegetable matters, it is requiſite to examine with accuracy what the alteration may conſiſt in; and this examination, to which we have a great number of times directed our attention, has ſhewn the error adopted with regard to the pretended decompoſition of the acid, and the true cauſe of the changes thus produced in organic bodies. When the carbonic precipitate of the experiment above deſcribed is ſeparated, it is found that the ſulphuric acid which floats above it, is ſingularly weakened, and contains the acid of vinegar, which may be ſeparated by diſtillation. Here therefore we ſee the vegetable matter converted into acetous acid and carbone. If the quantity of this laſt, together with that of the vinegar, be compared with the quantity of vegetable matter employed, a conſiderable loſs is perceived in the maſs of theſe products, compared with that of the ſubſtance which afforded them. Nothing can have been loſt in the experiment, becauſe no elaſtic fluid is diſengaged; and as the ſulphuric acid has loſt much of its denſity, and becomes much leſs heated with water, ſince it is, in a word, extremely diluted, it is evident that the water it has acquired could not have been formed but at the expence of the principles of the vegetable, and that it contains the weight which is wanting to the carbone and acetous acid to repreſent the whole of the vegetable matter ſubmitted to experiment. For it is very certain that an action perfectly the ſame, and a reſult accurately identical, takes place in cloſed veſſels, as well as in ſuch as are open; and that the water which ſaturates the ſulphuric acid does not come from the atmoſphere. We are therefore forced to conclude, from this examination, that the vegetable ſubſtance treated with the concentrated cold ſulphuric acid, has ſuffered a decompoſition or analyſis by ſeparating a portion of its carbone nearly pure, and that another part of carbone has united with hydrogene and oxygene to form the acetous acid, at the ſame time that a ſecond portion of oxygene has united with a ſufficient quantity of hydrogene to form the water which dilutes the acid. All theſe changes in the vegetable ſubſtance are therefore made at the expence of its own proper principles. Nothing has come to paſs but a change of equilibrium between them, a ſeparation and unequal combination of their component parts, which produce the three new products obtained.

In what manner does the concentrated ſulphuric acid determine this change of equilibrium? How can it convert an homogeneous organic matter into acetous acid and coal? What is the force or forces whoſe energy or concurrence deſtroys the connection which united the principles of the vegetable ſubſtance, while it has no influence on the ſulphuric acid? A ſingle argument well applied, from the reſult of experiments, will lead us to the ſolution of this problem. Since a vegetable ſubſtance requires a new equilibrium of principles after the total action of the concentrated and cold ſulphuric acid, and remains in the triple ſtate of carbone, acetous acid, and water, at which period the ſulphuric acid has become much leſs denſe and concentrated than before;—we muſt conclude, that the affinity of this acid for water, as well as that of the oxygene for hydrogene and carbone, at the ſeveral proportions required to form vinegar, prevail over the attractions which held theſe three principles united in other proportions under the original form of homogeneous vegetable matter. But, in this new ſum of elective attractions, the only cauſe which determines the change that takes place is the affinity of the ſulphuric acid for water, of which the principles do not unite nor the compoſition take place, till the moment at which this affinity acts. When once this tendency of the acid for water is ſatisfied, the equation between the ſeveral principles is effected, an equilibrium is eſtabliſhed, and no further alteration follows.

It ſeems, however, at firſt ſight, that the imagination refuſes to admit, in the ſulphuric acid, an affinity for a body which does not exiſt completely formed, or to conceive the formation of the water as reſulting from the attraction which the acid exerts upon it, though it do not yet exiſt in the vegetable matter, but in its principles. But whatever repugnance a reſult of this kind ſeems at firſt to produce, it does not appear leſs certain, ſince it is evident, as matter of fact in this experiment, that the acid originally concentrated becomes feeble and aqueous; that the water did not exiſt ready formed in the vegetable matter; that it was not taken from the atmoſpheric air; and that, when once the acid has become diluted, every ſubſequent action ceaſes. Theſe four points are equally eſtabliſhed, and there can be no doubt of their certainty. Beſides which, a great number of facts are at preſent well known to chemiſts, which compel them to admit ſuch affinities as are productive of ſubſequent events, and have been denoted for ſome years by the name of diſpoſing affinities. Thus it is that the attraction of acids for the metallic oxides favours the decompoſition of water by means of the metals, which do not in fact become oxided to unite to acids, but at the expence of the oxygene which they take from the hydrogene. Thus likewiſe it is, that the affinity of the cauſtic earths and alkalis for the ſulphuric acid determines the combuſtion of the ſulphur of ſulphurets by means of water, and the ſeizure of oxygene from the latter, which would not take place without the exiſtence of this diſpoſing affinity. When the long conſideration of chemical phenomena, the habit of beholding them, and of appreciating their reſults, has deſtroyed this firſt repugnance in the mind of the chemiſt, he ſoon conceives that it is not more difficult to admit and explain this diſpoſing affinity, than it is in natural philoſophy to adopt, and admit as proved, the attraction at great diſtances which takes place between the planetary bodies.

We may expreſs the reſults of the chemical effect as it has here been at length explained by a ſimple formula, which in reality is nothing more than an argument diſpoſed in a cloſe arrangement, like all the algebraic formulæ; and this method, by preſenting chemical phenomena in a conciſe language, will poſſeſs the advantage, when more generally employed, of repreſenting, with clearneſs and preciſion, every thing that happens in the moſt complicated operations. Thus we may ſay,

Aqueous ſulphuric acid + liquid acetous acid + precipitated carbone = concentrated ſulphuric acid + entire vegetable matter. In this manner it was that Lavoiſier exhibited the whole myſtery of the vinous fermentation in this ſimple form of an equation:—Juice of grapes = carbonic acid + alcohol; becauſe, in fact, the reſult of this intereſting obſervation, formerly ſo obſcure, but at preſent ſo perſpicuous, is the converſion of muſt, or liquid ſaccharine matter, into alcohol and carbonic acid.

If the effects be truly ſuch as we have here explained, it muſt be neceſſary for their exiſtence that the ſulphuric acid ſhould be concentrated, and the vegetable matters in the dry ſtate: for, if they contained water ready formed, this, by its immediate application to the ſulphuric acid, would ſaturate it, and prevent its re-action upon the principles of theſe ſubſtances. The ſame thing would happen if the acid employed was already weakened by the previous addition of water. This, in fact, takes place in both caſes, as the experience of every chemiſt muſt ſhew. It muſt, however, be obſerved in this place, that in order that the action between vegetable matter and ſulphuric acid combined with water ſhould be nothing, the water ought to be enough to ſaturate the acid: for, if its tendency to combination with water be not entirely ſatisfied, its decompoſing action on vegetable ſubſtances muſt be in proportion to its want of ſaturation. In fact, the decompoſition here mentioned, namely the formation of water and vegetable acid with the precipitation of carbone, cannot be obtained but by increaſing the quantity of acid. This alſo is confirmed by experience, which ſhews that the quantities of ſulphuric acid neceſſary to produce the required decompoſition, do not follow the ſimple ratio of the quantities of water they contain, but are at leaſt in proportion to the ſquares of thoſe quantities. So that if we ſuppoſe, for example, that the moſt concentrated ſulphuric acid contains a quantity of water equal to two, and that another acid contain a quantity equal to four, there will be required not only the double of this laſt to produce the ſame effect upon a vegetable ſubſtance, but it will be found neceſſary to uſe at leaſt ſixteen times as much. And this agrees very well with reaſoning, ſince it is known that the affinity of the ſulphuric acid for water diminiſhes in proportion to the ſquare of the quantity of water it contains.

Though we have reduced the general action of the concentrated ſulphuric acid upon vegetable matters to the formation of water, of acetous acid, and the ſeparation of carbone, we muſt here remark, that in ſome caſes the action becomes more complicated; that ſometimes, alſo, vegetable acids are formed; that the formation of a ſmall quantity of alcohol, even in ſubſtances not ſaccharine, ſuch as gum, unſized paper, &c. likewiſe occaſionally accompanies the foregoing phenomena; and, laſtly, that there is in many circumſtances a diſengagement of carbonated hydrogene gas more or leſs abundant. But theſe multiplied products do not take place ſo frequently as the compoſition of water, of vinegar, and the precipitation of carbone, the conſtancy of which is invariable during the action of concentrated ſulphuric acid upon vegetable matters. They are in ſome meaſure phenomena neceſſary to the former. They depend upon the different proportion of the principles which compoſe vegetable ſubſtances. They take place more eſpecially in thoſe which contain a greater quantity of hydrogene. The deſcription of theſe, and the conſiderations to which they may lead chemiſts, belong to the general and complete hiſtory of vegetable analyſis which we propoſe to publiſh when our work ſhall be finiſhed. Laſtly, theſe productions of alcohol, of various acids at the ſame time, and of carbonated hydrogenous gas, indicate a more profound alteration in the vegetable matters treated by the ſulphuric acid; a complication of effects, which, though it originates in the change of equilibrium occaſioned by the powerful attraction of this acid for water, cannot be well developed until after the explanation of the new proceſſes of analyſis, the methods and reſults of which we ſhall hereafter preſent to the chemical world.

Beſides this multiplication of effects and complicated combinations which are ſometimes obſerved in vegetable matters treated with the concentrated ſulphuric acid, another alteration ſometimes takes place in this laſt. When the ſubſtances placed in contact with it contain too ſmall a proportion of oxygene to form the water required for its ſaturation, their hydrogene then unites with a portion of the oxygene of the acid itſelf, and the ſulphureous acid is formed. This effect is more particularly remarked in oily ſubſtances. It takes place only till the quantity of water neceſſary for the ſaturation of the acid is afforded, and ceaſes as ſoon as this ſaturation is effected.

This action of the concentrated ſulphuric acid is ſtill more complicated on animal than on vegetable matters, becauſe the animal matters themſelves are more compounded. Though the affinity of the acid for water be the cauſe, the formation of this liquid is not only accompanied with that of a vegetable acid and the precipitation of carbone, but ammoniac is likewiſe formed. No eſcape of any volatile principle is perceived. The azote, which forms part of theſe ſubſtances, unites with a portion of hydrogene, and conſtitutes the volatile alkali, which combines with the ſulphuric acid, or with the acid which is at the ſame time compoſed, while another part of the hydrogene forms with the oxygene of the ſame ſubſtances that water which is requiſite to ſaturate the ſulphuric acid. But in theſe caſes, as in the experiments with vegetable ſubſtances, the quantity of carbone, which exceeds that required to compoſe the new acid, together with a portion of hydrogene and oxygene, will be ſet at liberty, and falls down in the form of magma, of flocks, or of black powder. In this manner it is that in the caſe of burns, frequently ſo dangerous, which are produced by the concentrated ſulphuric acid, there is formed, at the expence of the ſkin, water, vegetable acid, and ammoniac, while carbone is precipitated, and a complete deſtruction of the burned parts is the reſult of theſe ſimultaneous effects, as appears by the brown or black ſcar which afterwards falls off, and is ſeparated as a foreign body. Hence may be deduced how great the diſorganizing power of this concentrated acid muſt be on the coats of the ſtomach or the œſophagus, and the utility of preſenting to it, at the moment it is ſwallowed, a mucous liquid ſubſtance, on which its action may be directly and ſpeedily exerciſed.

From the whole of what is here ſtated concerning the reciprocal action of concentrated ſulphuric acid, and the ternary or quaternary compounds which belong to the vegetable and animal kingdoms, we may perceive that its tendency to deſtroy theſe complicated compoſitions, and convert them into others more ſimple, muſt open a new path in the vegetable analyſis. We may perceive that, ſince its effects are confined to thoſe here deſcribed, while the ſubſtances and the ſulphuric acid are permitted to act ſpontaneouſly and without foreign aſſiſtance, the energy of the acid will proceed much further in this decompoſition when it is increaſed by the addition of new quantities of this fluid, or the application of accumulated heat. But the new reſults thus produced would carry us from the ſubject here treated, and to which we at preſent mean to confine ourſelves.

In this accurate deſcription of the ſpontaneous action of ſulphuric acid upon vegetable and animal matters, our intention was to ſhew more particularly that this knowledge may have a direct influence upon pharmaceutic operations. Our aſſociate Vauquelin has ſhewn in another memoir, which forms the continuation and uſeful reſult of the preſent, that the principles here explained are alone capable of developing the theory of the formation of ether, hitherto ſo vague and uncertain; and at the ſame time to perfect the operations proper to afford theſe important products. This firſt work, which we offer jointly to the ſchool of pharmacy, will be a confirmation of the truth conſigned in the diſcourſe on the indiſſoluble union of chemiſtry and pharmacy, namely, that the former cannot make the ſlighteſt advance, without the latter receiving immediately a proportional degree of perfection.

Concerning the Action of the Sulphuric Acid upon Alcohol, and the Formation of Ether.

THE preparation of ether is a complicated pharmaceutic operation, the reſults of which are no leſs known than the theory is obſcure. This is proved by the different explanations given by the chemiſts who have treated of this operation before the eſtabliſhment of the pneumatic doctrine.

The greater number of modern philoſophers are in general of opinion, that the whole mechaniſm of the formation of ether conſiſts in the decompoſition of the ſulphuric acid, and the tranſition of part of its oxygene to the elements of the alcohol. According to this hypotheſis, water, carbonic acid, and ſulphureous acid are formed. This opinion contains the ſame error as was pointed out in the foregoing memoir. In order to explain the operation with perſpicuity and effect, we ſhall deſcribe its circumſtances from the beginning to the end. And while we thus treat on a ſubject familiar to every chemiſt, we hope to prove that this operation, ſo common, and ſo often repeated, does nevertheleſs offer, in the ſeries of its phenomena, certain facts which have not yet been properly deſcribed, and of which the accurate knowledge alone can afford a ſolid theory of etherification.

Experiment I. Equal parts of concentrated ſulphuric acid and rectified alcohol being mixed together, diſengage a quantity of caloric capable of elevating the temperature of the maſs to 70 degrees (190 Fahrenheit). Bubbles of claſtic fluid are formed, the liquor becomes turbid, aſſumes an opal colour, and at the end of ſeveral days becomes of a deep red colour.

Experiment II. A combination of two parts of ſulphuric acid and one part of alcohol elevates the temperature to 75 degrees (201 Fahrenheit), becomes immediately of a deep red colour, which changes to black a few days afterwards, and emits a ſmell perceptibly ethereal.

Experiment III. When we carefully obſerve what happens in the combination of equal parts of alcohol and concentrated ſulphuric acid expoſed to the action of caloric in a proper apparatus, the following phenomena are ſeen:

1. When the temperature is elevated to 78 degrees (208 Fahrenheit), the fluid boils, and emits a vapour which becomes condenſed by cold into a colourleſs, light and odorant liquor, which, from its properties, has received the name of ether. If the operation be properly conducted, no permanent gas is diſengaged until about half the alcohol has paſſed over in the form of ether. Until this period there paſſes abſolutely nothing but ether and a ſmall portion of water, without mixture of ſulphureous or of carbonic acid.

2. If the receiver be changed as ſoon as the ſulphureous acid manifeſts itſelf, it is obſerved that no more ether is formed, but the ſweet oil of wine, water, and acetous acid, without the diſengagement hitherto of a ſingle bubble of carbonic acid gas. When the ſulphuric acid conſtitutes about four-fifths of the maſs which remains in the retort, an inflammable gas is diſengaged which has the ſmell of ether, and burns with a white oily flame. This is what the Dutch chemiſts have called carbonated hydrogene gas, or olefiant gas, becauſe, when mixed with the oxygenated muriatic acid, it forms oil *. At this period the temperature of the fluid contained in the retort is elevated to 88 or 90 degrees (230 or 234 of Fahrenheit).

3. When the ſweet oil of wine ceaſes to flow, if the receiver be again changed, it is found that nothing more paſſes but ſulphureous acid, water, carbonic acid gas; and that the reſidue in the retort is a black maſs, conſiſting for the moſt part of ſulphuric acid thickened by carbone.

The ſeries of phenomena here expoſed will juſtify the following general inductions:

1. A ſmall quantity of ether is formed ſpontaneouſly, and without the aſſiſtance of heat, by the combination of two parts of concentrated ſulphuric acid and one part of alcohol.

2. As ſoon as ether is formed, there is a production of water at the ſame time; and while the firſt of theſe compoſitions takes place, the ſulphuric acid undergoes no change in its intimate nature.

3. As ſoon as the ſulphureous acid appears, no more ether is formed, or at leaſt very little; but then there paſſes the ſweet oil of wine, together with water and acetous acid.

4. The ſweet oil of wine having ceaſed to come over, nothing further is obtained but the ſulphureous and carbonic acids, and at laſt ſulphur, if the diſtillation be carried to dryneſs.

The operation of ether is therefore naturally divided into three periods; the firſt, in which a ſmall quantity of ether and water are formed without the aſſiſtance of heat; the ſecond, in which the whole of the ether which can be obtained is diſengaged without the accompaniment of ſulphureous acid; and the third, in which the ſweet oil of wine, the acetous acid, the ſulphureous acid, and the carbonic acid are afforded. The three ſtages have no circumſtance common to all, but the continual formation of water, which takes place during the whole of the operation. The principles eſtabliſhed in the preceding memoir, and the compariſon of the phenomena here deſcribed, would in ſtrictneſs be ſufficient to explain how the ſulphuric acid acts upon alcohol in the formation of ether; but as this operation includes ſeveral particular circumſtances which are not compriſed in the expoſition of the general principles ſketched out in the firſt memoir, we ſhall in this place enter into ſome details neceſſary for the ſolution of the propoſed problem.

---

* Philoſophical Journal, I. 44.

The ether which is formed without the aſſiſtance of caloric, and the carbone which is ſeparated without decompoſition of the ſulphuric acid, prove that this acid acts on alcohol in a manner totally different from what has hitherto been ſuppoſed. It cannot, in fact, be affirmed, that the acid is altered by the carbone, becauſe daily experience ſhews that no ſenſible attraction takes place between theſe two bodies in the cold; neither can it be affected by the hydrogene, for in that caſe ſulphureous acid would have been formed, and it is known that no trace is exhibited during this firſt period. We muſt therefore have recourſe to another ſpecies of action, namely, the powerful attraction exerciſed by the ſulphuric acid upon water. It is this which determines the union of the principles which exiſt in the alcohol, and with which the concentrated acid is in contact: but this action is very limited if the acid be ſmall in quantity; for an equation of affinity is ſoon eſtabliſhed, the effect of which is to maintain the mixture in a ſtate of repoſe.

Since it is proved that ether is formed in the cold by the mixture of any quantities of alcohol and ſulphuric acid, it is evident that a maſs of alcohol might be completely changed into ether and vegetable acid by uſing a ſufficient abundance of ſulphuric acid. It is equally evident that the ſulphuric acid would not by this means undergo any other change than that of being diluted with a certain quantity of water. This obſervation proves, at the ſame time, that alcohol contains oxygene, becauſe water cannot exiſt without this principle, which muſt be afforded by the alcohol only, ſince the ſulphuric acid ſuffers no decompoſition. It likewiſe ſhews that the ſulphuric acid here, inſtead of favouring the decompoſition of water as in many other caſes, does, on the contrary, determine its formation; ſo that its action on the oxides of carbone and hydrogene, which conſtitute vegetable matters in general, is abſolutely the reverſe of that which it exerciſes upon the ſimple combuſtible bodies, ſulphur and metals, for example, of which it effects the oxidation by the decompoſition of water.

We muſt not however imagine, from theſe facts, that ether is alcohol minus oxygene and hydrogene. Its properties alone would contradict this, for a quantity of carbone proportionally greater than that of the hydrogene is at the ſame time ſeparated. It may in fact be conceived, that the oxygene, which in this caſe combines with the hydrogene to form water, not only ſaturated that hydrogene in the alcohol, but likewiſe the carbone. So that, inſtead of conſidering ether as alcohol minus hydrogene and oxygene, we muſt, by keeping an account of the precipitated carbone and the ſmall quantity of hydrogene contained in the water which is formed, regard it as alcohol plus hydrogene and oxygene. We muſt obſerve, that the comparative analyſis of alcohol and of ether, of which the details would be out of place in this memoir, correſpond perfectly with the reſult afforded by the theory.

The foregoing are the effects produced by a combination of alcohol and ſulphuric acid, ſpontaneouſly produced without foreign heat. Let us in the next place obſerve how this combination is effected when caloric is added. The phenomena are then very different, though ſome of the reſults are the ſame.

In the firſt place we muſt obſerve, that a combination of ſulphuric acid and alcohol in equal parts does not boil at leſs than 78 degrees of temperature, while that of alcohol alone boils at 64. Now ſince ebullition does not take place till the higher temperature, it is clear that the alcohol is retained by the affinity of the ſulphuric acid, which fixes it more conſiderably. Let us alſo conſider that organic bodies, or their immediate products, expoſed to a lively briſk heat, without the poſſibility of eſcaping ſpeedily enough from its action, ſuffer a partial or total decompoſition, according to the degree of temperature. Alcohol undergoes this laſt alteration when paſſed through an ignited tube of porcelain. By this ſudden decompoſition it is converted into water, carbonic acid, and carbone. The reaſon, therefore, why alcohol is not decompoſed when it is ſubmitted alone to heat in the ordinary apparatus for diſtillation, is, that the temperature at which it riſes in vapours is not capable of effecting the ſeparation of its principles; but when it is fixed by the ſulphuric acid or any other body, the elevated temperature it undergoes without the poſſibility of diſengagement from its combination is ſufficient to effect a commencement of decompoſition, in which ether and water are formed, and carbone is depoſited. Nothing more therefore happens to the alcohol in theſe circumſtances than what takes place in the diſtillation of every other vegetable matter in which water, oil, acid, and coal are afforded.

Hence it may be conceived that the nature of the products of the decompoſition of alcohol muſt vary according to the different degrees of heat, and this explains why at a certain period no more ether is formed but the ſweet oil of wine and acetous acid. In fact, when the greateſt quantity of the alcohol has been changed into ether, the mixture becomes more denſe, and the heat which it acquires previous to ebullition is more conſiderable. The affinity of the acid for alcohol being increaſed, the principles of this acid become ſeparated; ſo that, on the one hand, its oxygene ſeizes the hydrogene and forms much water, which is gradually volatilized; while, on the other, the ether retaining a greater quantity of carbone, with which at that temperature it can riſe, affords the ſweet oil of wine. This laſt ought, therefore, to be conſidered as an ether containing an extraordinary portion of carbone, which gives it more denſity, leſs volatility, and a lemon-yellow colour.

During the formation of the ſweet oil of wine, the quantity of carbone which is precipitated is no longer in the ſame proportion as during the formation of ether.

What we have here ſtated concerning the manner in which ether is formed by the ſimultaneous action of the ſulphuric acid and heat, appears ſo conformable to truth, that nearly the ſame effects may be produced by a cauſtic fixed alkali. In this caſe alſo a kind of ether and a ſweet oil of wine are volatilized, and coal is precipitated. It is therefore only by fixing the alcohol that the ſulphuric acid permits the caloric to operate a ſort of decompoſition. It may alſo be urged as a proof of this aſſertion, that the ſulphuric acid, which has ſerved to make ether as far as the period at which the ſweet oil of wine begins to appear, is capable of ſaturating the ſame quantity of alkali as before its mixture with the alcohol.

With regard to the formation of the oil of wine by the augmentation of temperature from the concentration of the ſulphuric acid, it is ſo true, that if water and alcohol be added in the ſame proportion as the volatile product comes over, oil of wine will never be formed, and all the alcohol will be converted into ether.

It is to the ſame cauſe, namely the heat at a higher degree of intenſity, that we muſt attribute the development of carbonated hydrogene gas, or the olefiant gas, which ſeems to be nothing elſe but the ſweet oil of wine, with a ſmall portion leſs of oxygene and more of caloric.

CONCLUSION.

From the facts and obſervations contained in this memoir, it follows:

1. That the formation of ether is not owing, as has been thought, to the immediate action of the principles of the ſulphuric acid upon thoſe of the alcohol, but to a true reaction of the elements of the alcohol upon each other, and particularly the oxygene and hydrogene, occaſioned merely by the ſulphuric acid.

2. That we might in ſtrictneſs change any quantity whatever of alcohol into ether, without the aſſiſtance of heat, by ſufficiently increaſing the proportion of ſulphuric acid.

3. That the operation is divided into two principal ſtages, in one of which ether and water are only formed; and in the other, the ſweet oil of wine, water, and the acetous acid.

4. That ſo long as ether is formed, the ſulphuric acid is not decompoſed, and there is no product of ſweet oil of wine; but as ſoon as this laſt appears, little, if any, ether is afforded, and the ſulphuric acid is at the ſame time decompoſed.

5. That the formation of ſweet oil of wine may be prevented, by keeping the temperature of the mixture between 75 and 78 degrees; which may be eaſily done, by ſuffering a few drops of cold water to fall from time to time into the retort.

6. That alcohol differs from ether by containing more carbone and leſs of hydrogene and oxygene, and that the ſweet oil of wine is to ether nearly what alcohol is to the oil[*].

II.

On the Multiplier of Electricity. By the Inventor, Mr. T. Cavallo: with Obſervations.

To Mr. Nicholson, Editor of "The Journal of Natural Philoſophy, &c."

SIR,

HAVING a few days ago ſeen for the firſt time your Journal of Natural Philoſophy, Chemiſtry, &c. I found in the third article of No. 1. an erroneous account of my Multiplier of Electricity, a deſcription of which is contained in the third volume of my Treatiſe on Electricity; and truſting that your candour will allow a correction of this error to come before the Public, I have taken the liberty to ſend you the following explanation, with a requeſt that you will be pleaſed to inſert this letter in your next number.

In the firſt paragraph of the above-mentioned article, you compare an electrical inſtrument of your contrivance (which you deſcribe in the ſequel) with my multiplier, in the following words:—"I then (viz. in the year 1787) mentioned my intention to conſtruct an inſtrument on that principle, which ſoon afterwards I did. It was ſhewn to Sir Joſeph Banks and his friends, at his houſe, nearly at the ſame time, and in the ſame year transferred to the celebrated Mr. Van Marum of Haerlem, who now poſſeſſes it. From various other avocations I was prevented from cauſing any others to be made. It is not therefore wonderful that the ſame thought ſhould ſince have occurred to ſo great a maſter of the ſubject as Mr. Cavallo, who, in the third volume of his Electricity, publiſhed in 1795, gives a deſcription and engravings of an inſtrument very different in form, but the ſame in principle, &c."

---

[*] Theſe Memoirs are tranſlated from the Annales de Chimie, XXIII. 186.

Now the error I allude to conſiſts in the aſſertion *that the two inſtruments are the ſame in principle;* and I flatter myſelf that the following explanation will be ſufficient to ſhew that they are ſo far from it as to be totally different in principle and in effect.

Your inſtrument does nothing more than collect a conſiderable quantity of diffuſed electricity into a ſmall compaſs, which is exactly the ſame office as is performed by Mr. Volta's condenſer, or by my collector of electricity, which is deſcribed in the Philoſophical Tranſactions for the year 1788; whereas my multiplier renders an abſolutely ſmall quantity of electricity manifeſt, by accumulating a conſiderable quantity of the contrary electricity. Or, to be more explicit, your inſtrument cannot communicate to the electrometer a greater quantity of electricity, or rather not nearly ſo much of it, as is contained in the electrified body which is to be examined; whereas my multiplier accumulates a quantity of electricity many times greater than that of the body in queſtion, and of courſe much more perceptible.

Suppoſe, for inſtance, that a certain body, having its ſurface equal to that of the electrometer which belongs to your inſtrument, contain 100 parts of electricity; and ſuppoſe, likewiſe, that the ſaid electrometer requires 200 ſuch parts of electricity in order to ſhew any divergency. Now your inſtrument will be found incapable of manifeſting the electricity of the given body; for, when the upper tinfoil ſegments have conveyed the greateſt portion, or we may even ſay all the 100 parts of electricity from the given body to the hook with the electrometer, they cannot, in the ſubſequent rotations of the inſtrument, collect any more. The electrometer, therefore, being at moſt loaded with 100 parts of electricity, cannot ſhew any divergency; ſince, according to the ſuppoſition, at leaſt 200 parts of electricity are required in order to produce any divergency in it.

But if the above-mentioned electrified body be applied to the plate A of my multiplier, and the inſtrument be worked, the plate C, and of courſe an electrometer which is connected with it, will ſoon acquire 300 or 400 parts, or, in ſhort, a much greater quantity of electricity than is ſufficient to cauſe a divergency in the electrometer; becauſe, by working the inſtrument, the original quantity of electricity in the plate A is not diminiſhed, or removed from it; but the contrary electricity (which is repeatedly and unlimitedly induced upon the plate B by the plate A) is accumulated upon the plate C, &c.

Sir, I remain your obedient humble Servant,

Wells-Street, Oct. 30, 1797. T. CAVALLO.

AFTER returning my ſincere thanks to Mr. Cavallo for the detection of the error he mentions, into which I certainly ſhould not have fallen if I had conſulted his Treatiſe at the time the paper he alludes to was written, I ſhall take the preſent opportunity of making ſome remarks on the inſtruments we at preſent poſſeſs for manifeſting the preſence and kind of weak electricities. Theſe are: the condenſer, the ſimple doubler, the doubler with mechaniſm,

chaniſm, two inſtruments by Mr. Cavallo, and the inſtrument deſcribed at p. 17 of this Journal.

After the diſcovery of the method of inſulation by Stephen Gray, the firſt form in which compenſated electricity was obſerved appears to have been in the combination called the electric jar; in the explanation of which ſo many of the earlier electricians have ſhewn their ingenuity. The next inſtance conſiſted in the application of conductors to each other; by which it was ſhewn, that electricity is not equally diffuſed over the ſurface of a conducting body, while any part of the electrified maſs which differs in intenſity is ſeparated from the reſt by the interpoſition of air or other non-conductors. The cann and chain of Franklin, and the electrical well of Beccaria, exhibited the principle of compenſation in another light; and the ſubject was rendered ſtill more difficult when the ſtrong oppoſite electricities of ribbands and ſilk ſtockings, placed in contact, expoſed to friction in that ſituation, and then ſeparated, were ſhewn in the experiments of Cigna and Symmer. The vindicating electricity of Beccaria appeared to be ſtill more remote from the ſimple general laws which had been applied to explain the effect of the jar; and the electrophore of Wilcke preſented difficulties ſcarcely leſs conſiderable.

The firſt proceſs for accumulating electricity, which differs from that of mere friction, appears to have been invented by Profeſſor Lichtemberg [*], and alſo by Dr. Klincock [†], of Prague. It was performed by means of two reſinous plates like thoſe of the common electrophore, and one metallic plate with an inſulating handle. When one of the reſinous plates had been ſlightly rubbed, it was uſed to produce the oppoſite electricity in the metallic plate. This plate was then conveyed to the other reſinous plate, and made to depoſit its electricity by applying its edge, or otherwiſe, to the non-conducting ſurface. The laſt-mentioned ſurface could then be uſed in the ordinary manner to give an electricity to the metallic plate of the ſame kind as that of the original reſinous ſurface, to which it was again applied by the edge. The electricity of the firſt reſinous plate became thus increaſed, and, by the firſt-mentioned proceſs, the metallic plate was returned to the other reſinous ſurface in a higher ſtate of electrization. By a repetition of theſe manœuvres, the two plates were ſpeedily put into as high a ſtate of electricity as they were capable of retaining.

An accidental circumſtance directed the attention of Profeſſor Volta to the advantages which might be derived from the increaſed electrical capacity of the metallic plate of the electrophore, when placed not upon the reſinous plate, but on an imperfectly conducting ſubſtance [‡]. If the laſt-mentioned ſubſtance were a good conductor, it would at ſome part of its ſurface be ſo far in contact with the upper plate as to carry off any electricity communicated to this laſt; and if it were a non-conductor, it might, and in moſt caſes probably would, poſſeſs an electric ſtate of its own which would influence that of the upper plate. But the imperfect conductor will not readily produce the firſt effect, and is incapable of the latter. It therefore ſerves only to aſſume the contrary ſtate of the upper plate whenever electricity is communicated to this laſt, and renders it ſuſceptible of abſorbing a much greater quantity than it elſe could have done; the nature and intenſity of which becomes perceptible when the plate is liſted up. Mr. Cavallo improved this inſtrument, by communicating the electricity of the metallic plate, when raiſed up, to another plate of the ſame kind, but much ſmaller, and in contact with an imperfectly conducting ſurface. And the Rev. Mr. Bennett adapted the ſame to his electrometer *. This is the condenſer of Volta; and the imperfectly conducting plate may be a piece of dry marble, or a common wooden table, or any kind of plate covered with thin ſilk.

---

[*] Journal de Rozier, January 1780, p. 20.

[†] Philoſophical Tranſactions, LXVIII. p. 1029.

[‡] Journal de Phyſique for May and Auguſt 1783, and alſo the Philoſophical Tranſactions, Vol. LXXII. p. 237.

Mr. Bennett was, I believe, the firſt who improved the condenſers of Volta, by making uſe of a manipulation ſimilar to that of the Profeſſors Lichtemberg and Klincock. His inſtrument, called the doubler, conſiſts of three metallic plates, capable of being applied to each other with their flat ſurface, but prevented from contact in this ſituation by a thin coating of varniſh. They have inſulating handles at the ſide, and may be brought into actual contact edgewiſe. Let the three plates be called A, B and C; and the manipulation for increaſing the intenſity of one of them, for example A, will be as follows:

The plate A being inſulated, place B upon it. In this ſituation communicate electricity to it, while B is touched with the finger. The conſequence will be, that A will receive a much larger quantity of electricity than it would elſe have abſorbed if B had not been preſent.

Remove the communication from A, and the finger from B, and raiſe the latter by its inſulating handle. A and B will then exhibit the oppoſite electric ſtates more ſtrongly than while together.

Place C upon B, and touch C with the finger. It will aſſume the oppoſite ſtate to B, or, in other words, the ſame ſtate as A.

Place B upon A, as before, touching B with the finger; and at the ſame time apply C edgewiſe to A. In this ſituation, C, being without compenſation, will give the greateſt part of its electricity to A.

Remove C; take the finger from B, and ſeparate B from A. The oppoſite electricities will be ſtronger than before, on account of the increaſe afforded by C.

Place C upon B again, as in the third ſtage of the original proceſs, and repeat the ſubſequent manipulations. In each of them the intenſities of A and B will be nearly doubled.

Though this proceſs is ſimple and evident, yet it requires to be learned, and takes a certain time for the performance. It therefore appeared a deſirable object to form an inſtrument which ſhould by ſome very ſimple movement complete this ſmall ſeries of operations. In the month of December 1787 Mr. Partington lent me an inſtrument contrived by Dr. Darwin, and conſiſting of four metallic plates, two of which were moveable by wheel-work into poſitions which required them to be touched with the hand in order to produce the effect. It appeared to me that the whole operation, including the touching, might be done by a ſimple combination without wheel-work by the direct rotation of a winch. This was ſoon afterwards effected, and communicated by me to the Royal Society in the year 1788. The inſtrument is alſo deſcribed in Mr. Bennett's *New Experiments on Electricity*, publiſhed in 1789, and in other books.

Mr. Bennett and Mr. Cavallo obſerved, ſoon after the diſcovery of the doubler, that it never fails to exhibit an electric ſtate by the mere operation, without any communication of electricity having been previouſly made. The former made ſeveral valuable obſervations and experiments to inveſtigate the cauſes and remove the uncertainty produced by this ſpontaneous electricity; and the latter preſented to the Royal Society [*] an account of a new inſtrument called a collector of electricity, in which, as he aſſerts, this imperfection does not take place. It conſiſts of a plate of tin, ſupported by two upright ſticks of glaſs; on each ſide of which plate are two frames of wood, covered with gilt paper, which do not touch the tin plate, but ſtand parallel to it at a ſmall diſtance. Theſe frames are faſtened to the platform of the inſtrument by hinges; ſo that if electricity be communicated to the plate, it will receive a large quantity without any conſiderable intenſity, becauſe its capacity is much augmented by the vicinity of the plane of gilt paper on each ſide. But if theſe planes be thrown back into the horizontal poſition, which is eaſily done by means of their hinges, the electricity, which before was compenſated in the plate, will have its intenſity greatly increaſed. An electrometer connected with this plate will therefore ſhew ſigns of electricity by means of a communication made between a large ſtock of electricity, and the tin plate in its firſt poſition, though the intenſity of that ſtock may have been too ſmall to have affected the electrometer without this contrivance.

---

* Philoſophical Tranſactions, LXXVII. p. 32.

It does not appear, in the author's deſcription of this inſtrument, that it removes the equivocal effect of the doubler; for it is evident that it does not, in its ſimple proceſs, enter the province of the doubler in which this effect takes place. The doubler requires ſix or ſeven turns before it will exhibit ſpontaneous electricity; at which period the firſt charge is magnified above twelve thouſand times: but his ſimple inſtrument will ſcarcely exceed one hundred times, and therefore requires the electricity to be one hundred and twenty times as ſtrong as that which cauſes the uncertainty of the doubler. Whence it may be inferred, that the doubler would have acted unequivocally with all ſuch electricities as this inſtrument is capable of exhibiting.

But the ſpinning inſtrument, as Mr. Cavallo in the foregoing letter juſtly obſerves, does nothing more than collect a conſiderable quantity of diffuſed electricity into a ſmall compaſs, though it does not appear to be exactly the ſame office as is performed by M. Volta's condenſer, or his collector, as deſcribed by their reſpective inventors. For thoſe inſtruments, if very ſmall, may exhibit very little intenſity in conjunction with an electrometer of conſiderable ſurface; whereas the ſpinning inſtrument, beſides its facility in performing the operations, may be conſidered as poſſeſſing a ſurface indefinitely great. Theſe advantages, ſuch as they are, conſtitute all the difference I perceive between them.

Mr. Cavallo's multiplier, deſcribed in the third volume of his Complete Treatiſe on Electricity, which I had miſtakenly compared with the ſpinning inſtrument, conſiſts of four metallic plates, A, B, C, and D. The plate A is ſupported on a ſtick of glaſs fixed in the platform of the inſtrument. The plate B is alſo ſupported on a ſtick of glaſs which is fixed on a radius or arm, by means of which it can be brought very near and parallel to A, and again removed, ſo as to touch the third plate C, by means of a wire projecting from B. The plate C is ſupported, like A, by a ſtick of glaſs fixed in the platform. And, laſtly, the plate D is fixed by a metallic ſtem upon a ſliding-piece, by means of which it can be brought very near and parallel to C, and occaſionally removed.

---

[*] Philoſophical Tranſactions for 1788.

The operation is this:—Let B be brought as near to A as the conſtruction will admit. In this caſe it muſt be remarked that the wire proceeding from B will touch another wire that communicates with the earth. Let electricity be then communicated to A. The plate A will receive about one hundred times as much electricity when B is thus applied at about 1-40th of an inch diſtance, than it would have otherwiſe acquired at a like intenſity, and B will poſſeſs the contrary ſtate nearly equal. Let B then be removed ſo as to come in contact with C, of which the capacity is no leſs increaſed by the vicinity of D. The electricity of B will be ſhared between itſelf and C in proportion to their capacities; that is to ſay, about 99 parts in C, and 1 in B. At the next repetition B will bring 100 parts, which, together with the 99 parts in C, will be ſhared in the ſame manner; that is to ſay, 197 in C, and 2 in B. In this manner, without entering into the conſideration of ſeries where the data cannot pretend to minute accuracy, it is clear that the proceſs, indefinitely prolonged, can never produce a greater charge in C than 100 times the quantity conveyed at each ſingle motion by B; that is to ſay, 100 × 100 times, or 10,000 times the intenſity of the original oppoſite electricity given to A, provided D be drawn back to leave C in a diſengaged ſtate.

It has always appeared to me that the doubtful reſults of the doubler muſt ariſe from the natural or ſpontaneous ſtate of the plates A and B, at their firſt conjunction. If this charge be too ſtrong to be deſtroyed by the energy of the communicated electricity, the doubler muſt in the reſult ſhew the ſame electricity as it would have ſhewn without ſuch communication, though ſomewhat later. And if the ſame circumſtance were to obtain in the plates A and B of Mr. Cavallo's laſt inſtrument, or any other contrivance operating by a charge, I do not ſee but that the reſults muſt be equally uncertain. If the electricity of B, inſtead of being accumulated in C of the multiplier, had been employed to produce the contrary ſtate in D, and thence by communication to A, as in the doubler, it does not appear that any uncertainty could follow, provided only that the firſt effect of the conjunction of A and B had been governed by the communicated electricity. And if this reaſoning be true, it will follow from that and the facts, that the uncertainty of the doubler exiſts alſo in the condenſer, the collector, the ſpinning inſtrument, and the multiplier, though the firſt-mentioned inſtrument alone is delicate enough to ſhew it; and that in all electricities which are ſtrong enough to affect any of the latter, the doubler may be uſed without fear of an equivocal reſult.

III.

A Memoir on certain Methods of Economy and Improvement in the Manufacture of Hats.

By Citizen CHAUSSIER[*].

THE progreſs of the arts has been long retarded by prejudice, and the ſervile diſpoſition which attaches itſelf without enquiry to the ancient practices of buſineſs; and more particularly by the ſpirit of individual intereſt and ſuſpicion which have prevailed among artiſts, and induced them to conceal their operations under a kind of myſterious veil. By this means the knowledge of the arts was in a great meaſure confined to thoſe who actually practiſed them. Error was propagated under the ſhade of ſecrecy, at the ſame time that uſeful proceſſes and improvements, which fortunate experiments had diſcovered, were alſo confined to the limits of individual works, beyond which they were never extended. At preſent, however, the arts cannot but participate in the ſalutary reform which has taken place on the ſoil of equality. Every effort ought to be directed to the ſame object, and every petty intereſt ought to be ſacrificed to the public welfare. Thoſe myſterious reſervations, thoſe practices ſo carefully concealed under the name of ſecrets, ought to be diſcloſed. The means of amelioration and improvement ought not to remain concentrated in any particular workſhop. As ſoon as experience ſhall have confirmed their efficacy, they ought to become the public property *. Theſe conſiderations induce me to point out certain proceſſes relative to the art of the hatter, which for ſome time paſt have been employed with much ſucceſs in a large manufactory in the department of La Côte d'Or.

---

[*] Journal Polytechnique, cahier I. p. 160.

The art of the hatter is well known to conſiſt of a method of forming, with the wool or fur of different animals, a kind of ſtuff of a denſe compact texture, capable of aſſuming and preſerving any figure which may be given to it. To obtain this object a variety of proceſſes are employed, partly chemical and partly mechanical, which may be reduced to four principal operations; namely, felting, fulling, dyeing, and preparation.

I ſhall not here deſcribe the operation of felting, nor the previous preparations which ſeveral kinds of hair demand in order that they may be uſed in manufactures. This would lead us too far from our preſent object, and would be merely a repetition of what is generally well known; eſpecially ſince the publication of the intereſting memoir † of Citizen Monge, in the ſixth volume of the Annales de Chimie.

I ſhall

---

* The learned author has been carried a little too far by attending to one ſide of the queſtion only. No private property can be better grounded than a man's right to the reſults of his own intellect; and there is none that he can better defend. How far the public may acquire a right of the kind here mentioned, by propoſing an adequate equivalent for individual exertions and expence, is a queſtion of great difficulty, whether it be morally or politically conſidered. N.

† As this memoir is not generally known in England, it will be uſeful to inſert an abſtract in this place. Annales de Chimie, VI. 300.

1. When a ſingle hair is inſpected by the microſcope, under whatever magnifying power, it appears ſmooth and poliſhed: 2. Yet it is certain that the ſurface is not equally ſmooth when rubbed in different longitudinal directions, but is compoſed either of ſcales like thoſe of fiſh, or imbricated zones like the horns of animals. For, 3. when a hair is held by the root, and drawn between the finger and thumb, there is little friction, and no noiſe; but, on the contrary, when it is held by the point, and drawn in the oppoſite direction, the reſiſtance is more conſiderable, the motion tremulous, and attended with a chirping noiſe. And again, 4. If an hair be held between the finger and thumb, and rubbed, by alternately moving them in the direction of its length, a progreſſive motion will be produced, which is always with the root end foremoſt. 5. The ſame ſtructure may be ſhewn by tying two hairs together, and then giving the knot a few blows between the palms of the hands; for the knot will either untie itſelf, or draw cloſer, accordingly as the aſperities of the ſurfaces are placed in the tying. 6, From this mechaniſm, which is common to wool and every other kind of hair, may be deduced the harſh feel of woollens againſt the ſkin compared with linens, and their irritating effect upon wounds. 7. It is the diſpoſition to progreſſive motion endwiſe which cauſes hair to entangle and felt itſelf together, when preſſed by the hatter between two linen cloths; to which laſt they do not unite, becauſe its fibres are ſmooth. 8. Cut hair is better than ſuch as is plucked, becauſe the bulbous roots of the latter prevent the progreſſive motion. 9. The fibres of wool, being crooked, muſt naturally move in curve lined directions; but thoſe of the hare, the rabbit,

I ſhall confine myſelf to the obſervation, that a workman, by means of the bow, beats, ſpreads, and diſtributes equally in ſmall flocks upon a plane ſurface the quantity of wool he has ſelected: that he then covers it with a linen cloth ſlightly moiſtened, and by means of a gradual preſſure in every direction he connects the different fibres, which, by intertwining and croſſing each other, form a piece of ſtuff of a ſoft and ſpongy texture. Upon this firſt piece he applies another formed in the ſame manner, and ſometimes a third or fourth, according to the nature of the materials, and the intended thickneſs and conſiſtence of the work. Theſe different pieces are ſucceſſively brought together, and diſpoſed in a form ſuitable to the article* he propoſes to fabricate; and, in order to effect the coheſion, he uſes a number of preſſures and alternate motions in different directions, during which he preſerves the ſuppleneſs and flexibility of the material by ſlight aſperſions of warm water.

This work, which is entirely mechanical, forms the felt, which is a kind of ſoft ſpongy ſtuff, of greater or leſs thickneſs, and in its firſt ſtate of a looſe imperfect texture. The fibres it is compoſed of, are yet weakly connected, and would ſoon diſunite again. In order to give the requiſite denſity and conſiſtence, it is neceſſary that it ſhould undergo the operation of fulling.

This operation, which is in a certain reſpect the completion of felting, has for its objects the intimate connection of the fibres, and a more perfect and durable coheſion of the whole maſs. But for this purpoſe the mere mechanical act of preſſure is inſufficient. In this way the reſult would be a formleſs maſs, without conſiſtence. Experience has long ſhewn, that for the fulling it is neceſſary to make uſe of a bath of water heated nearly to ebullition, into which are put ten or fifteen pounds of lees of wine *(lie)* for each hundred pounds of water. The heat is kept up during the whole time of working, and every three or four hours a new quantity of lees is added.

Into this bath the workmen plunge their felt, and begin their ſecond proceſs. The felt

---

rabbit, and the beaver, being ſtraight, cannot be uſed alone in felting till after a previous operation, which conſiſts in rubbing them with a nitrous ſolution of mercury, by means of a bruſh, before they are ſeparated from the ſkin. The ſolution, acting on one ſide of the hairs only, renders them crooked. 10. The ſtraight hairs are uſed for facing hats, in which operation the action muſt be continued for a determinate time only, otherwiſe they would paſs through and come out on the oppoſite ſide. 11. The diſpoſition of wool to felt itſelf is an impediment to the carding and ſpinning proceſſes; in which the operators therefore uſe oil, which diminiſhes the power of the hairs to act on each other. 12. But at the fulling-mill ſoap and marle are uſed, which carry off the oil, and reſtore the wool to its former ſtate; in conſequence of which the fulling proceſs takes place, and the cloths are rendered narrower, ſhorter, cloſer, ſtronger, and thicker. 13. The balls of hair in the ſtomachs of certain animals which lick one another are felted by the action of the ſtomach. 14. To which I may add, that a ſimilar cauſe is the chief reaſon why beds of cock and hen feathers are inferior to thoſe of gooſe feathers. The latter being much ſtraighter do not form the hard flat balls which abound in the former. If a machine were conſtructed either to cut off the quill part of cock and hen feathers, or to break the ſtem in three or four places, it would probably render the article of much more value. This laſt operation might be done by two fluted cylinders. N.

* Felt may be uſefully employed in a variety of objects. Some time ago an artiſt imagined it might be applicable to the ſoles of ſhoes, and made ſeveral pair. They were of good uſe in dry weather; but, notwithſtanding his having placed a reſinous ſubſtance between the inner and outer ſole, they eaſily abſorbed moiſture, and became heavy and unpleaſant. But it may be practicable, by a farther attention to the ſubject and new expedients, to give more compactneſs and leſs permeability to the felt. The trial deſerves to be made; for which reaſon I have mentioned it in this place. C.

is dipped in, and immediately again taken out and ſqueezed bended and rolled, by preſſure in different directions, ſometimes with the hand defended by leather, and ſometimes by means of a roller or other ſimilar inſtrument. The immerſion and working of the felt is repeated, and the operation continued until the ſtuff is well condenſed, and has acquired the requiſite ſolidity.

Such are the proceſſes uſed in manufactories, and they conſtitute the whole of what is explained in the *Deſcription des Arts*, and in the new edition of the *Encyclopédie Methodique*.

Since the operation of fulling is employed to form a denſe and compact ſtuff with the fibres or hairs, and to determine the intimate coheſion of its component parts; and ſince the mere mechanical operation is not ſufficient for this purpoſe, even with the aſſiſtance of a water-bath at the boiling-heat, without the addition of the lees as a neceſſary condition;—this laſt muſt be conſidered as a chemical ſolvent, which acts directly on the ſubſtance of the hairs themſelves, and produces, either by ſoftening or ſwelling them, an alteration neceſſary to inſure the coheſion of the different fibres of the ſtuff. But the lees being compoſed of the mucilaginous and colouring parts which are ſeparated, together with a great quantity of tartar, or the acidulous tartrite of pot-aſh, it became neceſſary to aſcertain, in a poſitive manner, what might be the principle of its action.

The editor of the Encyclopédie has not heſitated to affirm, that it is the alkali or pot-aſh of the lees which determines the fulling. But in order to ſhew how erroneous this aſſertion is, nothing more is neceſſary than to dip a piece of blue paper into the bath, by which the former becomes inſtantly red; and if, after ſeveral hours work, the ſtate of the bath be again examined, it is found that the acidulous tartrite of pot-aſh is partly exhauſted, and the workmen ſoon perceive, from the difficulty of continuing their work, that a new quantity is required to be added. And again, if we conſider the ſparing ſolubility of the acidulous tartrite of pot-aſh in cold water, it is eaſily ſeen why in this proceſs the water muſt be kept nearly boiling. Whence it is evident that it muſt act by the portion of acidule it contains.

This firſt obſervation induced me to think that the ſulphuric acid might be advantageouſly ſubſtituted in the place of the lees; and as twelve pounds of lees are uſually added to one hundred of water, I eſtimated by approximation that one gros of ſulphuric acid would be equivalent to at leaſt one pound of the lees, and conſequently that twelve gros of ſulphuric acid would be ſufficient for one hundred pounds of water.

Experiment ſoon confirmed my conjectures; and after a firſt trial, which the manufacturer did not conſent to make without trepidation, it was aſcertained that the uſe of the ſulphuric acid is greatly to be preferred to that of wine-lees; that it is not only much more economical, but ſtill more convenient in the uſe; and, what is ſtill more important, the health of the workman is not injured by the exceſs and duration of the heat, the thick vapours, and the diſguſting odour which exhales from the bath, particularly when the lees have been altered by mouldineſs and putrefaction, which is very common in theſe manufactories *.

---

* I cannot avoid mentioning a ſmall inconvenience which was experienced the firſt time the ſulphuric acid was uſed. A bottle was filled with the quantity requiſite for the bath intended to be made uſe of; and as the acid was poured out from ſome height without any precaution, ſome drops were diſperſed, which, falling on the hands or clothes of the operator, occaſioned ſpots by corroſion. To avoid this, it is only required that the bottle ſhould be plunged in the bath, and its contents poured out, ſo as to occaſion no ſcattering of drops. C.

In fact, when the ſulphuric acid is employed, it is uſeleſs to keep the bath nearly boiling, as was formerly done. A degree of heat of twenty-five or thirty degrees (90° or 100° of Fahrenheit) is ſufficient for good fulling. The ſaving of fuel is an object of importance in manufactories; and as very little fire is neceſſary when ſulphuric acid is uſed, cauldrons of lead may be ſubſtituted inſtead of copper boilers, the firſt coſt and annual repair of which are very conſiderable.

The felts prepared by the new proceſs are alſo of a very ſuperior quality to thoſe which have been worked in the bath with wine-lees. In fact, the mucilaginous and colouring matters of the lees, which are ſuſpended in the bath, penetrate the texture of the ſtuff, and adhere with more or leſs force; and when after having paſſed the hats through the dye they are beaten, a fine black duſt flies off in great abundance, which not only weakens the texture of the felt, but by diffuſing itſelf through the manufactory greatly incommodes the workmen, and frequently occaſions coughs and diſorders of the throat.

The manufacturer, accordingly, who made this firſt trial, continues to employ it in preference to the old proceſs; and, according to his report, the advantages are incalculable: "Hats felted in this manner," ſays he, in one of his letters, "are not only clear of the powder which abounds in the others, but they take the dye better, *ils s'ejarrent mieux*, and are cleaner. The workman has not his bath ſo hot, which is to him a convenience no leſs valuable than the ſaving of fuel to the maſter.

Beſides theſe advantages, the wine-lees formerly made uſe of by the hatters in large quantities will remain for other uſes, in the fabrication of ſaltpetre, ſoft ſoaps, and for other deſirable purpoſes *.

When I had aſcertained that the fulling of hats was principally determined by the portion of acid which exiſts in the lees, I determined to ſubſtitute the oil of vitriol, or ſulphuric acid, becauſe it is found in the market at a reaſonable rate. But though all the acids have common properties in which they reſemble each other, they have alſo others by which each is peculiarly diſtinguiſhed. It would therefore be of importance to examine, among the numerous claſs of acids, whether ſome one might not have a more direct action upon the ſtuff than any of the others, ſo as to ſhorten the labour, without prejudice to the article or the economy of its preparation. Could not acid liquors be prepared in the manufactory itſelf, by fermenting barley, or bran, or ſome ſuch material? This is an object of reſearch, to which we may invite manufacturers, and others who are attached to the cultivation of the arts.

I ſhall not here deſcribe the proceſſes of dyeing. In the manufactory of La Côte d'Or, the nut-gall is not uſed; and oak-bark has been ſubſtituted with advantage: but theſe methods are known, and have been already publiſhed. I proceed to the fourth and laſt operation of hat-making.

It conſiſts in lining the inner ſurface of the crown, as well as of the brim of the hat, with a glutinous ſubſtance, which in drying gives firmneſs to the work, and preſerves its form. The uſual compoſition is made of gum arabic, common gum, and Flanders glue; which are diſſolved together in a ſufficient quantity of water, and brought to the requiſite thickneſs by boiling.

---

* The ſulphuric acid, more or leſs diluted with water, might be advantageouſly uſed in many of the arts. Thus the braziers uſually employ lees to clean and brighten their work; and moſt certainly they would obtain the ſame effect by ſimply dipping the piece in water ſlightly acidulated with ſulphuric acid, which would alſo ſave them the trouble of rubbing the work for a long time, as they are obliged to do when they uſe lees, &c. C.

This preparation, ſimple and eaſy as it appears, is not indifferent with regard to the beauty and duration of the work. If it be too tenacious, it renders the ſtuff dry and brittle, and after ſome months' uſe, a kind of greyiſh incruſtation is formed on the ſurface, which alters the texture. It appeared to me, that this effect was cauſed by the gum arabic which is added to the glue. I therefore ſought among the plants of our own country for a ſimple preparation, which might be ſubſtituted inſtead of theſe natural and friable gums. The mucilaginous principle abounds in a great number of plants, and may be eaſily extracted by ebullition; and a gum may even be formed by evaporation, which preſerves its ſuppleneſs and flexibility. Theſe conſiderations induced me to recommend, inſtead of the uſual preparation, a ſolution of glue in a decoction loaded with the mucilage of linſeed-oil. This preparation has long been uſed with economy in the manufactory, and advantage in the excellence of the work.

Since that time Citizen Margueron having communicated to me his obſervations on the mucilage which may be extracted from the leaves of the horſe-cheſnut-tree (*marronier d'Inde*), and having aſcertained how great a portion of mucous and adheſive matter theſe leaves afford, eſpecially when the foliation is in its vigour; a ſtrong decoction of theſe leaves has been uſed with much ſucceſs to make the preparation with glue.

There are a great many other native plants, which would be equally proper to afford factitious gums, and of which the uſe would be very advantageous in the arts. We are at preſent buſied upon this object, and hope in the courſe of time to preſent the reſults of our reſearches.

P. S. Since this memoir was written, a philoſopher attached to the arts, and who has obſerved much, has informed me that oil of vitriol or ſulphuric acid is uſed in ſome foreign manufactories in the fulling of hats, and that the proceſs is conſidered as a great ſecret. I was ignorant of this. The publications I have examined make no mention of it, and the workmen I have conſulted had no knowledge of its uſe. Beſides which, truth is always new while it continues unknown, and uſeful obſervations require to be repeated in order to make them generally adopted.

IV.

Doubts concerning the Exiſtence of a new Earth in the Mineral from New South Wales examined by WEDGWOOD *in the Year* 1790.

A PORTION of the ſame mineral from Sydney Cove in New South Wales as was formerly analyſed by the late Mr. Wedgwood, and had been preſented to M. Haidinger, Counſellor of Mines, by Sir J. Banks, has been lately examined by the celebrated M. Klaproth*. As the reſult ſeems to have inclined this able chemiſt to reject the new earth of Wedgwood, the ſubject appears ſufficiently intereſting to admit of being ſtated to the public.

---

* Beytrage zur Chemiſchen Kenntniſs der Mineral. Kœrper, b. ii.

M. Klaproth had two ſamples; the firſt conſiſting for the moſt part of black brilliant ſcales, which might have been taken for plumbago or carburet of iron, but appeared to him to be ferruginous mica; the ſecond, which contained much leſs of this ſubſtance, was conſidered by him as purer, and ſubjected to experiment.

Thirty grains were reduced to fine powder. The greyiſh colour it naturally poſſeſſed became blueiſh by levigation. Upon this powder the concentrated muriatic acid was boiled, and decanted off when cold. New acid was boiled upon the reſidue, and this operation was three times repeated.

The acid after filtration through double paper was not rendered turbid by water gradually added in ſufficient quantity. The mixture, when expoſed to heat, preſerved its tranſparency.

By ſaturating the acid with carbonate of pot-aſh, a ſmall quantity of precipitate was afforded in light flocks, which, being collected on the filtre, edulcorated and dried, weighed 3.25 grains. This precipitate being added to diluted ſulphuric acid, afforded cryſtals of alum, and left a ſmall portion of ſiliceous earth.

The reſidue, which was not acted upon by the muriatic acid, was treated in the dry way with three parts of pot-aſh, and afterwards with the muriatic acid. The gelatinous inſoluble portion, ſeparated by the filtre, edulcorated and dried, weighed 19.5 grains. It was ſilex.

The muriatic ſolution, eſſayed by pruſſic acid, afforded a blue precipitate, correſponding with about 0.25 grains of iron.

From this ſolution, when decompoſed by carbonate of pot-aſh, a quantity of alumine was ſeparated, which, when edulcorated, and dried by heat, weighed 8.5 grains, and upon being combined with ſulphuric acid was totally converted into alum.

The whole of what this chemiſt obtained from the ſand of Sydney Cove was alumine, ſilex, and iron, without the leaſt indication of any other principle. Though he operated upon thirty grains only, and it was impoſſible for him to repeat his analyſis, he obſerves that the reſult is ſufficient to render the preſence of a new earth doubtful. He refers the ſolution of this doubt to time; and on the ſuppoſition of error in Mr. Wedgwood's experiments, he is diſpoſed to think he did not filtre his acid ſolution before he added water to it, and that the earth depoſited by the diluted acid was probably nothing but ſilex chemically united to alumine.

Previous to a diſcuſſion of the probability how far Wedgwood could be miſtaken in his facts and deductions, and on what the difference between his reſults and thoſe of M. Klaproth might depend, I had purpoſed to give an abridgment of the paper of the former chemiſt from the eightieth volume of the Philoſophical Tranſactions. But after having made the ſame, and begun my comparative remarks, I found that ſo much of theſe laſt would depend upon the ſtrict ſtatement of experiments, that I determined to give ſo much of the entire paper as relates to the new earth. And this I do the more willingly, as it is not, as I am aware, to be found in any work of general circulation. What follows, therefore, is in the words of Mr. Wedgwood *.

Analytical Experiments on a Mineral from Sidney Cove in New South Wales.

THIS mineral is a mixture of fine white ſand, a ſoft white earth, ſome colourleſs micaceous particles, and a few black ones, reſembling black mica or black lead, partly looſe, or detached from one another, and partly cohering together in little friable lumps.

---

* Phil. Tranſ. LXXX. 307.

None of theſe ſubſtances ſeem to be at all acted upon by the nitrous acid, concentrated or diluted, nor by oil of vitriol diluted with about equal its meaſure of water. In the cold, or in a boiling heat, the mineral remained unaltered in its appearance, and the acids had extracted nothing from it that could be precipitated by alkali.

Oil of vitriol boiled upon the mineral to dryneſs, as in the proceſs of making alum from clay, produced no apparent change in it; but a lixivium made from this dry maſs with water, on being ſaturated with alkali, became ſomewhat turbid, and depoſited, exceeding ſlowly, a white earth in a gelatinous ſtate, too ſmall in quantity for any particular examination, but which, from its aſpect, from the manner in which it was obtained, and from the taſte of the lixivium before the addition of the alkali, was judged to be the aluminous earth.

The marine acid during digeſtion ſeemed to have as little action as the other two; but on pouring in ſome water, with a view only to dilute and waſh out only the remaining part of the acid, a remarkable difference preſented itſelf; the liquor became inſtantly white as milk, with a fine white curdly ſubſtance intermixed; the ſtrong acid having extracted ſomething which the ſimple dilution with water precipitated.

The white matter being waſhed off, more ſpirit of ſalt was added to the remainder, and the digeſtion repeated with a long tube inſerted into the mouth of the glaſs, ſo as nearly to prevent evaporation. The acid, when cold, and ſettled fine, was poured off clear, and on diluting it with water the ſame milky appearance was produced as at firſt.

The digeſtion was repeated ſeveral times ſucceſſively with freſh quantities of the acid, till no milkineſs appeared on dilution. The quantity of mineral employed was 24 grains, and the reſiduum, after the operations, waſhed and dried, weighed ſomewhat more than 19 grains, ſo that about one-fifth of it had been diſſolved. In ſome parcels of the mineral, taken up promiſcuouſly, the proportion of ſoluble matter was much leſs, and in none greater. It is only the white part, and only a portion of this, that the acid appears to act upon; the white ſand, much of the white ſoft earth, and all the black particles remain unaltered.

To try whether this tedious proceſs of ſolution could be expedited by triture or calcination, ſome of the mineral was rubbed in a mortar; and in doing this, it appeared pretty remarkable, that though the black part bore but an inconſiderable proportion to the reſt, yet the whiteneſs of the other was ſoon covered and ſuppreſſed by it; the whole becoming an uniformly black, ſhining, ſoft, unctuous maſs, like black lead, rubbed in the ſame manner with a few gritty particles, perceptible on preſſing hard with the peſtle. A pennyweight of this mixed, ſpread thin on the bottom of a porcelain veſſel, was calcined about an hour, with a fire between 30 and 40 degrees *; it became of an uniform, dull, white or grey colour, excepting a very few, and very ſmall, ſparkling, black particles, ſuſpected to be thoſe which had eluded the action of the peſtle; it loſt in weight ſix grains, or one-fourth.

The mineral thus ground and calcined was found to be juſt as difficult of ſolution as in its crude ſtate, with this additional diſadvantage, that the undiſſolved fine particles are indiſpoſed to ſettle from the liquor.

---

* By degrees of *fire* or heat *above ignition*, I mean thoſe of my thermometer; and ſome idea may be formed of their value by recollecting that they commence at viſible redneſs; and that the extreme heat of a good air-furnace of the common conſtruction is 160° or a little more. W.

In all the experiments of diſſolution, as often as the heat was at or near the boiling point of the acid, frequent and pretty ſingular burſts or exploſions happened, though the matter lay very thin in a broad-bottomed glaſs. They were ſometimes ſo conſiderable as to throw off a porcelain cup with which the glaſs was covered, and once to ſhatter the glaſs in pieces. In a heat a little below this, the extraction ſeemed to be equally complete, though more ſlow; but a heat a little below that in which wax melts, or below 140° of Fahrenheit's thermometer, appeared inſufficient.

To determine the degree of ſolution neceſſary for the precipitation of the diſſolved ſubſtance, and whether the precipitation by water be total, a meaſure of the ſolution was poured into a large glaſs, and the ſame meaſure of water added repeatedly. The third addition of water occaſioned a ſlight milkineſs, which increaſed more and more to the ſixth. The liquor being then filtered off, another meaſure of water produced a little freſh milkineſs, and an eighth rather increaſed it; a ninth and a tenth had no effect. The liquor being now again paſſed through a filter, ſolution of ſalt of tartar did not in the leaſt alter its tranſparency; ſo that after the ſolution has been diluted with eight or nine times its meaſure of water, there is nothing left in it that alkali can precipitate.

From the manner in which the ſolution is neceſſarily prepared, it cannot but contain a great redundance of acid; for the ſmall quantity of acid ſufficient for holding the ſoluble part ſuſpended, would be ſoked up or entangled by the undiſſolved part, ſo as ſcarcely to admit of any being poured off; and it cannot be diluted or waſhed out but by the ſtrong acid itſelf. The ſolution with which the above experiment was made, was reckoned to have only about ſix grains of the ſoluble matter to three ounces of ſpirit of ſalt, having been prepared by digeſting that quantity of the ſpirit, by half an ounce at a time, on thirty grains of the crude mineral.

A ſaturated ſolution was obtained by digeſting, on a ſmall portion of the ſolutions thus prepared, the precipitate thrown down by water from the larger portions till the acid would take up no more. A ſolution thus ſaturated cannot bear the ſmalleſt quantity of water; a ſingle drop, on the firſt contact, producing a milky circle round it.

Examination of the above Subſtance, extracted from the Mineral by Marine Acid, and precipitated by Water.

THIS ſubſtance waſhed and dried is indiſſoluble in water, as indeed might be expected from the manner of its preparation.

Nor is it acted upon by the nitrous or vitriolic acids, concentrated or diluted, cold or hot, nor by alkaline ſolutions, mild or cauſtic, of the volatile or fixed kind.

It is diſſolved by ſtrong marine acid, but not without the aſſiſtance of nearly the ſame degree of heat that is neceſſary for its extraction from the mineral. From this ſolution it is precipitated by water; and after repeated diſſolutions and precipitations it appears to have ſuffered no decompoſition or change.

Spirit of nitre, added to the ſaturated ſolution, makes no precipitation; and if the quantity of nitrous acid exceeds, or at leaſt does not fall much ſhort of, that of marine acid in the ſolution, the mixture ſuffers no precipitation from water. Nor does any precipitation happen, though the nitrous ſpirit be previouſly mixed with even a large quantity of water, provided the quantity of ſolution added to it does not exceed that of the nitrous ſpirit in the mixture. The appropriate menſtruum for this ſubſtance (that is, for keeping it in a ſtate of dilute ſolution) appears, therefore, to be aqua regia; and the due proportion of the two acids, of any given ſtrength, might be determined, if neceſſary, with greater accuracy and facility for this than for any other body I know of; becauſe, if there be even a very minute ſurplus of marine acid in the ſolution, that ſurplus will inſtantly betray itſelf, on dropping a little into water; all that was diſſolved by it, and no more, being precipitated by the water. It may be obſerved, however, that where an addition of nitrous acid is uſed, a ſaturated ſolution cannot be obtained (unleſs by ſubſequent evaporation), the ſame quantity of marine acid being neceſſary with as without that addition: the change or modification which the nitrous acid produces in the marine, ſerves, in the preſent inſtance, not for effecting the ſolution, as in the caſe of gold and ſome other metals, but merely for enabling it to bear water without depoſiting its contents.

Oil of vitriol dropped into the ſaturated marine ſolution, occaſions no change till its quantity comes to be about equal to that of the ſolution; a conſiderable effervescence and heat are then produced, the liquor becomes milky, and the marine acid is extricated in its uſual white fumes. The mixture, heated nearly to boiling, becomes tranſparent, and afterwards continues ſo in the cold. This vitriolic ſolution is precipitated by water, and the precipitate is rediſſolved by marine acid.

The ſaturated marine ſolution is indiſpoſed to cryſtalliſe. By continued evaporation in gentle heat it becomes thick and butyraceous, and in this ſtate it ſoon liquefies again on expoſure to the air. The butyraceous maſs, in colour whitiſh or pale yellow, is not corroſive, like the ſimilar preparations made from ſome metallic bodies, nor is it more pungent in taſte, but rather leſs ſo, than the combination of the ſame acid with calcareous earth. In a heat increaſed nearly to ignition, the acid is diſengaged, and riſes in white fumes, which, received in a cold phial, condenſe into colourleſs drops, without any appearance of ſublimate. From the remaining white maſs, ſpirit of nitre extracts ſo little as to exhibit only a ſlight milkineſs on adding alkali; a proof that nearly all the marine acid had been expelled; for, while that acid remains, the whole is diſſoluble by the nitrous.

The ſubſtance in queſtion is not precipitated by Pruſſian lixivium. A drop or two of the lixivium do indeed occaſion a little white or blueiſh white precipitation in the ſaturated marine ſolution; but in the more dilute no turbidneſs appears till the quantity of lixivium is ſuch as to produce that effect by its mere water; and when the precipitate has at length been formed, it rediſſolves in marine acid as eaſily as that made by water; whereas the precipitates reſulting from the union of the Pruſſian matter are not acted upon by acids till that matter has been extracted from them by an alkali. For further ſatisfaction, in this important point, the experiment was repeated with a ſolution in aqua regia. Here the Pruſſian lixivium, in whatever quantity it was added, occaſioned no precipitation at all (only the uſual blueiſhneſs ariſing from the iron always found in the common acids); and pure alkali added afterwards precipitated the original white ſubſtance unchanged.

The following experiments of precipitation by alkalis were made with the marine ſolution before the effect of an addition of nitrous acid had been diſcovered; and they were made with ſo much care and attention, that it was not thought neceſſary to repeat them afterwards. To obviate as much as poſſible the equivocal reſults that might ariſe from water contained in the precipitants, the different alkalis were applied in the drieſt ſtate I could reduce them to; viz. pure ſalt of tartar, kept for ſome time in a heat juſt below redneſs; cryſtals of marine alkali melted and dried in the ſame manner; volatile alkali in cryſtals, a little ſurplus acid being in this inſtance previouſly added to the ſolution to counteract the water of cryſtallization in the alkali; *ſalt of tartar cauſticated* by quicklime, and haſtily evaporated to dryneſs; the *marine alkali cauſticated* in like manner; and the vapour of cauſtic volatile alkali, ariſing with a very gentle heat from a retort into a phial containing the ſolution. All theſe alkalis occaſioned copious precipitations. All the precipitates, after waſhing and drying, were found to rediſſolve in marine acid; and from all theſe ſolutions the original ſubſtance was precipitated unaltered on diluting them with water.

In ſtrong fire, from 142 to 156 degrees, this ſubſtance diſcovers a much greater fuſibility than any of the known ſimple earths. In a ſmall veſſel made of tobacco-pipe clay, it melted, and glazed the bottom; and on a bed of powdered flint, preſſed ſmooth in the manner of a cupel, it did the ſame. Magneſia or chalk would indeed vitrify in the clay veſſel; but on flint no one of the known earths ſhews any tendency to vitrification in that heat *. In a cavity ſcooped in a lump of chalk, this ſubſtance, in the heat above mentioned, ran into a ſmall round bead, ſmooth, whitiſh, and opake, not in the leaſt adhering to the calcareous maſs. On a bed of powdered quick-lime, it formed a browniſh ſcoria, which in great part had ſunk into the lime, and ſeemed to have united with it. On Mr. Henry's magneſia, uncalcined, it melted and ſunk in completely, leaving only a ſlight browniſh ſtain on the ſurface where it had lain. On beds of the baro-ſelenite, and barytic quick-lime, it likewiſe melted and ſunk in, leaving a diſcoloured ſpot behind; but whether it really united with the ſubſtrata, or only penetrated into their interſtices, could not be determined with certainty, on account of the ſmallneſs of the quantity of the mineral I had to work upon.

On a bed of powdered charcoal, in a crucible cloſely luted, this ſubſtance likewiſe melted: and therefore it may be preſumed not to have owed its fuſion in the above experiments to the ſame cauſe to which ſome of the common ſimple earths, in certain circumſtances, owe theirs, namely their union with the matter of the veſſel or ſupport; that is, with an earth or earths of a different kind from themſelves; but to poſſeſs a fuſibility ſtrictly its own, which takes place in a fire of 150 degrees, or perhaps leſs.

As charcoal in fine powder aſſumes a kind of fluidity in the fire, ſimilar to that which powdered gypſum exhibits in a ſmall heat, its ſurface had changed from concave to horizontal, and the bead had ſunk to the bottom; it was rough and black on the outſide, and whitiſh within. On repeating the experiments in a cavity ſcooped in a piece of charcoal, the reſult was a blackiſh bead like the former, only ſmooth on the outſide, with ſomething of metallic brightneſs, not unlike that of black-lead. Both beads were very light, and had a conſiderable cavity within. All the internal part was whitiſh, without the leaſt metallic aſpect; and the external gloſſy blackneſs appeared to be only the ſtain which charcoal powder communicates in ſtrong fire to ſome earthy bodies that have a tendency to vitrify. By boiling in concentrated marine acid, a part of the beads was diſſolved, precipitable, as at firſt, by water; but an accident prevented the proceſs from being continued ſufficiently to determine whether the whole could be diſſolved or not.

---

* It may be proper juſt to mention that I find this to be a very commodious and ſure method of trying, in ſmall, whether any given earthy body be fuſible with other earths. If the body is diſpoſed to vitrify with any proportion of clay or flint, for inſtance, it will equally vitrify when a little of it is applied, or even duſted only, on the bottom of a ſmall cup made of clay, or on a ſmooth cloſe bed of finely powdered flint. The body, in this mode of application, ſeems to unite with only juſt ſo much of the matter of the ſubſtratum as is requiſite for their moſt perfect fuſion together, and has nothing elſe in contact with it, ſo that no deception can ariſe; whereas, if mixed with the ſame matter, there might be no appearance of fuſion, unleſs certain favourable proportions of the two ſhould chance to be hit upon; and even then, if the quantity be ſmall, it would not be certain but that the fuſion might have originated from the matter of the crucible. W.

By this fuſibility in the fire, ſolubility in one only of the common mineral acids, and parting with the acid in a heat below ignition, precipitability by water, and non-precipitability by Pruſſian lixivium, this ſubſtance is ſtrongly diſcriminated from all the known earths and metallic calces. And as it ſuffers no decompoſition from any of the alkalis in any of the uſual modes of application, I preſume it cannot be conſidered as a combination of any of thoſe earths or calces with any of the known acids; for all the combinations of this kind would, in one or other of the above methods of trial, have had the earth or metal diſengaged from the acid.

Whether this ſubſtance belongs to the *earthy* or *metallic* claſs, I cannot abſolutely determine; but am inclined to refer it to the earthy; becauſe, though brought into perfect fuſion in contact with inflammable matter and in cloſe veſſels, it does not aſſume the appearance which metallic bodies do in that circumſtance *.

The black ſubſtance, which ſeems to have compoſed about one-fifth part of the crude mineral, was found to reſemble plumbago in its leading properties, but its reſidue did not appear to be iron. The remaining three-fifths of the mineral, which reſiſted the humid attacks in Mr. Wedgwood's experiments, was probably ſilex; but he does not ſpeak of any direct examination of its properties by fuſion with alkalis, the ſparry acid, or otherwiſe.

On the preceding experiments of M. Klaproth and the late Mr. Wedgwood, the queſtion which in the firſt place appears to demand ſolution, is, whether the ſame mineral was examined by both chemiſts? If this ſhould not appear probable, it will be unneceſſary to enter upon the ſubſequent enquiry, which ſet of experiments is moſt likely to be erroneous?

On the hiſtorical evidence, as far as regards the ſameneſs of the parcels of mineral, I think there is little to be ſaid. Both were obtained from Sir Joſeph Banks; the firſt directly, and the latter with the intervention of the reſpectable, and as it may be concluded careful, M. Haidinger. Neverthleſs, if the obvious deſcription and decided experiments ſhould point out a difference, it will afford more than a preſumptive proof that they were not the ſame. 1. M. Klaproth's mineral was greyiſh, and became blueiſh by levigation. Mr. Wedgwood's was a fine white ſand, a ſoft white earth, with ſome colourleſs micaceous particles, and a few reſembling plumbago. When levigated, it became black, ſhining, ſoft, and unctuous. 2. Klaproth's marine ſolution, after filtration, afforded no precipitate by the addition of water. Wedgwood's ſolution in the ſame acid, when cold, ſettled fine; and, decanted off clear, afforded a white precipitate with water to the whole amount of its contents. After ſix meaſures of water had cauſed much of the precipitate to fall, the liquor was filtered, and more water threw down a little more. 3. Klaproth's marine ſolution afforded a precipitate by the addition of an alkali which was ſoluble in vitriolic acid, and afforded alum. Wedgwood's precipitate by means of water was inſoluble in the nitrous or vitriolic acids. The indirect ſolution of this matter in vitriolic acid was precipitated by water. 4. M. Klaproth's experiments ſhewed nothing but ſilex, alumine, and a minute portion of iron. He thinks it probable that Wedgwood's precipitate by water was ſilex combined with alumine. But in Wedgwood's experiments this matter was fuſed without addition in an hole ſcooped in charcoal, and alſo in an hole in a lump of chalk, to which it did not adhere. Kirwan [*] found equal parts of alumine and ſilex infuſible at 160° of Wedgwood; and Achard found them infuſible in a porcelain furnace, in all proportions; not to mention that very refractory veſſels are made out of the ſame two earths mixed together. 5. I do not ſee how Wedgwood's method of ſubſidence and decantation could have left any thing ſuſpended which Klaproth's filtre could have detained.

---

* Here ends the extract in Mr. Wedgwood's own words.

Hence it ſeems fair to conclude that the two minerals were not the ſame, however this may have happened; and that the exiſtence of the new fuſible earth of Wedgwood ſtands on the ſame evidence as before, namely, his experiments, which have not yet been repeated, that I know of.

V.

A Philoſophical Memoir, containing—1. Experiments relative to the Propagation of Sound in different Solid and Fluid Mediums.—And 2. An Experimental Enquiry into the Cauſe of the Reſonance of Muſical Inſtruments. By M. PERROLE [†]. *With Annotations.*

AS the tranſmiſſion of ſound through water [‡], through air of greater or leſs denſity [§], and through different gaſeous ſubſtances [||], has added to the amount of our philoſophical knowledge, I have thought an abundant harveſt of new facts might be obtained by cauſing ſound to paſs through a great number of bodies of different kinds, as well ſolid as fluid, and comparing their effects together.

Theſe are the views which have directed the experiments of which I ſhall give an account in the firſt part of this memoir; and in the ſecond I ſhall avail myſelf of the reſults in an endeavour to aſcertain the cauſe of the reſonance of bodies.

PART I.

AS all the trials depend upon the following experiment, it is neceſſary to give particular attention to its detail.

Experiment I. and chief. Cloſe the ears with chewed or maſhed paper; ſuſpend a watch by an hook. Place the ear at the diſtance of two lines from the watch, and you will not hear its vibrations. Then take a ſolid body, ſuch as a ſmall cylinder of wood, of one foot or one foot and a half in length, and one or two lines in diameter. Place it in contact at one extremity with the watch, and apply the other end to one of the numerous parts of the head which propagate ſound by the touch*; for example, with the cartilaginous parts of the ear; you will hear the ſound much better than if the ear had not been cloſed, and the ſonorous body had been placed in the air at the ſmalleſt diſtance from the organ.

---

[*] Mineralogy, i. 58.

[†] Mem. de l'Acad. Royale de Turin, v. 195.

[‡] Nollet, Mem. de l'Acad. Royale de Paris, 1743.

[§] Muſchenbroek, n. 1442.—Nollet, Leçons de Phyſique, iii. 355.

[||] Prieſtley's Experiments and Obſervations, &c. and my Experiments in the Turin Memoirs for 1786, 1787. P.

As the ſound was not perceptible at the diſtance of two lines in the firſt diſpoſition, and was very ſtrongly perceived in the ſecond; it is evident that the ſmall cylinder propagated ſound better than atmoſpheric air.

When we reflect on this experiment, and the reſult it affords, we may, without difficulty, perceive, that, in order to aſcertain the reſpective force of the propagation of ſound through ſolid bodies, nothing more is required to be done than to procure ſubſtances of different natures, to give them the ſame form, and ſubject them to an experiment of this kind. In this manner I made the following experiments:

Experiment II. I procured ſmall cylinders of the dry wood of fir, oak, box, cherry-tree, cheſnut-tree, and logwood, each one line in diameter, and one foot in length. The ears being then cloſed, I placed them ſucceſſively in contact with the watch, and at the ſame time with the cartilaginous part of the ear, as in the preceding experiment.

The different cylinders tranſmitted the ſound very well; but its tone† ſeemed to vary whenever a new cylinder was uſed, and the intenſity appeared to be never exactly the ſame. We had no means of aſcertaining the difference of the tone *(timbre)*, but the intenſity appeared in the following order, beginning with thoſe cylinders which appeared to propagate ſound with the greateſt activity: fir, logwood, box, oak, cherry-tree, cheſnut.

Experiment III. I determined to extend my reſearches to the metals, and conſtructed metallic cylinders ſimilar to the former. When ſubjected to the ſame trial, they tranſmitted the ſound in general rather worſe than the cylinders of wood.

The nature of the ſound appeared likewiſe to differ in the cylinders of wood and thoſe of metal. The tone was not exactly the ſame in the different metals, and the intenſity followed this order: iron, copper, ſilver, gold, tin, lead.

Experiment IV. I hung my watch ſucceſſively to ſtrings of ſilk, of wool, of hemp, of flax, hair, and gut, which were nearly of the ſame diameter and exactly of the ſame length as the ſolid cylinders. One extremity of the ſtring was applied by the hand in contact with the cartilage of the ear, while the watch reſted on the oppoſite extremity of the ſtring, which touched no part of the body. The ſtrings thus extended propagated the ſound with leſs force than ſolid bodies, and modified it in a remarkable manner. The tone in each of the ſtrings appeared to be different, and the intenſity followed this order :—gut, hair, ſilk, hemp, flax, wool, cotton.

---

* Almoſt every part of the head propagates ſound when in immediate contact with a ſonorous body. This may be eaſily proved by applying a watch to the ſeveral parts after having cloſed the ears. See my Diſſert. Anat. &c. and my Recherches ſur l'Organe de l'Ouie et la Propr. des Sons, tom. iii. des Mem. de la Soc. R. de Medec. et le Journal de Phyſique, 1773, tom. ii. P.

† *Son timbre*, which does not, as I apprehend, imply any difference of acuteneſs or gravity in ſound, but another relation for which I know no Engliſh word but tone. Thus the tone *(timbre)* of the hautboy differs greatly from that of the flute when both are in uniſon, and ſo likewiſe do the different tones *(tons)* of the ſame kind of inſtrument. It does not appear from the text, whether the acuteneſs or gravity of theſe tranſmitted ſounds did differ from each other. N.

From the foregoing experiments it follows :—1. That hard bodies and ſtretched ſtrings tranſmit ſound much better than atmoſpheric air. 2. That each of theſe mediums tranſmits it in a manner which is peculiar to itſelf, ſo that the kind and intenſity of the ſound are never exactly the ſame, as far as can be judged on trials which do not always preſent very ſtriking reſults *. 3. That in general wood tranſmits ſound very well; that the metals tranſmit it with ſomewhat leſs energy, and that ſtretched ſtrings occupy the third place in the ſcale of power in this reſpect.

Experiment V. Being determined to extend my reſearches, I cauſed the ſound of the watch to paſs through pieces of zinc, antimony, glaſs, ſal gem, gypſum, dried clay, and marble. As I could not give the ſame form to theſe different ſubſtances, I was unable to determine with preciſion their reſpective powers of tranſmiſſion ; but I obſerved that all theſe bodies tranſmitted ſounds better than air, and that it was modified in a particular manner by each of them. Marble was remarkable for the little force with which it tranſmitted the ſonorous movements. Two pieces of this ſubſtance, of different form and ſize, propagated it in a weak and almoſt inſenſible manner.

Theſe are the experiments I have made with ſolid bodies. To complete the ſeries, it remained to ſubmit fluids to ſimilar trials.

I have already publiſhed my experiments on aëriform ſubſtances †. In this place I ſhall relate thoſe I have made upon liquids. This part of my operations required a different proceſs.

Experiment VI. I cloſed all the joints of my watch with ſoft wax, and then ſuſpended it by a ſilk thread. In this ſtate I hung it by an iron branch fixed in the wall, ſo that the watch remained ſuſpended in the middle of a glaſs veſſel five inches in diameter, and ſeven inches high, taking care that neither the watch nor the thread touched the veſſel in any part. I remarked the kind of ſound afforded by the watch, and the diſtance at which I ceaſed to hear it. After having marked this point, I then filled the veſſel with water, into which I again ſuffered the watch to deſcend, with the ſame precaution of not ſuffering it or the thread to touch the veſſel.

The tone *(timbre)* was changed in the water in a ſtriking manner. The ſound was propagated in ſo lively a manner that the glaſs, and a ſmall table of wood, on which it ſtood at a diſtance from the wall, ſeemed to undergo direct percuſſions from a ſolid body. But what appeared ſtill more aſtoniſhing was, that in the midſt of all theſe agitations the fluid in which the watch was plunged was perfectly tranquil, and its ſurface not in the ſlighteſt degree agitated.

By ſubſtituting different liquids in the place of the water, I had reſults in general analogous to thoſe I had obtained with that fluid ; but each medium gave a different modification to the ſound, of which the intenſity was noted as follows :

---

* Such chiefly are thoſe which were obtained with the cylinders of wood, and ſtretched ſtrings. P.

† Mem. de l'Acad. Roy. des Sc. de Turin, 1786, 1787.

Intenſity of Sound obſerved in different Fluids.

	Feet.
1. In the air ſerving as the term of compariſon, it ceaſes to be heard at the diſtance of	8
2. In the water, at that of	20
3. Oil-olive	16
4. Oil of turpentine	14
5. Spirit of wine	21

It is proper to obſerve, that on repeating theſe trials I obſerved ſome variations in the intenſity, which appeared to depend on the organ of ſenſe or accidental noiſes.

From the experiments made upon liquids, it follows:—1. That theſe as well as ſolids do tranſmit ſounds much better than air, and that even the fat oils are not to be excepted *.

2. That each fluid upon trial is found to modify the ſound in a peculiar manner.

3. Philoſophers maintain the opinion that ſound is propagated in the air by means of certain motions or undulations, which the tranſparence of that fluid prevents our ſeeing. My experiments with fluids which do not elude the ſight, and in which no motion was perceived, notwithſtanding the very effectual tranſmiſſion of ſound, may render this in ſome reſpect doubtful.

4. Laſtly, the experiments on ſolids, fluids, and thoſe I have publiſhed on the gaſes †, afford the probable concluſion that all mediums produce particular modifications in the tone *(timbre)* and the intenſity of ſound; or otherwiſe, that the ſame ſound varies as often as it paſſes through a different medium. I ſhall now paſs to the experiments which conſtitute the ſecond part of this memoir.

PART II.

EVERY one has obſerved, that if a watch be placed upon a table, the ſound is very evidently increaſed. The difference between the ſound of a tuning-fork, when it vibrates without being placed in contact with any ſolid body, and that which it affords when its handle is preſſed againſt a ſolid body with a large ſurface, is alſo well known. The experiments related in the former part of this memoir having led me to expect that the increaſe of force and harmony in theſe circumſtances was owing to the property poſſeſſed by wood of propagating ſound better than the external air, and modifying its tone; I determined to aſcertain the truth by experiment.

The different powers I had obſerved in wood and marble, with regard to the propagation of ſound, appeared proper to throw ſome light on this important queſtion. In fact, if the modifications of the ſound of the tuning-fork and the watch, when placed on a table of wood, be owing to the energy with which this ſubſtance tranſmits the ſound, and the marble propagating it very weakly, it would follow that a table of marble ought not to fortify the ſound, or at leaſt to produce very little effect on theſe ſonorous bodies. On theſe conſiderations I made the following experiments:

---

* Morhof, *Stent.* pag. 104. affirmed that fat oils could not tranſmit ſonorous modulations. P.

† Loco citato.

Experiment I. I applied the tuning-fork upon a table of wood; and when it had ceaſed to vibrate I applied my watch to the ſame ſurface. The ſound in each was increaſed in proportion to their reſpective intenſities. I then removed the wooden top of the table, and ſubſtituted in its place another of marble, of the ſame dimenſions. The tuning-fork and watch were applied as before. The ſound of the former was augmented, though much leſs than when it was applied on the wooden covering. The ſound of the watch was ſcarce perceptibly increaſed. I could perceive little difference between its ſound, whether it touched the marble or hung in the air at the ſame diſtance from the ear.

Though this experiment ſtrongly confirms my conjecture, I determined, nevertheleſs, to ſubject it to another proof:

Experiment II. I placed my watch on the wooden table, and cloſed my ears with chewed paper. When the ear was at a few lines diſtance from the table, I could not hear the vibrations of the watch. I then placed my ear in contact with one of the ſmall wooden cylinders uſed in the experiments already deſcribed. The oppoſite end of the cylinder was placed in contact with the table.

The ſound of the watch immediately and forcibly ſtruck the ear. I made this application to every part of the table, not excepting even the feet; and the ſound was in every caſe diſtinctly heard. The experiment was then repeated with the marble covering inſtead of the wood. The vibrations of the watch were heard in an imperfect or indiſtinct manner, and only when the wooden cylinder was applied at a ſhort diſtance from the ſonorous body. I did not uſe the tuning-fork in this experiment, becauſe, in ſpite of every precaution, it was not poſſible to cloſe the ears ſo perfectly but that the ſound ſtill remained perceptible.

In order to eſtabliſh my conjecture on the moſt ſolid baſis, it remained to repeat the experiments upon tables of all the ſubſtances uſed in the experiments related in the firſt part of this memoir, and to ſhew that the reſonances follow the proportion of the conducting powers of the bodies. But the difficulties which oppoſed the execution of this plan induced me to confine my enquiries to aſcertain whether the reſonance does not vary in different bodies, as well as the power of tranſmitting ſound.

Experiment III. I therefore placed the tuning-fork ſucceſſively upon plates of earthenware, of porcelain, on plates of glaſs, and upon thin iſolated plates of copper and tinned iron. The ſound was fortified by theſe bodies, and the tone never appeared to be exactly the ſame in any two experiments.

Theſe trials naturally led me to examine the ſame ſounds upon muſical inſtruments. With this view I applied firſt the tuning-fork and afterwards the watch, upon baſes, violins, mandolins, guitars, harpſichords, and horns. Both ſounds were proportionally increaſed.

They appeared to acquire more force and melody by means of the muſical inſtruments than on the other bodies. The intenſity ſeemed directly in proportion to the volume of the inſtrument.

From theſe experiments it follows:—1. That all the ſubſtances which were tried, which poſſeſſed extended ſurfaces, fortify the weak ſounds produced by bodies which touch them, and modify the tone in a manner peculiar to each.

2. That theſe effects ariſe from the tranſmiſſion of ſound by ſolid bodies being in general better than by the air, and the peculiar modification of the tone by each.

3. That the reſonance of muſical inſtruments is more particularly to be attributed to this cauſe [*].

4. The experiments with muſical inſtruments afford reaſon to conclude that the volume of bodies has an influence on their reſounding properties.

5. M. de Maupertuis [†] has affirmed that the reſonance of muſical inſtruments is owing to the inſtrument containing fibres of every poſſible length; each ſound puts thoſe in motion which are either in uniſon or concord with itſelf, while the others remain motionleſs [‡]. But the ſecond experiment, in which it is ſhewn that there is no part of any reſounding body which does not tranſmit the ſound, will not admit of the adoption of the ingenious thought of that celebrated author.

6. As marble in ſome degree extinguiſhes ſound, and bears the ſame rank among ſolid bodies as inflammable air among fluids, it is not adviſable to uſe it in the conſtruction of churches, concert-rooms, theatres, or other edifices in which the propagation of ſound is deſirable.

Such are the principal reſults of experiments which, on the whole, have engaged my attention for a number of years; notwithſtanding which, I have not been yet able to extend them, particularly thoſe belonging to the ſecond part, as far as they are capable of being carried. I ſhall not, however, have the mortification to think my endeavours uſeleſs, if the ſociety to whom my works are addreſſed ſhall think they may conſtitute an addition to the ſum of the diſcoveries in ſcience which they continually preſent to the world.

Annotations to the Paper of M. PERROLE *on Sound.*

THE philoſophy of ſound is ſtill exceedingly imperfect. Mathematicians have eſtabliſhed theories upon the ſimple data that ſonorous bodies vibrate, and that theſe vibrations are communicated to the ſurrounding elaſtic fluid, of which the denſity and ſpring are meaſurable. M. Perrole's experiments ſhew that the object of reſearch comprehends much more than this. The following queries and remarks may perhaps prove of ſome advantage to the curious enquirer:

1. There can be no doubt but that undulation in the air accompanies its propagation of ſound, and that ſonorous bodies are put into a vibrating ſtate by percuſſion. The proofs of this are too numerous to be detailed. Sound propagated through fluids ſeems to diminiſh in proportion to the diſtance, and ſo probably it does in ſolid bodies. Its intenſity muſt depend upon the elaſticity and denſity jointly. The military practice of lying down on the ground to liſten for footſteps is well known; and Dr. Franklin relates that he heard the blow of two ſtones ſtruck together at the diſtance of near a mile, ſmart and ſtrong as if cloſe at the ear [§]. We have no information reſpecting the velocities of ſounds propagated through ſolid bodies.

---

[*] The numerous ſurfaces which theſe inſtruments preſent muſt likewiſe render them more ſonorous. P.

[†] Acad. Paris, 1724.

[‡] The memoir of M. de Maupertuis relates to ſtringed inſtruments only. P.

[§] Experiments and Obſervations. London, 1774, p. 445.

2. As ſound paſſes through the air with a velocity of 1142 feet in a ſecond, and the ſwifteſt wind does not paſs with a velocity of 90 feet in the ſame time; it might naturally be inferred that the velocity of ſound with or againſt the wind ought to be ſomewhat affected, but its intenſity very little. The contrary, however, is the caſe; from which, and other circumſtances, ſome philoſophers have been diſpoſed to conclude that the medium by which ſound is tranſmitted is not air, but another fluid of greater ſubtilty.

3. Sounds ſeem more intenſe, and are heard to a greater diſtance, by night than by day. In a ſtill night, the voices of the workmen at the diſtillery at Batterſea may be heard at Weſtminſter Bridge, over an interval of about three miles; the watch-word at Portſmouth may be heard at Ride in the Iſle of Wight, the diſtance of which is between four and five miles; and numerous inſtances are related of the propagation of weak ſounds to much greater diſtances, without mentioning thoſe of which the intenſity is greater. It is a practical queſtion of ſome importance to aſcertain whether this difference may ariſe from the different ſtate of the air, the greater acuteneſs of the organ, or the abſence of the ordinary noiſes produced in the day. By attentive liſtening to the vibrations of a clock in the night, and remarking the difference between the time when no other noiſe was heard, and when a coach paſſed along, it has appeared clear to me that this difference ariſes from the greater or leſs ſtillneſs only, and that no voluntary effort or attention can render the near ſound much more audible, while another noiſe acts upon the organ. In this ſituation the ear is nearly in the ſtate of the eye, which cannot perceive the ſtars in the day time, nor an object behind a candle.

4. It would be impoſſible to uſe optical inſtruments in the day time, if the light of other objects beſides thoſe to which the attention is directed were ſuffered to mix itſelf with the rays admitted into the field of view. In the ſame manner it ſeems that we have little to hope with regard to the improvement of acouſtic inſtruments for day uſe, if the firſt requiſite, namely, the excluſion of unneceſſary ſound, cannot be obtained.

5. Numerous experiments have ſhewn that ſound can be reflected, and that the impreſſion on the ear is greater or leſs, according to the diſpoſition of the reflecting bodies. Optical inſtruments are diſpoſed in tubes of ſuch a length, that the rays of light which ariſe from a ſmall portion of the viſible hemiſphere can alone reach the organ of perception. All the others ſtrike the ſurface of the tube, and after one or more reflections are almoſt totally abſorbed or loſt. It remains to be aſcertained from reaſoning and experiment, how far the ſame effect may be produced with regard to ſound. With a cylindrical wooden pipe, three inches in diameter and eight feet in length, at the diſtance of two miles from London, I liſtened to the noiſes which came from that capital. I think I did not deceive myſelf by any prepoſſeſſion, when I diſtinctly heard the noiſe and agitation of wheels on the pavement much more ſtrongly than any other kind of ſound. Nearer ſounds, not in the direction of the tube, were leſs perceived; and ſuch as were loud aſſumed a muſical tone; moſt probably from the reiterated reflections under the ſeveral angles of its reception. I conſider this experiment as of little other value than as ſerving to convey an hint, that a tube lined with cloth might defend an acouſtic inſtrument from ſounds out of the line of its direction, while the inſtrument itſelf might magnify and render diſtinct the ſound required to be heard.

6. M. Perrole conjectures, at the end of the firſt part of his memoir, that ſonorous

bodies do not vibrate during the propagation of ſound, becauſe the water in his curious experiment of the watch was not ſeen to move. He has overlooked the very great number and minuteneſs of the vibrations required to produce ſound. They cannot be viſible but in caſes of extreme ſimplicity and intenſity. The ſtring of a muſical inſtrument emits ſound long after its vibrations have ceaſed to be viſible. He likewiſe, at the end of his ſecond part, objects to the theory of Maupertuis, which ſuppoſes that ſonorous and reſonant bodies ſubdivide themſelves ſo as to vibrate differently in different parts. But the body in queſtion may be conſidered under different points of view. 1. As the medium of ſound, it may conduct or tranſmit every ſound indifferently. 2. If its ſimplicity of figure and texture be ſuch as to produce very few ſounds at a time, it will, upon the whole, emit a muſical tone. For I conſider a muſical tone as one or more ſimple ſounds in concord with each other; and a noiſe, as a greater number not poſſeſſing the ſame relation. Thus, the noiſe produced by preſſing down an indiſcriminate number of contiguous keys in the organ, is ſo far from being muſical, that it ſeems aſtoniſhing that the aggregate of pipes ſo melodious ſhould produce a ſound ſo harſh. 3. Or the body in queſtion may aſſiſt or impede the ſound of another ſimple body, ſuch as a ſtring with which it is in contact. When it aſſiſts that ſound, it does not ſeem improbable that the effect pointed out by Maupertuis may really take place. For the diviſion of bodies, his ſuppoſition requires, is known to take place in the ſingle ſtring which gives the trumpet notes, and has on that account been called the trumpet marine; and alſo in bells, which not only give a ſet of diſtinct contemporaneous tones differing in acuteneſs, but to a certain extent are found to alter the ſyſtem according to the plan of percuſſion. And again, it will readily be imagined by thoſe who take ſo much pains in experiments for fixing the ſound poſt of the violin, that the reſonance which is ſo much affected by this diſpoſition, is more probably of the whole inſtrument than merely of its parts as conductors of ſound.

7. The figure of the external ear, which is made up of a ſeries of concavities with ſtops or bridges interpoſed, is an object which, as far as I know, has never yet been explained or enquired into. In the cat and other animals this ſtructure appears to be very complicated. After the ſound or aërial undulation has been modified and conveyed into the ear through this apparatus, it is received upon the ſtretched membrane called the tympanum. This, as well as the other membranous parts of the internal ear, ſeems evidently adapted to vibrate by the action of ſound; and moſt probably after the manner that a muſical ſtring vibrates when another ſtring is made to ſound in its vicinity, and emits a tone in concord not too remote from uniſon. From this fact and the formation of the ear, a queſtion may be propoſed of ſome conſequence with regard to the conſtruction of acouſtic inſtruments: Whether the ſound produced by a remote body or that which is emitted by a correſpondent vibrating body, at a leſs diſtance from the organ, be the moſt perceptible? For example, if two ſtrings, A and B, be tuned in uniſon, and the ſtring A be ten feet from the ear, while the ſtring B is placed at the diſtance, for example, of one inch from the ſame organ;—whether the ſecondary ſound of B, when A is ſtruck, may not be more perceptible than the original ſound of A? It is unneceſſary to enter into conſiderations of the mechanical circumſtances under which, from theory, this or the contrary effect may take place. It may be ſufficient in this very remote view of the ſubject to remark, that in point of fact the influence of ſounds may extend further than the organ can perceive them. I was once in a room facing

the ſtreet, in London, when I perceived the window to be agitated with certain tremulous motions, attended with a conſiderable ſound. The reiterations were ſhort and diſtinct, three or four at a time; and then ceaſed till a ſecond and a third repetition of the ſame effect took place. This remarkable proceſs continued to increaſe in intenſity of ſound, and engaged my attention for about a quarter of an hour; after which the nearer approach of one of thoſe inſtruments called a tambourin, compoſed of parchment ſtretched on a hoop, and played upon by rubbing the extremity of the finger upon its ſurface, evinced that the agitation and ſound emitted by the window had been cauſed by the vibrations of that inſtrument. Hence it ſhould appear, that the judicious conſtruction of an inſtrument for receiving and magnifying ſounds would not only require a ſcientific arrangement of an external part to exclude foreign ſounds, and reflecting ſurfaces to modify and augment the direct undulations; but that the laſt effect ſhould be received on a tympanum capable of adjuſtment in its tenſion, and thence conveyed by a proper veſtibule to the organ of perception itſelf. If the ſcience of receiving and augmenting ſounds were once improved to the degree here ſketched out, there would probably be no difficulty in magnifying ſounds intended to be conveyed from one place to another.

VI.

Concerning the Steam-Engine as originally invented by the Marquis of WORCESTER, *and the Improvements ſince made in Steam-Engines without the Piſton or Lever. With a Deſcription of an Engine of this Kind conſtructed by Mr.* PETER KIER, *of Kentiſh Town.*

THE Marquis of Worceſter is the undoubted inventor of the ſteam-engine, which is deſcribed in his Century of Inventions. From the title, he appears to have conſtructed one before the year 1655. His words, No. 68, are as follow:

"An admirable and moſt forcible way to drive up water by fire, not by drawing or ſucking it upwards; for that muſt be, as the philoſopher calleth it, intra ſphæram activitatis, which is but at ſuch a diſtance. But this way hath no bounder, if the veſſels be ſtrong enough; for I have taken a piece of a whole cannon whereof the end was burſt, and filled it three quarters full of water, ſtopping and ſcrewing up the broken end, as alſo the touch-hole; and making a conſtant fire under it, within 24 hours it burſt and made a great crack; ſo that, having a way to make my veſſels ſo that they are ſtrengthened by the force within them, and the one to fill after the other, I have ſeen the water run like a conſtant fountain ſtream forty foot high. One veſſel of water rarefied by fire driveth up forty of cold water. And a man that tends the work is but to turn two cocks, that, one veſſel of water being conſumed, another begins to force and re-fill with cold water, and ſo ſucceſſively; the fire being tended and kept conſtant; which the ſelf-ſame perſon may likewiſely abundantly perform in the interim between the neceſſity of turning the ſaid cocks."

Plate XVII. Fig. 1, repreſents the ſteam-engine which was made by Captain Savery, and generally ſuppoſed to be the ſame as the Marquis of Worceſter's. A repreſents a boiler containing water, the ſteam of which may be tranſmited into either of the veſſels B and C by means of the cocks D E. The veſſels have a communication at bottom with the vertical

tical pipe F G. Theſe communications are made between two valves H I and K L opening upwards; and the effect is as follows: Imagine the upper part of the pipe to be filled with water for the purpoſe of rendering the valves tight, and let the cock D be turned. The ſteam from the boiler, being lighter than the air included in the veſſel B, expels that air, which, not being permitted to paſs through the valve H, iſſues upwards through I. In this ſituation, let the cock D be ſhut, and cold water be thrown upon B, or, which is much better, ſpouted into it; and the ſteam will become condenſed, and leave an empty ſpace into which the preſſure of the atmoſphere at F will force the water upwards through the valve H. While this is performing, the cock E is to be opened, and the air from the veſſel C expelled by the ſteam in the ſame manner. This is the regular ſeries of operations, and is repeated a ſecond time by ſhutting the cock E, cooling the veſſel C, and opening the cock D. By theſe means the water riſes from F into the veſſel C, while the ſteam from the boiler preſſes the water out of B, through the valve I, to G. The whole proceſs therefore, ſimplified by attending to one of the veſſels only, is found to conſiſt in expelling the air by ſteam; forming a vacuum by condenſation into which the water riſes, and then forcing that water upwards by new ſteam, which, being condenſed as before, is replaced by new water; and ſo forth.

To what practical extent the experiments of the Marquis of Worceſter were carried, does not appear; but it was near the end of the ſeventeenth century, when Captain Savery propoſed this engine as an invention of his own, for raiſing water and draining mines. Prony, in his Architecture Hydraulique, i. 564, ſays, he publiſhed his *Miner's Friend* about 1699. Deſaguliers, in his Courſe of Experimental Philoſophy, 3d edition, ii. 465, does not ſcruple to affirm, that Savery had this invention from the Marquis of Worceſter's book; but has not, it muſt be confeſſed, given to his aſſertion all the proof it might require. He ſays, that Savery bought up all the copies of the Century of Inventions which he could procure, and burned them; and that he uſed to relate a ſtory as the firſt hint of his invention: that having drank a flaſk of Florence at a tavern, and thrown the empty flaſk upon the fire, he called for a baſon of water to waſh his hands; and perceiving the little wine left in the flaſk had filled it with ſteam, he took it by the neck and plunged its mouth in the water, which was immediately driven up by the preſſure of the air. On this incident, Dr. D. remarks, that it could not have ſo happened; becauſe when he himſelf had repeated the experiment by boiling half a glaſs of wine in a flaſk, and, putting on a thick glove to defend his hand, had plunged its mouth beneath the ſurface of water, the preſſure of the atmoſphere was ſo ſtrong as to beat the flaſk out of his hand againſt the cieling—a circumſtance with which he does not doubt but that the Captain would have embelliſhed his tale if he had made the experiment.

It appears to me, nevertheleſs, that ſomewhat more than theſe incidents ought to be required to eſtabliſh the charge of deception againſt Savery, who, in the courſe of probabilities, might have invented the ſteam-engine half a century after the Marquis of Worceſter, whoſe deſcription, clear as it now ſeems to us who know the engine, was not perhaps enough ſo as to attract much attention, in company with one hundred other enigmatical propoſals. Captain Savery being a patentee, and having probably a conſiderable intereſt depending, might be as ſtrongly urged to ſuppreſs the Marquis of Worceſter's book on that account, as for the reaſon aſſerted by Deſaguliers. And with regard to the experiment of the flaſk, it is hardly allowable for a philoſopher to conclude that the experiments of another are falſe or impoſſible, becauſe they do not agree with his own. I ſuppoſe Deſaguliers made his experiments with more wine and more heat than Savery; and if I were diſpoſed to reaſon againſt the relation of a poſitive fact, I ſhould be inclined to doubt the projection of the flaſk againſt the ceiling by an exertion of atmoſpherical preſſure, which muſt have been as ſtrong on the outer as on the inner ſurface of the veſſel. And in this doubt I ſhould be ſtill more ſettled, from having myſelf very often repeated the experiment without any ſuch reſult. But the integrity of Deſaguliers forbids any ſuch inſinuation. Under certain circumſtances the flaſk might have been beaten out of his hand; but it does not follow that thoſe circumſtances were preſent in Captain Savery's experiment.

The Marquis of Worceſter was the firſt inventor of the ſteam-engine. Savery was either the ſecond inventor, or he had the ingenuity to diſcern a valuable invention in the midſt of looſe hints, and to give it organization and effect.

We do not know how the Marquis of Worceſter condenſed his ſteam, or, indeed, whether he condenſed it at all; for it certainly does not require condenſation unleſs for what is called ſucking. It is ſaid, that the condenſation in the firſt engines was effected by water on the outſide of the veſſel; but in Savery's engines it was performed within the veſſel. Several of his engines were made with only one ſteam-veſſel, and Deſaguliers found by experiment, that this kind is conſiderably better than ſuch as have two; becauſe the action of the ſteam is rendered more ſudden, and the chemical condenſation during the proceſs of forcing is leſs. This excellent philoſopher and practical engineer made ſeveral other improvements in this engine and its parts, which may be ſeen in his work laſt quoted: but the ſubſequent invention of the ſteam-engine with a piſton and lever, and the improvements which have been made therein, ſeem to have greatly retarded the progreſs of the original ſimple machine.

Dr. Papin, well known for his invention of the digeſtor, was buſied in experiments on ſteam. Prony, in his Architecture Hydraulique, i. 566, mentions a work of his, printed at Caſſel in 1707, under the title of *Nouvelle Maniere d'élever l'Eau par la Force de Feu*, in which a ſteam-engine is deſcribed which differs from that of Savery, but may equally accord with the deſcription of the Marquis of Worceſter. From the engraving, fig. 268, in his ſecond volume, it appears to have conſiſted of a ſpheroidal boiler, a cylindrical ſteam-veſſel into which was fitted a float or piſton, and an air-veſſel which received the water from each ſtroke previous to its being forced by re-action to a greater height. The water flowed through a pipe with a valve opening downwards, whence it paſſed beneath the piſton which floated upon it. The deſcent of the piſton was effected by the ſteam, and its aſcent by the action of the water from the original ſtock; at which period the ſteam was ſuffered to eſcape into the air by means of a cock, and the communication between the boiler was ſhut off.

In theſe engines it may readily be apprehended that the action of the direct ſteam on any definite ſurface, ſuch, for example, as a ſquare inch, will be accurately equal to the re-action of the water which is forced up; and conſequently, that Savery's engine will require ſteam more elaſtic than the air of the atmoſphere, in every caſe except that wherein the water is raiſed by ſuction, and afterwards ſuffered to flow out of the bottom of the veſſel into a channel or ciſtern. But if this action of ſteam be, by the intervention of ſome mechaniſm, tranſmitted to the place where it is intended it ſhall operate, it will be poſſible to regulate the proportions in any deſired manner. It was very early in the preſent century that Newcomen and Cawley began their trials of improvement of the ſteam-engine with a cylindrical ſteam-veſſel and piſton depreſſed by the weight of the atmoſphere, by which means they obtained a ſafe application of power for raiſing water to heights far beyond any in which the old engine could be truſted, on account of the extreme ſtrength required in the boiler and ſteam apparatus. This event is probably the cauſe why ſo little has ſince been done to remedy that leading imperfection, or to adapt the original machine to work itſelf without an attendant. It is certain, however, that the weight of the apparatus, and frictions of the parts, in all ſteam engines with a piſton and lever, have prevented their being extenſively uſeful in ſmall undertakings, ſuch as the blowing of ſmall furnace-bellows, the drawing of boats and carriages, with numberleſs other operations where forces not exceeding one, two, or three horſes are wanted. In theſe operations it is highly probable that the engine of Savery would be very uſeful, if it were made to work without an immediate attendant; for Deſaguliers found the advantage greatly in favour of Savery's engine,* on a ſmall ſcale, compared with Newcomen's lever-engine. He had an engine of the former conſtruction in his garden, which raiſed ten tons of water an hour, about thirty-eight feet high; and a friend of his erected a working model of the lever-engine on the ſame ſpot. The boiler of this laſt was exactly of the ſame ſize as that of Deſaguliers, and his cylinder was ſix inches bore and about two feet in length. It raiſed four tons per hour into the ſame ciſtern. It coſt three hundred pounds; but the engine on Savery's conſtruction, having all copper pipes, coſt but eighty pounds.

It muſt not be overlooked, however, that this account affords no ſtatement of the quantity of fuel conſumed under each boiler. It might perhaps have been the caſe, that Savery's boiler could have fed a larger cylinder. But at all events there is no doubt of the fact, that the beſt lever-engines cannot be advantageouſly uſed when ſmaller than a determinate ſize; and that, on account of the charge of attendance to open and ſhut the cocks, no trials have been made to ſhew upon how ſmall a ſcale Savery's engine might be rendered uſeful.

An engine upon Savery's principle, with various judicious improvements, was erected four years ago by Mr. Kier, at his manufactory of coach axle-trees near Pancras, where it has almoſt conſtantly been worked without repair. It is perhaps on too large a ſcale to afford much information reſpecting this queſtion; but is certainly of conſiderable value, not only becauſe it works without an attendant, and regulates its own motions, but alſo becauſe, as might naturally be expected, the wear and tear is altogether inconſiderable. He has permitted me to give an account of this engine.

The ſketch Fig. 2. Plate XVII. repreſents this engine, without extreme pretenſions to accuracy, but upon a ſcale of a quarter of an inch to a foot. R repreſents an oval boiler ſeven feet long, five inches wide, and five deep. The proprietor conſiders it as being of dimenſions ſufficient to work a larger engine; a circumſtance which muſt, in a certain degree, diminiſh the effects of the preſent. It feeds itſelf in the uſual manner, with water conveyed through a pipe at the end of which is a valve. This valve does not open until the fall of the water within the boiler has ſuffered a float to ſubſide, which by its actual weight aſſiſts to draw it open, but by its tendency upwards, as the water in the boiler riſes, ſerves effectually to cloſe it. The boiler, therefore, remains conſtantly at or near the ſame degree of fullneſs. The ſteam is conveyed by a pipe TAV to a box B, through which, by the opening and ſhutting of a valve, it can be conveyed to the working chamber E. The axis C ſerves as a key to open and ſhut the valve. NO is a ciſtern of water, from which the ſupply is made through the vertical pipe in which the valve Q is placed; and GG is another ciſtern into which the water is delivered through the pipe F, which is provided with a valve H opening outwards. IM repreſents an overſhot wheel eighteen feet in diameter, moving on the axis KL, and communicating its motion to the lathes and other rotatory engines of the manufactory. The water in both the ciſterns becomes warmer than the hand after working a ſhort time; for which reaſon the injection-water is forced up by a pump from a well ſupplied by the ſmall ſtream on which theſe works are eſtabliſhed. A leaden pipe paſſes from this forcing pump to the upper or conical part of the chamber E, for the purpoſe of injecting cold water at the proper time. Neither of theſe could be repreſented with convenience in the preſent ſection.

The manner in which the ſteam and cold water are alternately admitted into the chamber E, remains to be explained. Upon the extremity K of the axis of the overſhot wheel there is fixed a ſolid wooden wheel about four feet in diameter, repreſented in fig. 3, as ſeen in front, and alſo in profile, where the ſmall letters denote the ſame things as are marked by the large letters of the alphabet in the front view. ABCD are four cleats, all or any number of which may be fixed on the wheel at a time. Each cleat has its correſpondent block EFGH on the oppoſite ſurface of the wheel. The uſe of theſe is to work the engine. Suppoſe the wheel IM or any part of the revolving apparatus be drawn round by hand, one of the cleats meets in its rotation with a lever which opens the ſteam-valve by a bar of communication reaching to the handle of the axis C, fig. 2. The ſteam conſequently paſſes into the chamber E, and the ſteam-valve ſhuts again as ſoon as the cleat has paſſed. Speedily after this the correſpondent block on the other ſide of the wheel meets another lever, which is ſimilarly attached to the handle of the forcing pump, and therefore throws a jet of cold water into the chamber, and condenſes the ſteam. The preſſure of the atmoſphere then forces the water from the ciſtern NO through the valve Q towards the chamber E. When the engine has been long out of work, I ſuppoſe two or three ſtrokes may be neceſſary to raiſe the water to the top of the chamber. As ſoon as this is the caſe, the injection of the ſteam ſuffers the whole body of water above the valve H to overcome the preſſure of the atmoſphere and ruſh out. The water which is raiſed is ſuffered to flow upon the overſhot wheel through a ſluice, and by that means keeps the work in motion, and repleniſhes the lower ciſtern.

Hence we ſee that in effect this engine is the ſame as figure 1. excepting that it is not applied for the immediate purpoſe of raiſing water, but gives motion to other apparatus. It is therefore unneceſſary to inſiſt more largely upon the mere operation; but the peculiarities of Mr. Kier's engine are deſerving of notice. In the firſt place it may be obſerved, that he uſes no reſervoir for his injection water, but drives the requiſite quantity up at each ſtroke. The advantage of this is, that the pump ſuſtains the action of a very ſhort column of water, though it apparently forces to the height of about 26 feet. For the column in the pipe and chamber PE by its re-action takes off the effect of an equal length of column which would elſe have acted againſt the forcer, and thus renders the injection more eaſy and quick. In the ſecond place it may be obſerved, that he has proceeded directly oppoſite to the firſt remark of the Marquis of Worceſter, who rejects drawing or ſucking upwards. For this engine does not force at all. The water merely falls out of the chamber, and conſequently never requires ſteam ſtronger than the atmoſphere. From the effect of this engine under circumſtances of ſuch advantage, it may fairly be concluded that the action of ſteam againſt water in forcing can never be beneficial except at a place where fuel could be had extremely cheap. Thirdly, it was found at the firſt conſtruction of this engine, that the conſumption of ſteam by contact with the water was ſo great that it could not be worked with the ſmalleſt advantage. This defect was remedied in the preſent engine, as well as in another at Norwich, by fixing a ſmall air-valve in the ſteam-box, which was ſtruck for an inſtant immediately before the admiſſion of the ſteam. It may be preſumed that the air occupied a ſpace above the water, and prevented their coming together. Mr. Kier, however, is diſpoſed to think that the effect does not take place in this manner, but by ſome mixture and ſudden dilatation of the two fluids; becauſe he imagines the miſchief from the wet cylinder would be the ſame upon deſcending ſteam. Much, however, may be ſaid in defence of the oppoſite opinion. The air-valve is not at preſent uſed, becauſe the engine does very well without it; which is ſuppoſed to ariſe from its being leſs air-tight than it was at firſt. Fourthly, the motion of the overſhot wheel is regulated by an apparatus called a governor, invented, as I think, by Mr. Watt, and repreſented in figure 4. The bar HI revolves by communication with the engine, and carries round the balls A and B, which move on a fixed joint C. When the rotation is very quick, the balls fly out, and draw down the points DE, and conſequently F, which is ſo conſtructed as to ſlide upon the bar or axis HI. A lever, FG, connected with the ſluice of the upper ciſtern GG, fig. 2. is therefore made to fall or riſe accordingly as the velocity is greater or leſs. By this diſpoſition, when the wheel moves very ſpeedily, from lightneſs of work or any other cauſe, the quantity of water thrown down from the upper ciſtern is immediately diminiſhed; and, contrarywiſe, the quantity of water is rendered greater when the ſlowneſs of the movement ſhews that it is wanted. When I ſaw the engine at work, it had but one cleat and block upon its wheel, and two men were at work at ſtrong lathes, roughing out certain pieces of iron about two inches in diameter. Whether it were the unſteadineſs of this work, or the want of ſufficient celerity of communication, I know not: but the variations in this governor were upon the whole more conſiderable than I have remarked in a lever-engine with a fly and this apparatus alſo. Fifthly, as the injection-water makes a conſtant addition to the ſum of the water contained in the ciſterns NO and GG, the ſurplus is allowed to overflow. If it were to overflow the lower ciſtern, it would warm the water in the injection well; for which reaſon a contrivance becomes neceſſary to prevent that effect. Mr. Kier's contrivance is this:—The water of the upper ciſtern, when at a certain elevation, overflows into a communication which conveys the water to the lower; for which reaſon its contents can never riſe above a certain level. There is alſo a pipe from the bottom of the upper ciſtern, which is recurved upwards to communicate with a gutter a little lower than the place of overflowing juſt mentioned. It is clear, therefore, that the upper ciſtern would always overflow at this pipe ſooner than at the place of communication with the lower ciſtern, if there were not a valve within the upper ciſtern, over the mouth of the recurved pipe. Now it is ſo managed by means of a float in the lower ciſtern, that this valve in the upper ſhall be opened whenever the water below riſes to a certain level. And conſequently, as ſoon as ever the lower ciſtern has received this portion, it can receive no more, becauſe all the ſubſequent addition of water will paſs away by the recurved pipe.

This engine conſumes ſix buſhels of good coals in twelve hours' work when in its beſt ſtate, or ſeven buſhels when at the worſt. Under theſe circumſtances it gives ten ſtrokes per minute, each throwing out ſeven cubic feet of water, at an aperture twenty feet above the water beneath. This quantity, namely 70 cubic feet per minute, will weigh 4345 pounds, which being doubled, to reduce it to Deſaguliers's ſtandard height of ten feet, will amount to 8690. And this divided by 580, the number of pounds in a hogſhead, will give a quotient of 15, repreſenting ſo many men, according to the eſtimate of that author, which he reckons equivalent to three horſes. This reſult is not more than half what is performed by the improved engines with a piſton of ſuch a ſize as to be equal to 20 or 25 horſes. But how far thoſe engines would prove effective upon the ſmall ſcale of three horſes, or the ſtill ſmaller ſcale of one or even leſs, remains to be decided.

The engine here deſcribed has been at work four years, and from the ſimplicity of its conſtruction has yet exhibited no proofs of wear. Mr. Kier thinks it a profitable engine to himſelf, and that it would be ſerviceable for raiſing water where coals are cheap. A contrivance he made in the year 1783 might, perhaps, with proper modifications, be uſed to obviate the neceſſity of forcing where the heights are conſiderable. It is liable, however, to ſome ſtrong objections, and certainly requires to be maturely weighed with regard to dimenſions and effects before it ſhould be attempted to be put in practice.

Fig. 5. repreſents the contrivance for forcing, as I find it in the original drawing; and I thought it unneceſſary to make any alteration to adapt it to what is called ſucking, which might, if required, be eaſily done by any perſon ſlightly acquainted with the ſubject. A is a boiler, and B a ſteam-veſſel. This laſt communicates with the veſſels M L K T, each of which, except the lower one, conſiſts of two veſſels; an interior veſſel, cloſed on all ſides excepting where it communicates with B; where pipes P, O, N, I, enter the upper part of each; and alſo where there is a valve at the bottom, opening upwards: the pipes I N O likewiſe communicate with the three exterior veſſels K, L, M. If ſteam be let paſs from A to B, it is inferred that the air will be driven from B, and preſs upon the water in the lower veſſel T, which will be driven into the exterior veſſel of K, but not into the interior veſſel, becauſe the preſſure of the air through the tube H is more than ſufficient to keep the valve of the apparatus K ſhut. The next ſtep in the operation conſiſts in cloſing the cock C, and opening D, out of which the re-action of the water forcing itſelf into T to its natural level, and into the interior veſſel of K on the ſame account, will drive a portion of ſteam. D is then to be cloſed, and C opened; in conſequence of which, the contents of T will be forced up to K, as before; and the interior veſſel K will evacuate its contents into the exterior of L. The ſteam being ſhut off at C, and the cock D being opened as before; the veſſel T, the interior veſſel K, and the interior veſſel L will fill as before, and a larger portion of ſteam will iſſue from D. A third repetition of the proceſs will drive the contents of theſe three interior veſſels a ſtep higher; and a fourth repetition will cauſe the contents of the upper interior veſſel M to flow out at P; after which every alternation of the work with the cocks C, D will throw out the ſame quantity from P.

I ſhall only remark on this contrivance, that the veſſel B muſt neceſſarily contain a quantity of air capable of occupying the whole interior ſpace contained in the cloſed veſſels T K L M, with an allowance for the loſs of bulk in condenſation under the preſſure of a column of water equal to one of the lifts, and that the quantity of ſteam to be diſcharged at each ſtroke muſt occupy a ſpace equal to that of all the water moved at each ſtroke, and muſt in all caſes be conſiderably ſtronger than the atmoſphere.

VII.

On the Mechaniſm by which the Mariner's Compaſs is ſuſpended.

If a bar of iron be rendered magnetical, by rubbing it on a natural magnet, or by any of the well-known proceſſes for that purpoſe; it acquires the property of diſpoſing itſelf nearly in a north and ſouth line, whenever it is ſuſpended ſo much at liberty that the energy of this power is ſufficient to overcome the friction or other impediments to its motion. The mariner's compaſs is an apparatus in which a bar of this kind, called the needle, is ſupported, for the highly uſeful purpoſe of determining the poſition of the meridian at ſea, and conſequently of enabling ſhips to ſteer their courſe by day or night, without obſervation of the ſtars, or any other external objects, as was neceſſary before the diſcovery of this inſtrument. In a well-conſtructed mariner's compaſs the needle is defended from the impulſe of the air, and is little ſubject to be diſturbed by the external motions or agitation of a ſhip at ſea. As this diſturbance is, however, the chief impediment to the convenient uſe of the compaſs in a boat, where the motions are ſudden and ſhort, or in a ſhip, when the waves are very turbulent; and as the artiſts in this branch have endeavoured to perſuade the world that certain pieces of mechaniſm were much ſuperior in their uſe to others differently diſpoſed;—I thought it might be of ſome utility to explain the ſimple principles of a good ſuſpenſion.

When the needle of the compaſs diſpoſes itſelf in the magnetical meridian, there is a certain line within the piece of ſteel, which joins its two poles, and may be conſidered effectively as the needle itſelf. But as this line is not viſible, the admeaſurements of poſition muſt be made with regard to ſome marks on the extremity of the needle; which marks will be truly placed when the needle is found to occupy the ſame poſition with reſpect to a fixed point, upon being reverſed, ſo that the lower ſide ſhall become the upper. If the magnetical power had been found on experience to occupy the ſame parts of the needle with proportional intenſity, during its decay, to that it poſſeſſed immediately after the touch, theſe marks, once made, would continue to ſhew the true magnetical points as long as the needle poſſeſſed any directive force. But it is well known that ſoft ſteel loſes its magnetiſm ſooner than hard; and conſequently it may be inferred that, unleſs both ſides of a needle were equally hard, the magnetic power would deviate in proceſs of time towards the harder ſide. Theſe conſiderations lead to an obvious method of diminiſhing ſuch growing error. It conſiſts in making the needle flat and thin, and ſuſpending it with its edge, and not its flat ſide, uppermoſt, as is more commonly done.

The needle is uſually ſupported on a ſteel point, which occupies the axis of a cylindrical box called the compaſs-box. For this purpoſe there is formed in the needle itſelf a cap or hollow conical centre of braſs, ſteel, or hard ſtone, which is applied over the point. The tendency of the needle to be diſturbed by agitation will greatly depend upon the poſition of the vertex of the conical cavity. It is neceſſary that it ſhould be above the centre of gravity; but this diſtance muſt be ſo ſmall as that the libration of the needle when one end is depreſſed ſhall be very ſlow, and yet ſpeedy enough to recover the horizontal poſition in a reaſonably ſhort time. In fact, the whole of the ſteadineſs of the compaſs and its box appears to depend on this principle of ſlow vibration. For, if a needle perform its vertical vibration in eight ſeconds, it will be very little diſturbed by an alternate action that laſts but a ſecond or two.

The greater number of workmen have imagined that the agitation of the compaſs is communicated by friction at the points or edges of ſuſpenſion, and have accordingly exerted their ingenuity to diminiſh this friction, by contrivances ſimilar to that of a conical cap balanced on a point, and itſelf affording another point to ſupport the needle. But it is very readily proved by experiment, as well as argument, that the greateſt diſturbance of the needle is produced by the quantity of horizontal progreſſive motion, and not by the mere inclination or angular motion. A compaſs-needle ſupported on a ſimple point will ſuffer very little agitation from any angular motion or moderate deviation from perpendicularity in the pin; but it will inſtantly begin to vibrate if moved horizontally. Thus the common experiment of tilting the compaſs-box in all poſitions while its centre remains immoveable is fallacious, and there are very few compaſſes indeed which will bear to be ſlided backwards and forwards upon a table.

It appears therefore that the ſteadineſs of a needle which vibrates ſlowly, is the conſequence not only of the length of time it allows for alternate actions to operate, and deſtroy each other; but alſo of the difficulty with which it yields to ſuch impreſſions. If the centres of ſuſpenſion and of gravity in the needle were coincident, no angular motion would be produced by any action of the pin, excepting by the effects of friction; and the angular motion produced in other caſes will be leſs, the ſhorter the diſtance between theſe two centres, or the lever by which it is propagated.

The ſimple ſuſpenſion of the needle on a point has been applied to the compaſs-box, for which it is little ſuited, not only becauſe of the wear upon ſo ſmall a ſurface, but alſo becauſe it admits the box to traverſe horizontally; an effect which is inconvenient, and cannot be remedied by any means not calculated in ſome reſpect to increaſe the effects of agitation. The method moſt generally received, and in fact the beſt adapted to this inſtrument, are the gimbals.

This well-known contrivance conſiſts of an hoop ſupported upon two pins diametrically oppoſite each other, and iſſuing from the external ſurface of the ring in ſuch a direction that both lie in the ſame diametrical line. When the hoop is ſuſpended on theſe pins, it is at liberty to turn freely round the diameter of which they conſtitute the prolongation. The notches or holes of ſupport are diſpoſed horizontally. The compaſs-box itſelf is placed in a ſimilar ring with two projecting pivots; and theſe pivots are inſerted in holes made in the former ring at an equal diſtance from each of its pivots. If, therefore, we ſuppoſe the whole to be left at liberty, the compaſs-box may vibrate upon the diametral line of the outer ring, and alſo upon a line formed by its own pivots, at right angles to that diametral line. The conſequence of this arrangement is, that the centre of gravity of the compaſs-box will diſpoſe itſelf immediately beneath the interſection of both lines on which it is at liberty to move:—that is to ſay, if the weight of the box or its parts be properly diſpoſed, the compaſs will aſſume a poſition in which its upper ſurface ſhall be horizontal.

The ſame principles which were applied to the ſingle centre of the magnetic needle will alſo apply to the axis of the gimbals. If the centre of gravity of the compaſs-box be ſo placed with reſpect to either axis as that its vibrations ſhall be quick, every horizontal action will greatly diſturb it, and it will not ſpeedily ſettle. The moſt favourable poſition of the pivots or edges of ſupport in the gimbals will be when they all lie in the ſame plane, and the centre of gravity of the compaſs-box is very little below that plane.

The practical application of theſe inferences appears to be, that, without pretending, as has been done, that any peculiar ſecret or great diſcovery is required to give ſtability to this uſeful inſtrument, nothing more is required than good workmanſhip, and a proper adjuſtment of the weight with regard to the centres or axes of ſuſpenſion. The needle ought to be adjuſted either by means of its cap, or by proper filing away, or elſe by additional pieces to the card, ſo that it ſhall vibrate very little, and that ſlowly, when placed upon a point and moved horizontally, whether in the direction of the needle or at right angles to that direction. The card is then ready for the compaſs-box. The box itſelf muſt be adjuſted with the card in its place, ſo that it ſhall exhibit the ſame ſteadineſs when moved in the line of direction of the outer pivots. And laſtly, the ſame diſpoſition muſt be made with regard to the motion in the direction of the inner pivots. It is ſcarcely neceſſary to add, that the means of this adjuſtment conſiſt in ſhifting the pivots themſelves, or, which is much better, in altering the diſpoſition of weight about the compaſs-box. An external ring of metal, encircling the box, and raiſed or lowered until the proper place for fixing it is found, would perhaps afford the moſt convenient method.

Upon the whole, the reader will perceive, that the leading aim of the preſent paper is to enforce the truth, that the compaſs is very little diſturbed, at ſea or elſewhere, by tilting the box on one ſide, but very much by ſudden horizontal changes of place; and, conſequently, that a ſcientific proviſion againſt the latter is the chief requiſite in a well-made inſtrument of this kind. And again, that nothing is more eaſy than to aſcertain whether a compaſs poſſeſſes ſtability; ſince nothing more is requiſite than to ſlide it upon a table in the ſeveral directions above-mentioned; and remark how far it is diſturbed. The good workmanſhip of the cap and pin of the needle may be aſcertained by inſpection with a magnifier, and alſo by drawing the card with a ſmall key or other piece of iron, a very little quantity, for example a quarter of a degree, out of its ſtation or poſition, and remarking whether it returns accurately to its original ſtation.

Before I quit this ſubject, I muſt take notice, that, as the ſuſpenſion on a point has been applied to the compaſs-box, ſo, on the other hand, the gimbals have been applied to the needle. This was done by the late Dr. John Lorimer, who diſpoſed a dipping needle on its own axis between the cheeks of a frame parallel to the diameter of a circle of braſs graduated to ſhew the angle of dip. This circle or meridian had pivots at its zenith and nadir, which moved in holes diametrically perforated in another circle. The needle was by this means not only permitted to diſpoſe itſelf in a line forming the angle of dip with the horizon, but was enabled to carry the meridional circle into the line of the magnetic meridian. It does not appear from the Doctor's paper and drawing in the 65th volume of the Philoſophical Tranſactions, that he had added the ſimple expedient of an horizontal gimbal-hoop to the croſs circle which ſupported the meridional pivots. Such a circle, divided, inſtead of the fixed horizon in his figure, would have rendered it a complete inſtrument for general purpoſes. It is probable however, that for ordinary naval uſe the ſimpler compaſſes of the preſent conſtruction would be preferable.

VIII.

On the Maintaining Power in Clocks and Watches.

IN a former paper on the compenſations for change of temperature in pendulums, a ſlight view was taken of the methods by which a train of wheels might be made to move either uniformly or by equal intervals of progreſſion, ſo as to afford a meaſure for the lapſe of time. The pendulum and the balance are at preſent the regulators which for juſt reaſons are preferred to every other contrivance. If we ſuppoſe a body to be ſuſpended at the extremity of a ſtring, its gravity, as is well known, will cauſe the ſtring to point towards the centre of the earth's gravitation at that part of the ſurface. If the body be then drawn aſide out of this perpendicular poſition, it is equally clear that it will be removed further from the earth's centre, towards which, when again ſet at liberty, it will fall through the arc of a circle, which is the only line the ſtring will permit it to deſcribe. At the inſtant the perpendicular poſition is thus regained, the body will not be at reſt, but will poſſeſs a portion of velocity in a direction at right angles to that of gravity. It will not, therefore, be in the power of gravitation to affect that velocity; and conſequently the body will proceed onwards on the oppoſite ſide of the perpendicular, in a ſecond circular arc, which elementary writers demonſtrate to be in theory perfectly ſimilar, and equal to the arc of deſcent:—that is to ſay, the action of gravity will more and more oppoſe the aſcending motion, as its direction becomes more remote from the horizontal line, and will at length not only deſtroy the motion of aſcent, but again generate a falling motion preciſely the ſame as the firſt. Such a pendulum would, therefore, if no other circumſtances preſented themſelves than have yet been ſtated, vibrate for ever; and its vibrations would be performed in times preciſely equal. But the truth is, that ſuch a body muſt in actual experiment give motion to the air through which it paſſes; and muſt alſo overcome a certain degree of rigidity to which the ſtring, or any other ſubſtitute in nature which might be propoſed, is ſubject. On theſe accounts, the quantity of motion in the pendulous body will be continually diminiſhed, and at length become inſenſible. It may alſo be eaſily apprehended, that a pendulum, though conſtructed with ſuch delicacy as to move for many hours before it comes to apparent reſt, could not be of much practical utility without ſome ready means to keep an account of its vibrations. For both reaſons, therefore, a pendulum without any other addition can ſcarcely be uſed as a time-keeper.

A train of wheels, urged by a weight or ſpring, is therefore attached to the pendulum for theſe two purpoſes, namely, to maintain its motion by a ſmall impulſe given at each vibration, or at moſt at each ſecond vibration, and to keep account of the number of times. The principle of the balance is, that its ſimilar vibrations are performed in equal times, like thoſe of the pendulum. The methods of adapting the train of wheels to one of theſe regulating organs will not therefore eſſentially differ from thoſe of connecting it with the other. In moſt of theſe contrivances, the pendulum or balance is, during ſome part of its motion, brought in the way of one of the teeth of the laſt wheel of the train, at the time when the whole ſyſtem of wheels are at liberty to move. The tooth, therefore, ſtrikes either the pendulum, or a part of the movement connected with it, and then ſlips off, or eſcapes from that obſtacle. The whole ſet of parts in a time-piece by which this alternate action is effected, is for that reaſon called the eſcapement.

[*To be continued.*]

MATHEMATICAL AND PHILOSOPHICAL CORRESPONDENCE.

ANSWER TO THE PHILOSOPHICAL QUESTION, p. 284.

To Mr. NICHOLSON.

SIR,

FROM the deſcription of the divergent rays ſeen in agitated water ſurrounding the ſhadow of the obſerver's head, which forms part of the queſtion in your ſixth number, I did not fail to look for the appearance on the firſt opportunity of ſunſhine which offered. I obſerved the fact as you deſcribe it, and found that the radiations appear without being modified in any reſpect by the diſtance of the obſerver from the ſurface of the water. This circumſtance indicated a probability, that the cauſe which I could not inveſtigate by direct reaſoning, might be developed by a nearer inſpection than could with convenience be made on the bank of a river. With this view I placed a veſſel of water, which was three feet deep, in an out-houſe, in ſuch a manner that the ſun ſhone upon its ſurface. When I placed myſelf ſo that the ſhadow of my head fell upon the water, I perceived no radiations whatever; but upon agitating the water with my hand, the water appeared full of the lines called ſun-beams, parallel to each other no doubt, but which, on account of the ſmaller optical diſtance between their remoter than between their nearer extremities, had the appearance of convergence towards the ſhadow. As it is perfectly intelligible from the well-known truth in perſpective, that all lines parallel to the line of ſight will vaniſh in the point of ſight; it remains only to be ſhewn how theſe diſtinct parallel beams are produced.

When the ſurface of the water is at reſt, and forms one plane, the ſun's light forms one uniform maſs in which no lines are diſtinguiſhable; but when the agitation takes place, a number of convexities and concavities are produced. Theſe ſcatter the rays in all directions, except at that ſmall portion where the contrary curvatures join. The ſame phyſical effect is produced as from ſo many ſmall planes; that is to ſay, the rays paſs ſtraight through each ſurface, and afford the parallel diſtinct beams of light. And though theſe ſmall planes are continually forming and diſappearing, yet there are always a certain number in exiſtence, and the effect of their tranſient individual variations is nothing more than to produce an undulation in the general ſyſtem of radiations.

And now, Sir, after having reſoved your queſtion, as I think, ſatisfactorily; I ſhall take the liberty to propoſe another in the ſame department of ſcience.

When the ſun ſhines, if two bodies, for example hands or fingers, be gradually moved, ſo that their ſhadows may meet; it is obſerved that the ſhadow of the body fartheſt from the ſun will ſwell out, and join the other ſhadow when the diſtance becomes diminiſhed to a certain quantity, while the ſhadow of the body neareſt the ſun undergoes no alteration. What is the nature of the inflection of light which occaſions this appearance?

I am, Sir, yours, &c.

A. Y.

⁂ The fact mentioned in the laſt paragraph of this letter has been ſtated and explained elſewhere, though I cannot now recollect where I have ſeen it. It does not depend upon inflection, but upon the different widths of the penumbræ. When the penumbræ firſt interfere, a faint lenticular ſhade is produced, which is nearer the ſhadow of the moſt remote body than that of the other; and as ſoon as the remoter body itſelf comes in contact with the bounding ray of the greater penumbra, it begins totally to intercept the ſun's light on the ſide next its own ſhadow. The time employed in paſſing from one extreme ray of the penumbra to the other will be leſs the nearer the bodies are to each other, becauſe the diſtance of thoſe rays is leſs the nearer the vertex. The ſhadow of the remoter body will conſequently run over the penumbra with greater ſpeed than that of its former progreſs; and if any part be convex, it will advance beyond the others.

To Mr. NICHOLSON.

SIR,

HAVING obſerved in the 4th Number of your Journal *, and likewiſe in the Annales de Chimie, an extract of Mr. Prevoſt's Memoir on the means of rendering viſible the emanations of odorant bodies; I take the liberty of communicating an obſervation which occurred to me a few years ago, while making experiments with different ſubſtances expoſed to the oxigenated muriatic acid-gas. That which afforded the curious phenomenon, to which I allude, was ſome highly-rectified animal oil. This matter, immediately on being expoſed to the gas (which was in a very dry ſtate), was ſeen to emit a copious ſteam, the particles of which riſing to the height of about four inches above the ſmall phial in which the oil was contained, were then obſerved gradually to deſcend, forming a very curious and pleaſing appearance.—Although ſtruck at the time with the ſingularity of this phenomenon, and endeavouring to account for the formation of the vapour by a ſuppoſed union of the hydrogen of the emanating matter with the ſuper-abundant oxigen of the gas; yet I omitted at the time, and have ſince neglected, to endeavour to verify my conjectures by further trials.—The circumſtance, however, revived in my recollection on reading your Journal; and conceiving it to be an experiment that affords a nearer approach to the abſolute *perceptibility* of an odorant vapour, than thoſe ſtated in the Memoir of Mr. Prevoſt, I impart it for your further inveſtigation, if thought worthy your attention, and am

Your obliged reader,

Nov. 24, 1797. W. HOWARD.

N. B. The oil was the only odorant ſubſtance tried: perhaps many others might afford a ſimilar or varied appearance.

⁂ The philoſophical conſideration of odorant bodies is ſomewhat obſcured by the old method of generaliſing, or referring the properties of bodies to ſome diſtinct principle or thing ſuppoſed capable of being ſeparated from the body itſelf. Thus the odors of bodies have been ſuppoſed to depend on a ſubſtance imagined in a looſe way to be common to them all and ſeparable from them. Hence the terms, principle of ſmell, ſpiritus rector; and even in the modern nomenclature we find *aroma*. There does not in effect ſeem to be any more reaſon to infer the exiſtence of a common principle of ſmell than of taſte. The ſmell of ammoniac is the action of that gas upon the organ of ſenſe; and this odorant inviſible matter is exhibited to the ſight when combined with an acid gas. But in the ſame manner as ammoniac emanates from water and leaves moſt part of that fluid behind, ſo will the volatile parts of bodies be moſt eminently productive of this action; and very few, if any, natural bodies will be found which riſe totally. The moſt ſtriking circumſtance in the effect is, that an act of ſuch power ſhould be attended with a loſs by exhalation which is ſcarcely to be appreciated by weight, or in any other method during a ſhort interval of time. But we know ſo little of nervous action, and of other phenomena of electricity, of galvaniſm, or even of heat, which ſtrongly affect the ſenſes but elude admeaſurement by gravitation, that the difficulty of weighing the effluvia of odorant bodies becomes leſs aſtoniſhing.

---

* Page 153. See alſo on this ſubject, p. 205. N.

QUESTION XI. *Anſwered by J. F—:—:—:—R.*

LET H b be the altitudes of the two ſignals or other objects, A their direct angular diſtance, and a their difference of azimuth, as ſtated in the queſtion. It is proved by the writers on ſpherics, that, in any ſpherical triangle, Coſ. any angle : radius : : radius × coſ. oppoſite ſide — rectangle coſines including ſides : rectangle ſines including ſides; which analogy, applied to the triangle formed by the zenith-diſtances and direct angular diſtance of the elevated objects, gives, Coſ. a : radius : : radius × coſ. A — ſine H × ſine b : coſ. H × coſ. b; ſo that coſ. A × radius^2 = coſ. a × coſ. H × coſ. b + ſine H × ſine b; or, putting radius = unity, Coſ. a = coſ. a × coſ. H × coſ. b + ſine H × ſine b. Q. E. D.

QUESTION XII. *Anſwered by Mr. WILLIAM CASTILAU, of Uffington, Salop.*

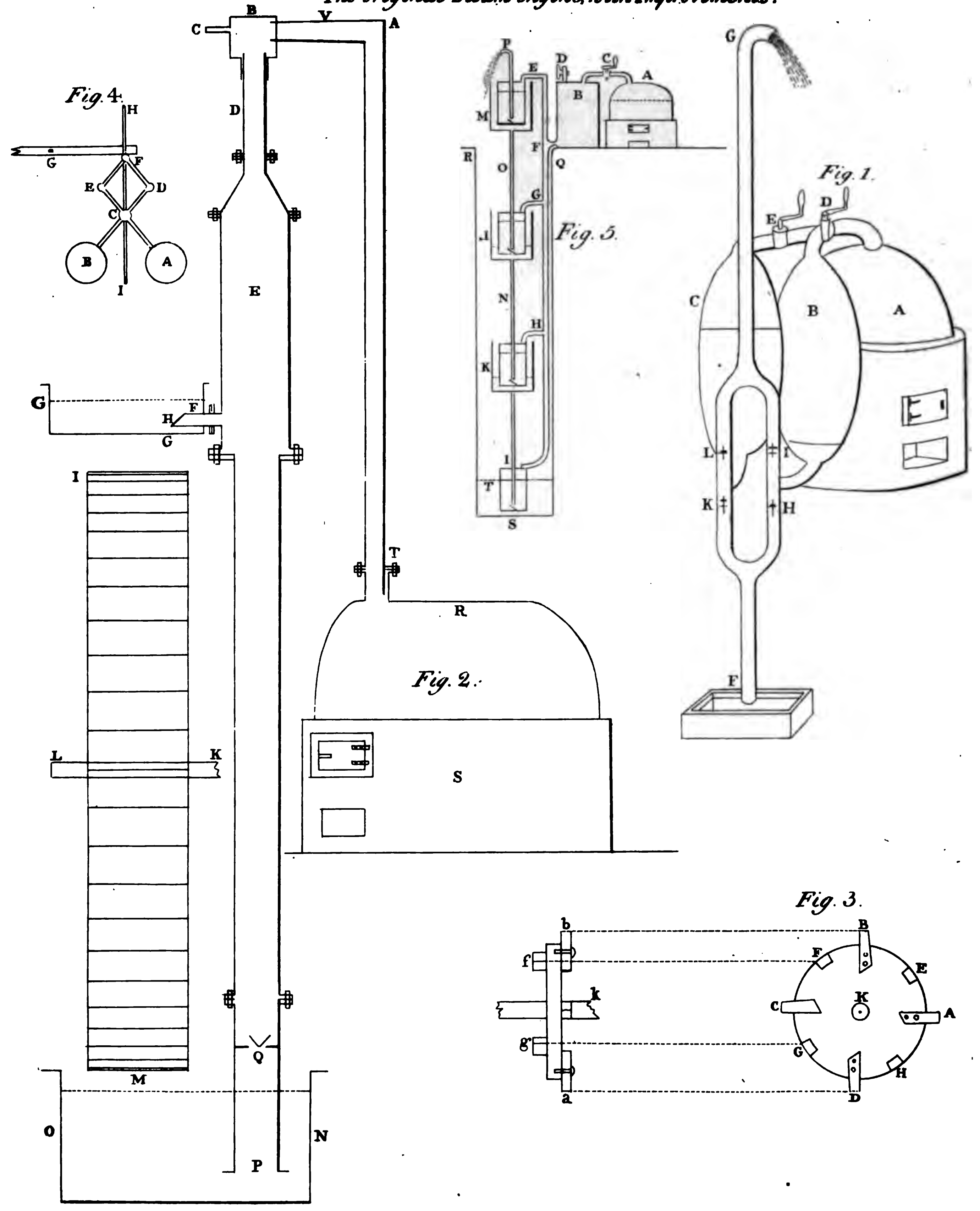
LET Z P $\odot$ repreſent the zenith, pole, and ſun reſpectively; x the coſ. of ∠ PZ $\odot$: then, per data and a well-known theorem in ſpherics, SZ $\odot$ × S PZ × coſ. ∠ $\odot$ ZP + coſ. Z $\odot$ × coſ. PZ = coſ. P $\odot$ (rad being 1); that is (putting d for ſine of the ſun's dec. 15° 9′ or .26135) $x \times x \times x + \overline{1 - xx} = d$: this equation ordered is $x^3 - x^2 = d - 1 = -.73865$, whence $x = -.66588$ = coſ. 131° 45′ ∴ 48° 15′ will be the azim. when equal to the lat. and alt. on the given day. The ∠ ZP $\odot$ alſo = $2^h\,3'\,54''$. Per Naut. Alm. the ſun's declination for noon, on the given day (at Greenwich), is 15° 18′ 28″, and its change for $24^{hrs} = 18'\,7''$ ∴ As $18'\,7'' : 24^h :: \overline{15^\circ\,18'\,28'' - 15^\circ\,9'} : 12^h\,32'\,27''$ the time from noon at Greenwich when the obſervation was made. The latitude of the place of obſervation, then, is 48° 15′ N. and its longitude $(\overline{12^h\,32'\,27''} - \overline{2^h\,3'\,54''})$ $10^{hrs}\,28'\,33''$ E. of Greenwich.

☞ The ſame was anſwered by *J. F—:—:—:—R.*

◆

Philos. Iournal Vol. 1 Plate XVII facing p.443

The original Steam engine; with Improvements.



THE NEW YORK
PUBLIC LIBRARY

ASTOR, LENOX AND
TILDEN FOUNDATIONS
R L

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JANUARY 1798.

ARTICLE I.

Experiments made with a View to aſcertain the Cauſe of Buildings, which have Metallic Conductors belonging to them, being ſtruck by Lightning. By Lieutenant-Colonel HALDANE.

IT is not neceſſary, in the preſent communication, to enquire what may be the nature of electricity; whether it exiſt in the form of one fluid only, and all its phenomena depend upon the preſence of a greater or leſs quantity than naturally belongs to all bodies; whether its effects may depend upon the influence of oppoſite polarities, and the operations of attractive and repellent powers in a ſingle fluid; or, laſtly, whether it exiſts in the form of two different, though always co-exiſting, fluids, produced by the union or the ſeparation of certain gaſes, which belong to electric and non-electric ſubſtances, according to their various elective attractions, and which may be brought into action by the attrition of thoſe ſubſtances, either in the form of heat or of electricity, according to their different combinations.

Theſe, it muſt be admitted, are points of curioſity and importance; but at preſent it is only neceſſary to admit, what was clearly aſcertained by Dr. Franklin, and ſince confirmed by the obſervations of many other ingenious and able philoſophers, that there is a ſameneſs between the matter of lightning and the electrical fluid. And electrical experiments, although the reſults of them cannot be compared with the effects produced in the vaſt and extenſive operations of nature; yet, by exhibiting to our view ſimilar appearances, they may furniſh us with notions of, at leaſt, the proximate, though we may for ever remain ignorant of the more remote, cauſes which produce thoſe moſt wonderful, and ſometimes moſt dreadful, effects.

Electricity, attached to the ſame kind of ſubſtances, ſhews itſelf in two different forms, which have been diſtinguiſhed by the terms *poſitive* and *negative* electricity. Theſe algebraical terms are well ſuited to deſcribe either two different fluids, or two different ſtates of the ſame fluid; for it is well known by experiment, that electricity, under theſe different forms, has the property (like poſitive and negative quantities in algebraical computations) of deſtroying the effects of each other, when united in equal quantities.

The enquiries hitherto made upon the ſubject of this paper, have in general been confined to what relates to the operation of one fluid only; but in theſe experiments the joint operations of the poſitive and negative electricities are conſidered, to which probably many appearances, that have hitherto occaſioned much controverſy amongſt electricians reſpecting the forms of conductors, may be aſcribed.

It is a known fact in electricity, that if the ſurfaces of two metallic bodies be placed oppoſite and near to each other, the one inſulated, and the other connected with the earth; if a thin plate of any electric ſubſtance be introduced between theſe ſurfaces, and extending beyond their extremities, if electricity be then communicated to the electric by means of the inſulated metallic ſubſtance, the other metallic ſubſtance connected with the earth has the power of putting the ſide of the electric with which it is in contact into the oppoſite ſtate of electricity; and the poſitive and negative electricities on the oppoſite ſurfaces of the electric, by their attractive powers, retain each other in their reſpective ſituations. And, in ſolid electrics, even after the removal of the metallic ſubſtances, if the ſame, or other conducting ſubſtances, having a conducting communication between them, be applied again to the oppoſite ſides of the charged electric, all electricity will diſappear, ſilently, or with an exploſion. Theſe are the phenomena of the electric jar.

It is alſo well known, that if a body of atmoſpheric air, included between metallic ſurfaces, be the electric employed in the experiment above deſcribed, ſimilar effects will be produced.

Hence it may be inferred, that the effects of lightning may ariſe from the diſcharge of large bodies of electrified air; the upper ſurface of which will be in one ſtate of electricity, and the ſurface adjoining the earth and terreſtrial bodies will be in the oppoſite ſtate; and when theſe bodies of charged air paſs over lofty buildings capable of forming a conducting communication between their oppoſite ſurfaces, the exploſion of lightning takes place.

The lower ſurface of a body of charged air may extend over the whole, over a part, or over no part, of a building, at the time it is ſtruck by lightning; the building forming only a part of the conducting communication.

Buildings in general have many metallic ſubſtances belonging to them: it is to theſe ſubſtances, and to their imperfect connection with the earth, that moſt of the miſchief that ariſes from the effects of lightning is generally aſcribed. To prevent this miſchief, metallic conductors have been attached to buildings.

The advantage expected from theſe conductors is founded upon a ſuppoſition, that the matter of lightning will always prefer, in its paſſage to the earth, a continued conducting ſubſtance to one that is broken or interrupted by non-conducting ſubſtances; and therefore it is ſuppoſed that a conductor, conſtructed of a perfectly continued metallic ſubſtance, and extending from above the upper ſurface of a building into the earth, below its foundation, will receive and convey the matter of lightning into the earth, and prevent the miſchief that might happen by its ſtriking any metallic ſubſtance which is not continued to the earth.

But buildings furniſhed with ſuch conductors have been ſtruck by lightning: it is obvious, therefore, that the lightning muſt either have paſſed to the damaged part without approaching the conductor, or it muſt have ſtruck from the conductor to the damaged part: for, although all metallic ſubſtances belonging to a building may be placed at a diſtance from the conductor, yet it cannot be ſuppoſed that any diſtance within the limits of a building can exceed the *ſtriking diſtance* of lightning.

The apparatus with which this experiment was performed, conſiſts of an electrical machine having a glaſs cylinder of about 18 inches diameter, conſtructed by Mr. Nairne, in the form of his patent electrical machines.

Two hollow cylinders [*], made of thin wood covered within and without with tinfoil, are placed, in a vertical poſition, upon two inſulated circular boards; the glaſs pillars which ſupport the inſulated boards are about a foot in height, and are fixed into thick circular boards, covered alſo with tinfoil, and reſting upon the floor of the room.

The interior diameter of each of theſe cylinders is 18 inches; the height from the inſulated board upon which it ſtands, eight feet ſix inches, including an hemiſpherical top; and the diſtance between them is about five feet.

From the centre of each of the boards, that reſt upon the floor, riſes a thick glaſs pillar, which paſſes through a hole of a foot diameter in the centre of the inſulated board into the hollow of the cylinder. This pillar of glaſs extends about ſix inches above the inſulated board, and ſupports, in a vertical poſition, another cylinder made of wood, and covered with tinfoil.

This interior cylinder is one foot in diameter, and its height is ſix feet ſix inches, including an hemiſpherical top. It is placed exactly parallel to the exterior cylinder, the interval in every part being three inches. To this interior cylinder is fixed a metallic chain, by which it can be connected with the exterior cylinder, ſo as to form only one inſulated body covered with tinfoil; or can be connected with the earth, whilſt the exterior cylinder remains inſulated.

The apparatus has alſo a moveable inſulated body, made of cork covered with tinfoil, to repreſent a cloud moving in the air.

And alſo two metallic rods placed upon inſulated ſtands, that they may be connected with, or detached from, the earth at pleaſure. Theſe rods are uſed to repreſent the conductors, or any metallic ſubſtances that belong to buildings; their forms are varied by fixing balls or points upon their upper extremities; and the ſtand in which they are placed being hollow, they can be placed at any height by means of a ſcrew.

To ſhew the effects of the paſſage of the electrical fluid, two inſulated metallic balls are placed within the *ſtriking diſtance* of each other; and, in the interval between them, is put ſome inflammable ſubſtance, ſuch as dried powdered roſin mixed with cotton.

---

[*] It may be of advantage to the reader to conſult the literal references at the end of this memoir before he proceeds. N.

which have been diſtinguiſhed by the terms *poſitive* and *negative* electricity. Theſe algebraical terms are well ſuited to deſcribe either two different fluids, or two different ſtates of the ſame fluid; for it is well known by experiment, that electricity, under theſe different forms, has the property (like poſitive and negative quantities in algebraical computations) of deſtroying the effects of each other, when united in equal quantities.

The enquiries hitherto made upon the ſubject of this paper, have in general been confined to what relates to the operation of one fluid only; but in theſe experiments the joint operations of the poſitive and negative electricities are conſidered, to which probably many appearances, that have hitherto occaſioned much controverſy amongſt electricians reſpecting the forms of conductors, may be aſcribed.

It is a known fact in electricity, that if the ſurfaces of two metallic bodies be placed oppoſite and near to each other, the one inſulated, and the other connected with the earth; if a thin plate of any electric ſubſtance be introduced between theſe ſurfaces, and extending beyond their extremities, if electricity be then communicated to the electric by means of the inſulated metallic ſubſtance, the other metallic ſubſtance connected with the earth has the power of putting the ſide of the electric with which it is in contact into the oppoſite ſtate of electricity; and the poſitive and negative electricities on the oppoſite ſurfaces of the electric, by their attractive powers, retain each other in their reſpective ſituations. And, in ſolid electrics, even after the removal of the metallic ſubſtances, if the ſame, or other conducting ſubſtances, having a conducting communication between them, be applied again to the oppoſite ſides of the charged electric, all electricity will diſappear, ſilently, or with an exploſion. Theſe are the phenomena of the electric jar.

It is alſo well known, that if a body of atmoſpheric air, included between metallic ſurfaces, be the electric employed in the experiment above deſcribed, ſimilar effects will be produced.

Hence it may be inferred, that the effects of lightning may ariſe from the diſcharge of large bodies of electrified air; the upper ſurface of which will be in one ſtate of electricity, and the ſurface adjoining the earth and terreſtrial bodies will be in the oppoſite ſtate; and when theſe bodies of charged air paſs over lofty buildings capable of forming a conducting communication between their oppoſite ſurfaces, the exploſion of lightning takes place.

The lower ſurface of a body of charged air may extend over the whole, over a part, or over no part, of a building, at the time it is ſtruck by lightning; the building forming only a part of the conducting communication.

Buildings in general have many metallic ſubſtances belonging to them: it is to theſe ſubſtances, and to their imperfect connection with the earth, that moſt of the miſchief that ariſes from the effects of lightning is generally aſcribed. To prevent this miſchief, metallic conductors have been attached to buildings.

The advantage expected from theſe conductors is founded upon a ſuppoſition, that the matter of lightning will always prefer, in its paſſage to the earth, a continued conducting ſubſtance to one that is broken or interrupted by non-conducting ſubſtances; and therefore it is ſuppoſed that a conductor, conſtructed of a perfectly continued metallic ſubſtance, and extending from above the upper ſurface of a building into the earth, below its foundation, will receive and convey the matter of lightning into the earth, and prevent the miſchief that might happen by its ſtriking any metallic ſubſtance which is not continued to the earth.

But buildings furniſhed with ſuch conductors have been ſtruck by lightning: it is obvious, therefore, that the lightning muſt either have paſſed to the damaged part without approaching the conductor, or it muſt have ſtruck from the conductor to the damaged part: for, although all metallic ſubſtances belonging to a building may be placed at a diſtance from the conductor, yet it cannot be ſuppoſed that any diſtance within the limits of a building can exceed the *ſtriking diſtance* of lightning.

The apparatus with which this experiment was performed, conſiſts of an electrical machine having a glaſs cylinder of about 18 inches diameter, conſtructed by Mr. Nairne, in the form of his patent electrical machines.

Two hollow cylinders *, made of thin wood covered within and without with tinfoil, are placed, in a vertical poſition, upon two inſulated circular boards; the glaſs pillars which ſupport the inſulated boards are about a foot in height, and are fixed into thick circular boards, covered alſo with tinfoil, and reſting upon the floor of the room.

The interior diameter of each of theſe cylinders is 18 inches; the height from the inſulated board upon which it ſtands, eight feet ſix inches, including an hemiſpherical top; and the diſtance between them is about five feet.

From the centre of each of the boards, that reſt upon the floor, riſes a thick glaſs pillar, which paſſes through a hole of a foot diameter in the centre of the inſulated board into the hollow of the cylinder. This pillar of glaſs extends about ſix inches above the inſulated board, and ſupports, in a vertical poſition, another cylinder made of wood, and covered with tinfoil.

This interior cylinder is one foot in diameter, and its height is ſix feet ſix inches, including an hemiſpherical top. It is placed exactly parallel to the exterior cylinder, the interval in every part being three inches. To this interior cylinder is fixed a metallic chain, by which it can be connected with the exterior cylinder, ſo as to form only one inſulated body covered with tinfoil; or can be connected with the earth, whilſt the exterior cylinder remains inſulated.

The apparatus has alſo a moveable inſulated body, made of cork covered with tinfoil, to repreſent a cloud moving in the air.

And alſo two metallic rods placed upon inſulated ſtands, that they may be connected with, or detached from, the earth at pleaſure. Theſe rods are uſed to repreſent the conductors, or any metallic ſubſtances that belong to buildings; their forms are varied by fixing balls or points upon their upper extremities; and the ſtand in which they are placed being hollow, they can be placed at any height by means of a ſcrew.

To ſhew the effects of the paſſage of the electrical fluid, two inſulated metallic balls are placed within the *ſtriking diſtance* of each other; and, in the interval between them, is put ſome inflammable ſubſtance, ſuch as dried powdered roſin mixed with cotton.

---

* It may be of advantage to the reader to conſult the literal references at the end of this memoir before he proceeds. N.

EXPERIMENT.

THAT this experiment might reſemble, as much as an electrical apparatus is capable of exhibiting, the operations of nature, it was performed by charging and diſcharging plates of air.

Arrangement of the Apparatus.

THE metallic chains affixed to the interior tinfoiled cylinders are connected with the tinfoiled boards which reſt upon the floor of the room. A metallic communication is then made between theſe boards, and another metallic communication between the exterior cylinders, which remain inſulated.

The exterior cylinders, thus united, are connected with one of the conductors of the electrical machine (in this experiment it is with the poſitive); the other conductor (or rubber) being connected with the earth.

The electrical machine being put in action, the exterior tinfoiled cylinders become electrified; and, the interior cylinders being connected with the earth, the body of air between them becomes charged; and, if a circuit be made between the exterior and interior cylinders, an exploſion takes place, and a ſtrong electrical ſhock may be felt.

The exterior cylinders repreſent the upper ſurface, and the interior cylinders repreſent the lower ſurface of a body of charged air in the atmoſphere.

The moveable body, repreſenting a cloud moving in the air, is connected with the exterior cylinders, by which means it becomes a part of the upper electrified ſurface of the body of charged air, and can be made to paſs over the building at pleaſure.

One of the metallic rods, placed in perfect communication with the earth (but ſeparate from the communication of the interior cylinders with the earth), repreſents a metallic conductor attached to a building.

The inſulated metallic balls, with the roſin placed in the interval between them, are ſuppoſed to repreſent any metallic ſubſtances belonging to the building.

A glaſs tube filled with water, and about 18 inches in length, is placed between the conductor and one of theſe balls, to repreſent a conducting communication, which muſt frequently occur during a thunder-ſtorm, between the conductor and other metallic ſubſtances belonging to the building.

Since a conductor may be raiſed to any height at pleaſure, it is, in this experiment, ſuppoſed to be raiſed rather beyond the diſtance at which any pointed metallic ſubſtances belonging to the building might, by their influence on the operations of the electrical fluid, endanger the building.

This experiment may be divided into three caſes. 1ſt, The electricity oppoſite to that of the cloud may extend over the conductor, and all other metallic ſubſtances belonging to the building. 2dly, It may extend over ſome metallic ſubſtances, but not over the conductor. 3dly, It may not extend over the conductor, or over any part of the building.

CASE I.

THE electricity oppoſite to that of the cloud, that is, the lower ſurface of the body of charged air, is ſuppoſed to extend over the conductor, and over all metallic ſubſtances belonging to the building.

The conductor is therefore connected with the communication between the interior cylinders; and one of the inſulated balls being connected by means of the glaſs tube with the conductor, let the other ball be connected with the interior cylinders.

RESULT.

WHEN the conductor was terminated by a point, and the cloud paſſed near it, an exploſion took place upon the point, and the electrified air between the cylinders was inſtantly diſcharged. When the cloud paſſed at a greater diſtance, no exploſion took place; but the electricity diſappeared from the tinfoiled cylinders, and the plate of air between them was ſilently diſcharged, as was ſhewn by the electrometer.

When the conductor was terminated by a ball, and the cloud paſſed near it; the ball was ſtruck, and the plate of air between the cylinders diſcharged as before: but when the cloud paſſed beyond the *ſtriking diſtance* to the ball, the tinfoiled cylinders continued electrified, as was ſhewn by the electrometer; and the plate of air remained in a charged ſtate.

In this caſe no ſparks paſſed between the balls where the roſin was placed.

CASE II.

THE lower ſurface of the body of charged air is now ſuppoſed to extend over ſome of the metallic ſubſtances belonging to the building, but not over the conductor.

The communication between the conductor and the interior cylinders being removed, the remainder of the apparatus was arranged as in the firſt caſe.

RESULT.

WHEN the conductor was terminated by a point, and the cloud paſſed near to it; a ſpark took place on the point, and the roſin in the interval between the balls was inſtantly ſet on fire. When the cloud paſſed at a greater diſtance, the electricity of the charged air diſappeared ſilently, as in the firſt caſe.

When the conductor terminated by a ball, and the cloud paſſed near to it; it was ſtruck, and the roſin between the balls inſtantly ſet on fire: but when no ſpark paſſed between the cloud and the ball, no ſparks paſſed where the roſin was placed, and no decreaſe of electricity appeared upon the tinfoiled cylinders.

CASE III.

THE lower ſurface of the body of electrified air is now ſuppoſed not to extend over any part of the building, and conſequently not over the conductor, or over any other metallic ſubſtance belonging to it.

There-

Therefore the metallic rod, which repreſents the conductor, remaining as in the laſt caſe; the metallic ball which was connected with the communication between the interior cylinders has now a ſeparate communication with the earth only.

RESULT.

WHEN the conductor was terminated by a point, and the cloud paſſed near it; ſome weak ſparks paſſed from it to the point on the conductor. If the cloud paſſed at a greater diſtance, no ſparks appeared, but the electricity on the tinfoiled cylinders was diminiſhed as in the former caſes.

When the conductor was terminated by a ball, weak ſparks paſſed between the cloud and the ball, and the electricity of the tinfoiled cylinders decreaſed; but when the cloud paſſed at a diſtance above the point at which no ſparks paſſed, there was no variation to be obſerved in the electricity of the charged air.

In this caſe no ſparks paſſed in the interval between the balls where the roſin was placed.

To the three caſes already exhibited, a fourth may be added, to ſhew that the lightning may have paſſed to the damaged part of the building without having ſtruck the conductor.

Although a conductor may be extended to a conſiderable diſtance above the building, yet ſome part of the upper ſurface of the body of charged air may approach as near to ſome of the metallic ſubſtances belonging to the building as to the conductor; and which may be exhibited in this caſe in two parts.

CASE IV.—PART I.

THE metallic rod, which repreſents the conductor, has a perfect communication with the earth only; the glaſs tube filled with water, which connected it with one of the inſulated balls, being removed. The other metallic rod, which is to repreſent any pointed metallic ſubſtance, is placed upon an inſulated ſtand, and is connected by a metallic communication with the inſulated ball to which the tube of water had been attached; the other inſulated ball remaining connected with the communication between the interior cylinders as before.

The lower ſurface of the body of charged air is here ſuppoſed not to extend over the conductor, or over the other metallic body, but to extend over ſome ſubſtance within ſtriking diſtance of it.

That the cloud might paſs at nearly the ſame diſtance from the point on the metallic rod as from the conductor, the point on the metallic rod was fixed nearly at the ſame height as the upper extremity of the conductor.

RESULT.

WHETHER the conductor was terminated by a point or by a ball, and the cloud paſſed near to it as well as to the point upon the metallic rod, ſometimes the conductor was ſtruck, ſometimes the point upon the metallic rod was ſtruck, and which inſtantly ſet fire to the roſin between the balls.

Sometimes, when a ſpark paſſed between the cloud and conductor, and no ſpark upon the point of the other metallic rod; ſparks would appear between the inſulated balls, and of ſufficient ſtrength ſometimes to light the roſin.

Theſe ſparks were the weakeſt when the conductor was terminated by a point, and much ſtronger when it was terminated by a ball.

This circumſtance ſeemed to ſhew, that the paſſage of the electrical fluid is not confined to one circuit only.

CASE IV.—Part II.

THE lower ſurface of the body of charged air is now ſuppoſed to extend over the conductor; the other metallic ſubſtances remaining in the ſame ſtate as before.

The conductor is now connected with the communication between the interior cylinders.

RESULT.

WHEN the cloud paſſed near to the conductor, and alſo within ſtriking diſtance of the point upon the other metallic rod; no exploſion or ſpark appeared in any of the intervals; but the body of air was ſilently diſcharged, as was ſhewn by the electrometer: and the ſame effect was produced, when, by lowering the point upon the conductor, the cloud paſſed nearer to the point on the metallic rod than to the conductor.

When the conductor was terminated by a ball, the ball was ſtruck when the cloud paſſed near to it; and if the ball was ſituated as the point was, ſo that the cloud paſſed nearer to the point upon the metallic rod than to the ball on the conductor, the point on the metallic rod was ſtruck, and the roſin inſtantly lighted.

CONCLUSION.

FROM this experiment it appears to be manifeſt, that the advantages ariſing from metallic conductors, erected for the purpoſes of ſecuring buildings from the effects of lightning, depend more upon the lower ſurface of the body of charged air extending over them, than upon their form or conſtruction.

It is not eaſy to decide, from the reſult of the experiment, whether they ſhould be terminated by *points* or by *balls*. Probably, with reſpect to the large operations of nature, there may be no difference. The ball uſed in this experiment is three inches diameter; the largeſt ſurface of charged air ſcarcely exceeded 70 ſquare feet: but if ſome acres of charged air were brought into action, the ball of three inches would ſoon be reduced to a point. However, as the ſubject has occaſioned much controverſy, we may take the opportunity of inveſtigating it from the data afforded by this experiment.

If we reaſon upon what is exhibited in the firſt and third Caſes, it is obvious that pointed conductors are to be preferred to thoſe terminated by balls. They have the property of acting at a much greater diſtance than balls, and have the power of deſtroying, ſilently, the effects of lightning, when balls can only accompliſh it by means of an exploſion, which muſt always be attended with ſome danger.

But if we reaſon upon the reſult in the ſecond Caſe, we ſhall find that the power which points have of acting at a greater diſtance than balls, will make them more liable to produce the miſchief ſhewn in that experiment.

Under theſe conſiderations, the conductor terminated by a ball might be thought preferable to one terminated by a point: but when we recollect that moſt of the metallic ſubſtances which belong to buildings are generally terminated in edges and points; that they have the ſame influence as the point upon the conductor; that their operation may be attended with more danger, and that they may extend their influence beyond that of a conductor terminated by a ball; we may therefore conclude, that the height to which a pointed conductor is generally raiſed above the other metallic ſubſtances belonging to a building, when compared with the vaſt diſtances at which lightning can act, will not increaſe, in any great degree, the danger to which a building may be expoſed under the circumſtances exhibited in the ſecond caſe of this experiment: and therefore conductors that are terminated by points are more likely to produce the good effects expected from them than thoſe which are terminated by balls.

Theſe conſiderations upon the effects that may be produced by the joint operations of poſitive and negative electricity, have, thus far, been confined to what may happen within a ſpace occupied by a building; but they may be extended to much greater ſpaces upon the ſurface of the earth. A conductor may be ſtruck by lightning, and (if the lower ſurface of the body of charged air extends over no part of the building) may convey the lightning into the earth, without the leaſt miſchief being done to the building; but when the lightning arrives at the lower extremity of the conductor, there is no proof that by its union with the earth it becomes decompoſed. That decompoſition cannot happen until it becomes united with its oppoſite electricity, which may be attached to a body of earth or ſubſtances far removed from the conductor. The lightning will therefore continue its operations until it arrives at the place where the oppoſite electricity is preſent, and in its paſſage will occaſion much miſchief if it does not meet with good conducting ſubſtances.

The accidents which have been known to happen at a diſtance from the place which was ſeen to be ſtruck by lightning, have been aſcribed (by reaſoning according to the theory of Dr. Franklin) to what is called a *returning ſtroke*; but it might, with an equal degree of probability at leaſt, be aſcribed to the object to which the accident happened being within the circuit which the lightning makes between the oppoſite ſurfaces of a body of charged air.

London, Nov. 29, 1797. HENRY HALDANE.

In addition to what has been already written two figures are annexed (Plate XVIII). The firſt is a drawing to repreſent a building furniſhed with a conductor, which is ſuppoſed to be ſtruck by lightning.

The ſecond is a plan of a part of the electrical apparatus, arranged as deſcribed in the ſecond Caſe of theſe Experiments.

References to Fig. I.

A A repreſents the upper ſurface of a body of charged air.

B B B the lower ſurface of the ſame body of charged air, which is attached to the earth and terreſtrial bodies, and in an oppoſite ſtate of electricity to that of the upper ſurface A. A.

This lower ſurface may extend to the places marked 1, 2, 3; that is, the limit of charged air may be repreſented by the dotted lines A 1, A 2, A 3, as deſcribed in the three Caſes of the Experiment.

C the conductor.

C F The

C F, the paſſage which the lightning may take, according to the ſecond Caſe of this Experiment, and damage the ſame building at F, if the limit of charged air be at A 2; or the building near to it, if the limit of charged air be at A 3.

References to Fig. II.

A A, the exterior cylinders.

B B, the interior cylinders.

The interval between them is ſhaded, and contains the air which is charged.

A A, the metallic rod which connects the exterior cylinders.

B B, the metallic rod which connects the interior cylinders.

C, the conductor.

D and E, the two inſulated metallic balls.

C D, the glaſs tube of water which connects the conductor C with the ball D.

E R, a metallic rod, which connects the ball E with the communication B B, between the interior cylinders.

F, ſome inflammable ſubſtance, as roſin mixed with cotton, placed between the balls D E.

G, a metallic rod, to repreſent any metallic ſubſtance belonging to the building, to be placed or removed at pleaſure. (It was uſed in the fourth Caſe, but was removed from the apparatus in the ſecond Caſe.)

H H, circular pieces of cork covered with tinfoil, fixed to a metallic rod P H.

This rod P H is fixed at O, into a glaſs pillar of about ten inches in height; the lower end of the glaſs pillar is fixed into the centre of a wheel S, which turns upon a pivot that is fixed into the table T.

The edges of the wheel S being grooved, two cords are applied to the two grooves. Theſe cords, paſſing through pulleys in the table and alſo in the ſtand of it, are conveyed to the hand of the perſon who is turning the cylinder of the electrical machine, by which means he can make the body H revolve at pleaſure.

From the rod at O riſes a metallic ſtem, with a ſwivel upon the top of it. To this ſwivel is fixed K L, which connects it with the exterior cylinder A.

Over the top of this ſtem below the ſwivel paſſes a cord, fixed at one end to the centre of H, and at the other to the balance weight P, which ſerves to keep P H in an horizontal poſition.

M N, a metallic rod which connects the exterior cylinders A A with the poſitive conductor of the electrical machine.

The metallic rod M N is about eight feet above the floor of the room, and the moveable rod P H traverſes at about ſix feet above the floor. H. H.

II.

New Conſtruction of the Air-Pump. By JAMES SADLER, *Eſq. Chemiſt to the Admiralty.*

THE conſtruction of the air-pump, with the improvements of Mr. Cuthbertſon and the Rev. J. Prince, have been explained in a former communication *. In order that the effect of this inſtrument may be as perfect as its theory will allow, it is neceſſary that its valves ſhould infallibly open at the requiſite periods of the operation, and that the ſpace between the piſton and the upper or the lower valve, when at its extreme poſitions, ſhould be the leaſt poſſible, or rather that there ſhould be no vacuity remaining. It has been ſhown that in the air-pump of Prince the lower ſpace is abſolutely out of the queſtion, becauſe there is no lower valve; and the upper ſpace may be, and probably is, much diminiſhed by the effect of the oil. The air-pump of Cuthbertſon, in which the conſtruction of the valves is moſt admirable, and their action abſolutely ſecured, ſeems to poſſeſs a ſtill greater portion of this laſt advantage from the circulation of the oil, though not directly intended to anſwer this purpoſe. Mr. Sadler has directed his attention immediately to the good effects which a due application of that fluid is calculated to produce, and has conſtructed an air-pump, poſſeſſing the deſirable requiſites of ſimplicity, cheapneſs and power, of which Figure 1. Plate XIX. is a ſketch.

---

* Philoſophical Journal, i. 119.

A B repreſents the barrel, Q a ſolid piſton or plunger fitted into the barrel, and leathered ſomewhat looſely. O is the piſton-rod, capable of being raiſed and depreſſed in the uſual way by a handle and toothed wheel which acts upon the ſtraight rack-work. The barrel communicates, by means of the pipe C, with a chamber D, at the upper part of which is a valve K, opening upwards into a ciſtern L. P is the plate for the receiver, through which a communication-pipe N E paſſes to the interior part of the veſſel, D. At the lower extremity E of this pipe there is a valve, connected with the lever F H, by a wire F E paſſing through a collar of leathers. G is the axis of the lever F H, upon which, by means of a weight at H, an action is produced which preſſes the valve E upwards into its place. H I is another wire or tail, by means of which the extremity H of the lever may be raiſed, and conſequently the valve E opened.

From theſe particulars the effect of the machine may be explained. Let the piſton be drawn out and the barrel filled with oil, which will of courſe riſe to the ſame level in D. Let the piſton then be put in and preſſed downwards by turning the handle. The oil will be driven from the barrel into D, driving the air before it through the valve K ſo completely, that a portion of the oil itſelf will follow into the ciſtern L. Let the piſton be again drawn upwards, and the oil will follow, becauſe D is higher than the valve, and will conſequently leave an empty ſpace in this veſſel D. As ſoon as the piſton-rod has arrived at a certain height, it will act by means of a prominence upon the catch I, and open the valve E. A portion of the air in the receiver will therefore ruſh into D. Upon the return of the piſton the valve E will in the firſt place ſhut, and the air in D will be extruded through K. This extruſion will be as complete as before, becauſe at every ſtroke a portion of oil from the ciſtern L will paſs through the pipe L M to the upper part of the barrel, and eſcape beſide the piſton while riſing, on account of the looſe manner in which it is leathered. From this contrivance it is provided that the internal part of the machine ſhall contain oil ſufficient not only to diſplace the air, but even to paſs through in a ſmall quantity after it by means of the valve K; and conſequently, by working the apparatus the exhauſtion may be carried to any deſired degree. Fig. 2. repreſents the ſame machine viewed in another direction.

In the preſent pump the valve K may be conſidered as repreſenting the valve which cloſes the upper part of the barrel in Cuthbertſon's pump, and the valve E as the lower fixed valve. We ſee therefore from this combination that the ſolid piſton is made to act both ways, in the ſame manner as if a ſide valve had been made in Cuthbertſon's barrel, opening outwards beneath the piſton, and the piſton itſelf had been made ſolid. For in ſuch an arrangement the riſing ſtroke would have drawn the air out of the receiver, and the deſcending ſtroke would have driven it out ſideways. But Mr. Sadler has removed the objections which might be made to ſuch a diſpoſition, by making his piſton in effect fluid, and therefore capable of adapting itſelf to every cavity or irregularity in the veſſel D.

How far it might be found, in proceſs of time, that the oil might become changed by the circulation, and leſs fit for the purpoſe; whether it might carry minute bubbles of air along with it, and whether mercury or any other fluid in veſſels of ſuitable materials might be preferable, I know not. But thus much is certain, that the means of conſtructing an inſtrument of this kind, with a few ſlight variations to facilitate the execution, are within the power of many ingenious philoſophical men who have neither the ability nor the ſkill to procure a good air-pump of the other conſtructions.

Fig. 3 repreſents a barrel in which the imperfection of a ſpace between the piſton and lower valve is obviated, and the action of this laſt valve ſecured. A B repreſents the barrel; O, the piſton rod; C, the valve in the piſton; I I, the leathers; L L, a hollow cylindrical piece of metal, which nearly fits the cavity M M, which ſurrounds the lower valve D and its tube. The piece L L does not touch the barrel, and there are two holes, K K, communicating with the central cavity in the piſton, from the ſpace between L and the barrel. This piſton alſo is not leathered very tight, becauſe it is intended that the preſſure of the atmoſphere ſhall force a portion of oil into the lower cavity of the barrel during the aſcent of the piſton. P is the tube which communicates with the receiver. N is a ſtopping-ſcrew ſerving to cloſe a hole through which any ſuperfluous oil may be drawn. G E is a lever applied to open the lower valve D, when the piſton has riſen to a certain height, and by a proper communication acts upon the extremity G by the wire H. Let it be ſuppoſed that the piſton is down, and that oil is poured into the barrel above it. While the piſton is drawn up, a portion of the oil will inſinuate itſelf paſt the leathering into the partial vacuum below. As ſoon as the piſton has riſen nearly to its limit, the valve D will be opened, and part of the air in the receiver will paſs into the barrel through P. Upon the return of the piſton the valve D will in the firſt place cloſe; and when the piſton ſhall arrive at its lower ſtation, the oil in M M, being diſplaced by the piece L L, will riſe through all the cavities of the piſton, and drive out whatever portion of air may not have eſcaped through C during the deſcent. And ſince every riſing ſtroke of the piſton will be attended with the tranſition of a portion of the oil from the upper to the under part of the barrel, the ſpace M M will, at the end of every deſcending ſtroke, be found to contain not only a ſufficient quantity to fill the place which would elſe have been occupied by air, but alſo another portion, which will paſs through C at the termination of the ſtroke, and complete the effect.

III.

Obſervations on Phoſphorus. By Citizen BRUGNATELLI, *Profeſſor of Chemiſtry, &c. at Pavia*[*].

IN one of the public meetings of the Laboratory of the Univerſity, when the ſubject related to the peculiar properties of Kunckel's phoſphorus, and required thoſe experiments to be repeated upon which Gœttling has built his theory in oppoſition to that of the French chemiſts; I repeated the valuable experiments of Fourcroy and Vauquelin, which are deſcribed in the preſent volume [†], which ſhew the error of the German chemiſts; and as I have obſerved ſome peculiar circumſtances not mentioned by the Pariſian chemiſts, I flatter myſelf that a detail of theſe experiments will not diſpleaſe my readers.

EXPERIMENT I.—*The rapid Solution of Phoſphorus in pure Oxygene Gas*[‡]*; and its Combuſtion, which takes place by the Mixture of other Gaſes.*

I introduced a cylindrical piece of phoſphorus ſupported on a tube of glaſs into a long jar, the upper part of which contained ſix cubic inches of oxygene gas obtained from nitre. The reſt of the jar was full of water. It was formed of thin white glaſs, three inches in diameter; and the experiment was made in the moſt perfect darkneſs with the hydro-pneumatic apparatus. The thermometer of Reaumur ſtood at 12 degrees above 0, (59, of Fahrenheit). After the phoſphorus had remained in this poſition for one minute, it was withdrawn, and a few bubbles of inflammable gas were introduced. This gas was obtained by diſſolving iron in the ſulphuric acid [§] diluted with water, which was conſtantly uſed in theſe experiments. As ſoon as the bubbles of this gas had entered the phoſphorated oxygene gas, the whole maſs of elaſtic fluid beneath the glaſs appeared phoſphoreſcent. Azotic gas, obtained from animal fibre by means of nitric acids, produced the ſame effect.

In ſome inſtances the phoſphorus introduced into the oxygene gas produced a ſlight phoſphoric vapour, which immediately ceaſed; but the ſame phenomenon was produced by introducing inflammable gas into this gas. But when by accident the phoſphorus introduced into the oxygene gas began to ſhine, the gas itſelf, which had thus been placed in contact with the phoſphorus, no longer ſhone, even after the expiration of ſome time by the addition of the afore-mentioned gaſes, in whatever proportion they were added, unleſs the white vapour was condenſed and the phoſphorus extinguiſhed.

---

[*] Annali di Chimica e Hiſtoria Naturale, tome xiii. ann. 1797, page 275. I do not poſſeſs this work, but have followed the tranſlation of Citizen Van Mons in the 24th volume of the Annales de Chemie, whoſe accuracy and knowledge of the ſubject afford the moſt perfect aſſurance of his fidelity.

[†] Ibid. page 3.

[‡] The author calls this gas *therm-oxygene*.

[§] The author calls this acid *oxi-ſulphuric*. He prefixes to the name of each acid the ſyllables *oxi*, ſuch as oxi-nitric, oxi-phoſphoric, &c.

M. Van Mons has preferred the nomenclature preſented by the French chemiſts; and juſtly remarks, that, if every chemiſt were to make one, it would ſoon become impoſſible to diſtinguiſh the ſubſtances concerning which they might write.

EXP.

EXP. II.—*Phoſphorated Oxygene Gas becomes luminous by the Contact of the Oxygenated Muriatic Acid Gas.*

AFTER having left a cylinder of phoſphorus for one minute in ſeveral cubic inches of pure oxygene gas, in which it did not ſhine at all, nor affect the tranſparency by white vapours when brought into the light; a bubble of oxygenated muriatic acid gas, obtained by diſtillation of the ordinary muriatic acid from the black oxide of manganeſe, was introduced. The mixture of the two gaſes produced a ſoft light through the whole maſs of elaſtic fluid contained in the veſſel.

EXP. III. *Phoſphorated Oxygene is alſo luminous with Nitrous Gas.*

NITROUS gas is very greedy of oxygene, in order to convert itſelf into nitrous acid. The nitric acid ſuddenly takes this gas from the oxygene gas, while heat is developed, and the combination is ſhewn in red vapours. I was deſirous of aſcertaining what would happen when nitrous gas was introduced into phoſphorated oxygene gas, according to the uſual proceſs. As I performed my experiment in the dark, I could not obſerve the moment in which the nitrous gas arrived at the upper part of the jar in contact with the phoſphorated gas. A very perceptible azure light was ſeen, which was more conſiderable than had been afforded by the uſe of any other kind of gas. A ſecond introduction of nitrous gas produced no further effect. The veſſel being afterwards inſpected by the light of a candle, the mixture of the two gaſes appeared ſo opake, and the glaſs ſo loaded with white vapours, that it appeared as if filled with milk.

EXP. IV.—*Phoſphorus is ſoluble in Hydrogene Gas.*

INFLAMMABLE gas diſſolves phoſphorus as ſpeedily as oxygene gas in the firſt Experiment, and the whole volume of this gas is impregnated and becomes phoſphoreſcent by the contact of oxygene gas, or the oxygenated muriatic acid gas. But, in order to obſerve this phenomenon in perfection, it is neceſſary to operate with pure inflammable gas, which has not long been in contact with water, becauſe this would prevent its acting upon the phoſphorus with the ſame energy; and as this gas abſorbs a certain portion of pure air from the water, it would follow that the phoſphorus would produce more or leſs of white vapour, and readily emit light. It is therefore neceſſary to operate over mercury.

EXP. V. VI. and VII.—*The Appearances of Phoſphorus in the Oxygenated Muriatic Acid Gas.*

A PIECE of phoſphorus was introduced into a cylinder of glaſs filled with oxygenated muriatic acid gas over water. It did not burn perceptibly in the dark; but in the light, white vapours were ſeen through the whole of the glaſs in ſuch plenty as to render it opake. A ſhort time afterwards the phoſphorus was fuſed and ran like wax by heat, or camphor by the nitric acid. The phoſphorus in this ſtate floated on the water, inſtead of falling to the bottom as before its fuſion. The temperature of the atmoſphere at the time of making this experiment, was about ten degrees above o. I put a large cylindrical piece of phoſphorus into a cryſtal decanter, placed on a table, and full of very pure oxygenated muriatic acid gas. The capacity of the bottle was eight cubic inches, and it was entirely full. The phoſphorus took fire with flame, and threw out a great number of very brilliant ſparks againſt the ſides of the veſſel, which it heated, and itſelf became entirely fuſed. The veſſel was

Therefore the metallic rod, which repreſents the conductor, remaining as in the laſt caſe; the metallic ball which was connected with the communication between the interior cylinders has now a ſeparate communication with the earth only.

RESULT.

WHEN the conductor was terminated by a point, and the cloud paſſed near it; ſome weak ſparks paſſed from it to the point on the conductor. If the cloud paſſed at a greater diſtance, no ſparks appeared, but the electricity on the tinfoiled cylinders was diminiſhed as in the former caſes.

When the conductor was terminated by a ball, weak ſparks paſſed between the cloud and the ball, and the electricity of the tinfoiled cylinders decreaſed; but when the cloud paſſed at a diſtance above the point at which no ſparks paſſed, there was no variation to be obſerved in the electricity of the charged air.

In this caſe no ſparks paſſed in the interval between the balls where the roſin was placed.

To the three caſes already exhibited, a fourth may be added, to ſhew that the lightning may have paſſed to the damaged part of the building without having ſtruck the conductor.

Although a conductor may be extended to a conſiderable diſtance above the building, yet ſome part of the upper ſurface of the body of charged air may approach as near to ſome of the metallic ſubſtances belonging to the building as to the conductor; and which may be exhibited in this caſe in two parts.

CASE IV.—Part I.

THE metallic rod, which repreſents the conductor, has a perfect communication with the earth only; the glaſs tube filled with water, which connected it with one of the inſulated balls, being removed. The other metallic rod, which is to repreſent any pointed metallic ſubſtance, is placed upon an inſulated ſtand, and is connected by a metallic communication with the inſulated ball to which the tube of water had been attached; the other inſulated ball remaining connected with the communication between the interior cylinders as before.

The lower ſurface of the body of charged air is here ſuppoſed not to extend over the conductor, or over the other metallic body, but to extend over ſome ſubſtance within ſtriking diſtance of it.

That the cloud might paſs at nearly the ſame diſtance from the point on the metallic rod as from the conductor, the point on the metallic rod was fixed nearly at the ſame height as the upper extremity of the conductor.

RESULT.

WHETHER the conductor was terminated by a point or by a ball, and the cloud paſſed near to it as well as to the point upon the metallic rod, ſometimes the conductor was ſtruck, ſometimes the point upon the metallic rod was ſtruck, and which inſtantly ſet fire to the roſin between the balls.

Sometimes, when a ſpark paſſed between the cloud and conductor, and no ſpark upon the point of the other metallic rod; ſparks would appear between the inſulated balls, and of ſufficient ſtrength ſometimes to light the roſin.

Theſe ſparks were the weakeſt when the conductor was terminated by a point, and much ſtronger when it was terminated by a ball.

This circumſtance ſeemed to ſhew, that the paſſage of the electrical fluid is not confined to one circuit only.

CASE IV.—PART II.

THE lower ſurface of the body of charged air is now ſuppoſed to extend over the conductor; the other metallic ſubſtances remaining in the ſame ſtate as before.

The conductor is now connected with the communication between the interior cylinders.

RESULT.

WHEN the cloud paſſed near to the conductor, and alſo within ſtriking diſtance of the point upon the other metallic rod; no exploſion or ſpark appeared in any of the intervals; but the body of air was ſilently diſcharged, as was ſhewn by the electrometer: and the ſame effect was produced, when, by lowering the point upon the conductor, the cloud paſſed nearer to the point on the metallic rod than to the conductor.

When the conductor was terminated by a ball, the ball was ſtruck when the cloud paſſed near to it; and if the ball was ſituated as the point was, ſo that the cloud paſſed nearer to the point upon the metallic rod than to the ball on the conductor, the point on the metallic rod was ſtruck, and the roſin inſtantly lighted.

CONCLUSION.

FROM this experiment it appears to be manifeſt, that the advantages ariſing from metallic conductors, erected for the purpoſes of ſecuring buildings from the effects of lightning, depend more upon the lower ſurface of the body of charged air extending over them, than upon their form or conſtruction.

It is not eaſy to decide, from the reſult of the experiment, whether they ſhould be terminated by *points* or by *balls*. Probably, with reſpect to the large operations of nature, there may be no difference. The ball uſed in this experiment is three inches diameter; the largeſt ſurface of charged air ſcarcely exceeded 70 ſquare feet: but if ſome acres of charged air were brought into action, the ball of three inches would ſoon be reduced to a point. However, as the ſubject has occaſioned much controverſy, we may take the opportunity of inveſtigating it from the data afforded by this experiment.

If we reaſon upon what is exhibited in the firſt and third Caſes, it is obvious that pointed conductors are to be preferred to thoſe terminated by balls. They have the property of acting at a much greater diſtance than balls, and have the power of deſtroying, ſilently, the effects of lightning, when balls can only accompliſh it by means of an exploſion, which muſt always be attended with ſome danger.

But if we reaſon upon the reſult in the ſecond Caſe, we ſhall find that the power which points have of acting at a greater diſtance than balls, will make them more liable to produce the miſchief ſhewn in that experiment.

Under

The bottle, when obſerved in the dark, exhibited no light; the water agitated in every direction produced no light; but it was ſeen to inflame immediately upon the contact of the atmoſphere by taking out the cork; and when the light diſappeared, it was capable of being produced again by agitating the ſame water, which immediately before did not exhibit any light by the ſtrongeſt agitation.

In this experiment the flame likewiſe aroſe from the phoſphoric particles on the ſurface of the water, with the vapours of the phoſphorus which it condenſed by burning. The total want of light in the included air before it was open aroſe from the total want of pure air, which is neceſſary for the production of this phenomenon.

EXP. X.

I DISSOLVED a ſmall piece of phoſphorus in pure oil of turpentine. The oil became ſlightly turbid, afterwards clear, aſſumed a yellowiſh colour, and emitted a ſlight ſmell of phoſphorus. A cryſtal phial was half filled with this ſolution. I was deſirous of obſerving whether agitation would afford a flame in the air, as in the former experiment; but I did not perceive the leaſt light. I obſerved the ſame thing with the prepared oil of thyme, containing phoſphorus. The temperature of the atmoſphere was 15 degrees above zero.

EXP. XI.—*Obſervation on Phoſphorated Alkohol.*

PHOSPHORUS diſſolves in alkohol when it is very pure. In proportion as the ſolution is effected, the alkohol loſes its proper ſmell, and aſſumes that of phoſphorus, which is very diſagreeable. The alkohol takes up very little of the phoſphorus, and continues entirely limpid. I attempted, but conſtantly in vain, to obſerve the phenomenon of the flame in the air of the veſſels, which I had partly filled with phoſphorated alkohol. Its habitude was in every reſpect ſimilar to that of phoſphorated oil of turpentine at the ordinary temperature of the atmoſphere.

EXP. XII. *Water cauſes Phoſphorated Alkohol to ſhine very ſtrongly.*

IF in perfect darkneſs a drop of phoſphorated alkohol be poured into a cryſtal decanter at the bottom of which is a ſmall quantity of pure water, a very ſingular phenomenon is obſervable. At the moment when the phoſphorated alkohol comes into contact with the water, a brilliant light is diſengaged, which paſſes with a ſerpentine motion, and rapid, crackling along the ſurface of the water. The air within the veſſel then becomes entirely luminous, and remains for ſome time in this ſtate. If the bottle be uncloſed, and air blown into it; the white vapours diſappear, the air becomes tranſparent, and the phoſphoreſcence diſappears: but if the mixture be again agitated, ſeveral luminous points are ſeen at its ſurface, the air of the bottle becomes again inflamed; and this may be renewed until the whole of the phoſphorated alkohol has been conſumed.

If a pen be dipped in water, and in this ſtate it be plunged in phoſphorated alkohol; at the moment of contact of the two fluids a ſudden light ſpreads through the air, and very perceptible flames are ſeen, in a dark place, to iſſue from time to time out of the mouth of the phial.

EXP. XIII. *Water is not the only Subſtance which ſets fire to Phoſphorated Alkohol.*

I HAVE obſerved that the phenomenon of the inflammation of phoſphorated alkohol not only takes place with cold and with boiling water, but alſo with concentrated ſulphuric acid, with the ſolution of pot-aſh, with the liquid nitrate of lime, and with the ſolution of various alkaline ſalts.

EXP. XIV. *Sulphuric Ether diſſolves Phoſphorus.*

I LEFT, during ſixteen days, in a well cloſed cryſtal decanter, two cylinders of good phoſphorus, with ſome ounces of ſulphuric ether.

This liquid did not appear to have loſt its uſual ſmell, as was the caſe with alkohol. It was very tranſparent; and, upon being decanted into another bottle and agitated in obſcurity, there were no ſigns of phoſphoreſcence. I afterwards poured ſome drops into water which I had put in another bottle; but here alſo I obſerved no appearance of light, though the place of operation was perfectly dark.

The phoſphorated ether was ſeen in the light floating upon the water, and very limpid. I added a ſmall quantity of alkohol to this mixture, which immediately rendered it turbid like milk. This phenomenon may afford a ſure means of aſcertaining the purity of ether. The mixture agitated in every direction afforded no light; but light appeared with much vivacity and at ſeveral repetitions after the mixture had been a little heated. A great number of luminous points appeared on the ſurface of the water after it had been long agitated. I made this experiment in a bottle of a certain ſize, cloſing and uncloſing it ſometimes in order that the white vapours might iſſue out, in proportion as they were formed.

The following are the conſequences which I have deduced from the foregoing experiments:

1. Phoſphorus is very readily diſſolved in pure oxygene gas at a mean temperature *(Exp. I.)* without emitting light; and the phoſphorated oxygene gas ſhines when it is diffuſed in any other azotic or mephitic gas. *(Exp. II. and III.)* This phenomenon appears to me to depend on the agitation which the integral parts of the oxygene gas and phoſphorus undergo when another gas is mixed with them, and from the diminution of affinity between the integral parts even of the oxygene gas, effected by the addition of the new gas. By this diminution of affinity, if phoſphorus is placed in the ſphere of chemical attraction with the oxygene, the oxygene gas becomes decompoſed, and the phoſphorus burns.

2. Phoſphorus is likewiſe ſpeedily diſſolved in inflammable gas *(Exp. IV.)* and this phoſphorated gas burns when it is brought into contact with any oxygene gas.

3. The oxygenated muriatic acid gas ſuddenly burns phoſphorus. The oxygene gas which contains this acid is decompoſed by the phoſphorus, which rapidly combines with the oxygene *(Exp. V. VI. VII.)* to form phoſphoric acid; and the concrete caloric, or other component principles of the oxygenated muriatic acid gas, being ſet at liberty, raiſes the temperature, and melts the phoſphorus which is not yet oxygenated.

4. Phoſphorus is not ſoluble in pure carbonic acid gas *(Exp. VIII.)*; nevertheleſs, at the temperature of 12 degrees above 0 of the thermometer of Reaumur, it burns better in this gas mixed with a ſmall portion of oxygene gas, than when in atmoſpheric air at the ſame temperature.

5. Water does not diſſolve phoſphorus, but it holds very ſmall particles in a ſtate of ſuſpenſion, more eſpecially thoſe which are impregnated with a principle of combuſtion,

and have been fuſed by caloric (*Exp. V. VI. and VIII.*). The air of the atmoſphere diſſolves phoſphorus at the moment it burns it, and the ſolution is ſenſibly phoſphoreſcent when the experiment is made in a cryſtal glaſs bottle. This cauſes a flame more or leſs brilliant, in proportion to the greater or leſs quantity of phoſphorus which is burned at the ſame time.

6. Phoſphorus which is burned in a decanter with water at the bottom, not only decompoſes the oxygene of the air in which it burns, but likewiſe that of the air which is found mixed with water (*Exp. X.*).

7. Oil of turpentine and oil of thyme, containing phoſphorus, do not burn in contact with atmoſpheric air at the ordinary temperature (*Exp. XI.*).

8. Alkohol diſſolves phoſphorus, and loſes its agreeable ſmell. Phoſphorus ſhines in the atmoſpheric air of cloſed veſſels, by pouring phoſphorated alkohol upon ſome ſubſtance which ſhall combine with the liquid and ſeparate the phoſphorus; ſuch are water, ſulphuric acid, &c. (*Exp. XII. XIII. XIV.*)

Sulphuric ether likewiſe diſſolves phoſphorus, and this ſolution is not decompoſed by water, upon which it floats; but when phoſphorated ether is diluted with alkohol, or is changed into anodyne liquor, it then combines with water, and quits the phoſphorus, which, being divided into very fine white particles, gives the mixture the colour of milk.

IV.

On the Advantage of inverting the Slider in many Operations on the Common Sliding Rule. By the Rev. W. PEARSON, *of Lincoln.*

To Mr. NICHOLSON, *Author of the "Journal of Natural Philoſophy," &c.*

SIR,

ON peruſing the eighth number of your Journal, I was much pleaſed with the ingenious inſtruments mentioned in the VIIIth article, upon which the graduations of Gunter's Line of Numbers are ſo diſpoſed as to be very conſiderably enlarged, whilſt yet the whole range of a ſingle line is contained in a ſmall compaſs. If an inſtrument of either the parallel, circular, or ſpiral conſtruction, of a convenient ſize, were manufactured for ſale, with a proper degree of accuracy, I am fully perſuaded that conſiderable advantage would be derived therefrom by purchaſers; particularly by thoſe perſons whoſe profeſſion is to make practical calculations. But as the public are in poſſeſſion of a ſliding rule, in the operations of which long uſage has made every mechanic expert, it is to be apprehended that prejudice would in this, as in other inſtances, prove an obſtacle to the general ſale of ſuch an inſtrument, were the introduction of one into general uſe attempted. This obſervation is, I believe, verified by the ſlow ſale of the patent, and alſo of the new improved ſliding rule, in compariſon of that of the common one, amongſt artificers, who conſtitute the moſt numerous claſs of purchaſers. I have been informed by a reputable tradeſman, who retails ſliding rules of the different conſtructions, that for every patent, or new improved ſliding rule that is purchaſed, more than twenty of Cogſhall's, or the common ſliding rules, are ſold.

fold. By the ſame prejudice, probably, any *new method* of working by the *old rule*, though more expeditious and equally accurate, would be rejected in practice if ſuch method were offered to the public. But, be this as it may, I think the conſideration that the probability is but ſmall of any particular improvement being generally adopted in practice, is not a ſufficient reaſon why that improvement ſhould be withheld from the public eye, whilſt it is conſidered as ſuch. There are always individuals whoſe minds are unfettered by prejudice *, and to whom any hint tending to produce an improvement always proves acceptable.

The logarithmic line of numbers, known by the name of Gunter's line, is ſo well underſtood by every one who is acquainted with the properties of logarithms, as to render any obſervation on the conſtruction of it unneceſſary. The uſe of two ſuch lines, uſually denominated A and B, working together by means of a direct ſlider, ſuperſedes the application of compaſſes, and is taught in moſt practical books of arithmetic.

There is however another, and I think more commodious method of applying the ſlider, than the one generally practiſed (except in working direct proportions), which I have not ſeen explained, nor practiſed by any perſon; and which, therefore, I ſhall beg the favour of you to communicate to the public through the medium of your intereſting Journal, provided you deem it of ſufficient importance. The method I have to communicate lays no claim to ſuperior accuracy when compared to the common method, the powers of the inſtrument being the ſame in both; but, beſides novelty, I truſt it will be found to have facility to recommend it.

The method is ſimply this: Invert the ſlider B on any common ſliding rule, whereby the numerical figures will aſcend on it, and on the fixed line A, in contrary directions: now, as the diſtance from unity to any multiplier, on Gunter's line, will invariably extend from any multiplicand to their product, it follows, that if any particular number on the inverted ſlider B be placed oppoſite to any other given number on A, the product of thoſe numbers will ſtand on the ſlider B, againſt unity on A; for, in any poſition of the inverted ſlider, the diſtance from unity to the multiplier on A, inſtead of being carried forward on B, as when the ſlider is in a direct poſition, is brought back thereby to unity again; ſo that unity (or *ten* on ſingle lines where the ſlider is too ſhort for the operation) is invariably the index for the product of any two coincident numbers throughout the lines.

In diviſion, by the ſame proceſs, if the dividend on B be put to the index, or unity on A, the diviſion and quotient will coincide on the two oppoſite lines; ſo that when one is given, and ſought for on either line, the other is ſeen on its oppoſite line at the ſame time.

---

* This prejudice is the effect of habit, and can ſeldom be eradicated from the minds of ſuch individuals as conſider the ready occurrence of a propoſition as a teſt of its truth. To eſtabliſh a new philoſophical theory has, in every inſtance, required time ſufficient to educate an entire generation of men. The rejection of the Ariſtotelian philoſophy, the adoption of experimental reſearch, the ſubſtitution of the doctrine of gravitation inſtead of that of vortices, and the rejection of phlogiſton by our contemporaries, are ſufficiently illuſtrative of this aſſertion. New practices are ſtill more difficult to be introduced. The new grammar, the new rudiments of ſcience, or the new inſtrument, however ſuperior to the old in ſimplicity, facility, and truth, muſt be leſs valuable to the ordinary teacher or artiſan, whoſe memory is familiarized with the precepts of the latter, and whoſe only ambition is to earn his ſubſiſtence with the leaſt poſſible exertion. N.

The next operation which offers itſelf here is reciprocal proportion, which can be effected by no other method than by inverting the ſlider, but which is rendered as eaſy by this application, as direct proportion is in the common way; for if any antecedent number on B inverted be ſet to its conſequent on A, any other antecedent on B, in the ſame poſition, will ſtand againſt its conſequent on A, ſo as that the terms may be in a reciprocal ratio. In ſquaring any number, it will appear from what has been already ſaid, that if the number to be ſquared be placed on B, inverted againſt the ſame on A, the ſquare will ſtand on B, againſt unity on A. Therefore, to extract the ſquare root of any number, let that number on B ſtand againſt unity on A; and then wherever the coincident numbers are both of the ſame value, that point indicates the root. If two dividing lines of the ſame value do not exactly coincide, the coincident point will be at the middle of the ſpace contained between thoſe two which are neareſt a coincidence; and as there is only one ſuch point, there can be no miſtake in readily aſcertaining it. The finding of a mean proportional between any two numbers is extremely eaſy at one operation; for if one of the numbers on B inverted be ſet to the other on A, the coincident point of two ſimilar numbers ſhews either of thoſe to be the mean, or ſquare root of their product, according to the preceding proceſs. Thus have we a ſhort and eaſy method of multiplying, dividing, working reciprocal proportion, ſquaring and extracting the ſquare root, at one poſition of the inverted ſlider, whereby the eye is directed to only one point of view for the reſult, after the ſlider is fixed: whereas, by the common method of extracting the ſquare root by A and B direct, the ſlider requires to be moved backwards and forwards by adjuſtment, the eye moving alternately to two points, till ſimilar numbers ſtand, one on B againſt unity on A, and the other on A againſt the ſquare number on B; which ſquare number, in the caſe of finding a mean proportional, muſt be found by a previous operation. Hence, for more convenience in the extraction of roots, and meaſuring of ſolids, an additional line called D, has been added to the rule, which renders it more complex, and conſequently ſeldom underſtood by an artificer. Upon examining the patent rule with a graduated piece of braſs, to be attached to, or detached from the ſlide at pleaſure; and alſo Mr. Horton's new improved rule with two ſliders; I perceive that the method of multiplying length, breadth, and depth into one another at one poſition, in which their excellence conſiſts, is nothing more than a combination of the two methods on the common rule; for the product of the two firſt numbers, effected by means of an inverted and a direct line, forms the multiplier to be uſed with the third number, by a third direct line, at the ſame poſition of the ſliders. Hence, a gauger's rule will anſwer the ſame purpoſe, if an additional ſlider be held in a proper ſituation inverted. No reaſon, however, has been given for the effect of this proceſs in any book that I have yet ſeen, nor of the application of an inverted ſlider to any other ſpecies of calculation *.

---

* In reply to the obliging poſtſcript of the author of this paper, I have only to remark, that the approbation of the intelligent cultivators of ſcience is the moſt eſtimable reward of my endeavours to diffuſe knowledge; and that inclination, as well as duty, will induce me to pay every attention to his future communications. N.

V.

Abſtract of a Memoir entitled " Enquiries concerning the Nature of Pruſſian Blue"

By Mr. PROUST *.

IF iron were ſuſceptible, as chemiſts imagine, of uniting with oxygene in all the intermediate proportions between $\frac{27}{100}$ and $\frac{48}{100}$, which appear to be the two extreme terms of its union with that principle, ought it not to afford, with any given acid, as many different combinations as it is ſuppoſed capable of producing oxides? Why, for example, does not this metal, which affords, with the ſulphuric acid, a ſalt conſtant in its properties when oxided no farther than $\frac{27}{100}$, exhibit different combinations, equally conſtant in their reſpective properties, when it contains $\frac{34}{100}$, $\frac{38}{100}$, or $\frac{45}{100}$ of oxygene?

A great number of facts prove, on the contrary, that iron does not reſt indifferently at all the different degrees of oxidation between the two terms abovementioned; and, notwithſtanding the different degrees of oxygenation through which iron is ſuppoſed to paſs when its ſulphate is expoſed to the air, we are acquainted with no more than two ſulphates of this metal.

The firſt is the green, or cryſtallizable ſulphate, in which Lavoiſier has proved that the iron contains $\frac{27}{100}$ of oxygene. This ſalt, when pure, is inſoluble in ſpirit of wine; its ſolution in water is of a very pale ſea green colour. It is not altered by the gallic acid, affords no blue with alkaline pruſſiates, &c.

The ſecond kind of ſulphate, no leſs conſtant in its properties, is that red deliqueſcent incryſtallizable combination which is ſoluble in alkohol, and is known by the name of mother water of vitriol; but is not really ſuch, unleſs when it produces no alteration in the oxygenated muriatic acid; that is to ſay, when its oxide contains $\frac{8}{100}$ of oxygene.

This ſulphate is eaſily obtained by treating it with nitric acid until its ſolution no longer diſengages nitrous gas, by the addition of a new quantity of acid.

This ſulphate, beſides, poſſeſſes excluſively the property of forming a black precipitate with the gallic acid, and of affording Pruſſian blue with the alkaline pruſſiates, as will hereafter appear.

Between theſe two ſulphates, which Mr. Prouſt calls the green ſulphate and the red ſulphate, there is no intermediate point. If the green ſulphates, by expoſure to the contact of air, aſſume a colour which appears to belong to neither of theſe ſpecies, it may be decidedly ſhown that they are ſimply a mixture of the two, by ſeparation by means of alkohol. Each ſalt will then exhibit all its diſtinctive properties. The green ſulphate will conſtantly produce a green precipitate with the cauſtic alkalis; which precipitate will ſoon become black if preſerved under water without the contact of the air, becauſe its particles continually approach each other, and render the colour more and more intenſe.

The red ſulphate, on the contrary, will afford, with the ſame alkalis, a yellow red precipitate, which is an oxide, incapable of attracting any thing from the air of the atmoſphere, nor from the oxygenated muriatic acid.

---

* This abſtract is tranſlated from the Annales de Chimie XXIII. 85. The editors do not ſay where the original is to be found.

This oxide, according to the experiments of Mr. Prouſt, contains $\frac{48}{100}$ of oxygene, and is capable of forming the baſis of a ſeries of combinations, which bear the ſame relation to thoſe which afford the black oxide, as the red ſulphate of iron bears to the green ſulphate.

So that in whatever degree of alteration an ordinary ſulphate may be examined, it will be found to conſiſt merely of theſe two ſalts, mixed in different proportions.

Whence it follows, that a ſulphuric or muriatic ſolution of this metallic ſubſtance is nothing but a mixture of the two ſalts, one of which has for its baſis the oxide of iron, containing $\frac{27}{100}$ of oxygene, and the other the ſame oxide, containing $\frac{48}{100}$ of oxygene. And as little attention has hitherto been paid to the diſtinctive properties of theſe ſalts; effects have frequently been attributed to the one, which belong excluſively to the other.

We muſt therefore diſtinguiſh two ſulphates, two muriates, two arſeniates, two pruſſiates, &c.: the latter ſalts more particularly conſtitute the object of Mr. Prouſt's Memoir.

The White Pruſſiate.

IN order to obtain this ſalt, a ſolution of the perfectly pure green ſulphate muſt be taken and mixed with a well ſaturated ſolution of the pruſſiate of pot-aſh. This laſt ſolution, if perfect, affords cryſtals of a beautiful orange-yellow in tetrahedral pyramids, truncated near their baſe.

The green ſulphate of iron is obtained and preſerved in a ſtate of purity, by keeping it in a well cloſed veſſel quite full, in which a ſmall piece of iron or tin is put. The ſame purpoſe is anſwered by reducing the red oxide, which may exiſt in the ſolution, to the ſtate of black oxide, by a mixture of water charged with ſulphurated hydrogene gas. This ſulphate, after purification, ought not to change by the addition of the gallic acid. In this ſtate, its colour is an extremely weak ſhade of ſea-green. The green colour does not appear, unleſs the ſolution be in contact with the atmoſpheric air.

After having poured the ſolution of the pruſſiate upon the ſulphate of iron, the bottle muſt be immediately cloſed. A plentiful white precipitate immediately falls down, which ſoon afterwards aſſumes a green tint, occaſioned by the air contained in the bottle, and likewiſe by another cauſe. But if the veſſel remain cloſed this ſhade does not become deeper, and light alone does not produce any change.

It is adviſable to pour an exceſs of the pruſſiate upon the ſulphate, in order to complete the decompoſition. After ſeveral hours repoſe, this pale pruſſiate is covered with a yellow liquor, which is a mixture of the pruſſiate and the ſulphate of pot-aſh. This liquor retains in ſolution a ſmall quantity of white pruſſiate, which acquires a blue colour by abſorption of oxygene, when it is placed in contact with the atmoſpheric air. By this union, it becomes inſoluble, and falls upon that which is at the bottom. The laſt-mentioned precipitate receiving in its turn the impreſſion of the atmoſphere, ſoon becomes blue at its ſurface. The intenſity of colour ſucceſſively increaſes, and proceeds downwards, until at length the white pruſſiate becomes converted into Pruſſian blue.

The action of the oxygene of the atmoſphere, in theſe circumſtances, is ſtill more confirmed by the phenomena which are ſeen when the white pruſſiate is expoſed to the action of the air on a filter. It becomes blue by abſorbing oxygene; and the colour does not acquire its greateſt intenſity until the oxide of iron has obtained $\frac{48}{100}$ of that principle.

Of all the ſaline combinations of iron, there is none of which the baſis becomes ſaturated with oxygene ſo rapidly as the pruſſiate. The carbonate of iron is not to be compared with it.

The ſulphuric and muriatic acids poured on the white pruſſiate of iron occaſion no change. The oxygenated muriatic acid, on the contrary, enlivens the pruſſiate in an inſtant, and loſes its odour. The nitric acid likewiſe converts it to blue, but more ſlowly, becauſe it does not abandon its oxygene with the ſame facility as the oxygenated muriatic acid.

The ſolution of ſulphurated hydrogene, which does not alter the green ſulphate, as has already been remarked, has likewiſe no action on the white pruſſiate. It merely reduces to white the ſmall quantity of pruſſiate which had become blue by the acceſs of air; or, in other words, it deprives it of that portion of oxygene which, it may already be ſeen, conſtitutes the difference between the white and the blue pruſſiates.

It may perhaps be an anticipation of the facts, to diſtinguiſh that pruſſiate by the name of white, which in ſtrictneſs is ſuch only at the moment of its formation, and afterwards becomes covered with a greeniſh white; but it is very probable that it acquires this ſhade merely from the air contained in the veſſel, and in the liquids made uſe of. There exiſts likewiſe another cauſe in the ſmall portion of red oxide, from which the alkaline and even the calcareous pruſſiates derive their yellow colour. This oxide becomes converted into blue pruſſiate, and mixes with the white pruſſiate at the moment wherein the pruſſic acid, being diſengaged from the alkalis, is at liberty to unite with the different oxides it meets with. Theſe oxides are, as we have ſeen, a great quantity of the black oxide mixed with the red oxide of the alkaline pruſſiate. It muſt therefore neceſſarily follow, that a ſmall portion of the blue pruſſiate will be mixed with a great quantity of the white pruſſiate; whence this laſt acquires its greeniſh tinge.

From the whole of theſe facts, it is evident that the oxide, which conſtitutes the baſe of the white pruſſiate, poſſeſſes the ſame degree of oxidation as when it formed the baſis of the green ſulphate. This oxide in effect paſſes from the ſulphate to the white pruſſiate, without the interference of any cauſe which can either increaſe or diminiſh the proportion of oxygene. Hence it follows, that ſince the alkalis ſeparate the oxide of the green ſulphate of a graſs-green colour, they ought alſo to ſeparate it from the white pruſſiate under the ſame tinge.

This in fact happens at the inſtant when the cauſtic alkali, or ammoniac, is poured on the white pruſſiate. But in order to judge better of the ſhades, it is adviſable to uſe theſe ſolutions diluted with water. The green oxide is not completely deprived of pruſſic acid till after the repeated application of alkalis. This may be ſhewn by taking up the green oxide, by an acid which does not diſſolve the white undecompoſed pruſſiate.

The Blue Pruſſiate.

AFTER what has been ſaid, it is eaſy to foretel the habitudes of the nitrate, and all the ſolutions of which the oxide is at its maximum of oxidation, when alkaline pruſſiates are applied. No interval is perceived between the precipitation and the moſt lively blue. The colour is perfect at the inſtant of precipitation, and expoſure to the atmoſphere adds nothing to its intenſity.

Pruſſian blue is, in a word, that pruſſiate, whoſe baſis contains $\frac{48}{100}$ of oxygene. It is the ſame with reſpect to the white pruſſiate as the red ſulphate is to the green. These two pruſſiates do not differ with regard to their acid. Their diſtinctive characters ariſe from the different oxidation of their baſes.

The blue pruſſiate is not altered by acids. The oxygenated muriatic acid changes it, by rendering it green, at the ſame time that itſelf undergoes alteration, as Berthollet has diſcovered. But the action of this deſtructive acid is exerciſed on the pruſſic acid, and not on the oxide, which can receive no greater doſe of oxygene than it has already acquired from the nitric acid, the air, &c.

The acids which are uſed to brighten ſuch Pruſſian blues as are imperfect, are of no other uſe than to diſſolve the great quantity of carbonate of iron, precipitated by the pot-aſh which is not ſaturated with pruſſic acid, and ſuper-abounds in ill-prepared lixiviums. If white pruſſiate were to exiſt in ſuch precipitates, it would be in no reſpect changed by the acid, and would acquire a blue colour only by abſorbing from the atmoſpheric air the quantity of oxygene neceſſary for that purpoſe.

In order to ſhew that oxygene is the principle which in the blue pruſſiate of iron affords the diſtinctive colour, nothing more is neceſſary than to remark the colour of the oxide when precipitated by alkalis. In the green ſulphate the precipitate was black; after the precipitation from Pruſſian blue it is red. No other principle but oxygene could have occaſioned this difference in theſe oxides.

Mr. Prouſt having ſpoken indifferently of the yellow and the red oxide of iron, as being completely ſaturated with oxygene, obſerves, that he uſes theſe expreſſions without diſtinction, becauſe a number of facts have convinced him that there is no difference between them. Every red oxide, ſays he, when diſſolved in any acid whatever, is precipitated of a yellow colour by alkalis, whether pure or ſaturated with carbonic acid. This laſt acid occaſions no difference in the precipitate, becauſe it has no tendency to unite with iron at that degree of oxidation. The red oxides when dried are brown, obſcure and often black, according to the degree of dryneſs or denſity they may have acquired. But if they be pounded in a mortar, the characteriſtic colour ſoon appears. It is affirmed, that theſe oxides have the power of decompoſing ammoniac; but our author kept them for ſeveral years in ammoniac, without obſerving that they were in any reſpect changed. He had no better ſucceſs with the oxide of mangane̓ſe at the ordinary temperature of the air.

The ſolution of ſulphurated hydrogene gas, when kept in a veſſel with the blue pruſſiate, is decompoſed. It ſeizes from the oxide that portion of oxygene which conſtitutes the difference between the blue and the white pruſſiates; and the pruſſiate thus rendered white exhibits the ſame phenomena with alkalis, as if it had been obtained immediately from the green ſulphate. The white pruſſiate kept under hepatic water undergoes no alteration; in which reſpect it reſembles the green ſulphate. Either of theſe readily yield to the hydrogene diſſolved in this water all the oxygene they poſſeſs, exceeding 27 per cent.

The ſame theory explains, why the red ſulphate and the nitrate of iron decompoſe ſulphurated hydrogene. The oxide of iron burns the hydrogene, the ſulphur is depoſited, and the ſupernatant liquor, inſtead of affording a red precipitate by alkalis, exhibits a green.

This obſervation points out a method of reſtoring the copperas of the ſhops to the ſtate of green ſulphate. When brown depoſitions are formed, it contains copper.

The ſolution of ſulphurated hydrogene is not the only practicable means to reduce the blue pruſſiate to the ſtate of white pruſſiate. The requiſite abſtraction of oxygene is made by keeping Pruſſian blue with water and plates of iron or tin in a well cloſed veſſel. By this treatment it acquires all the properties of the white pruſſiate.

The diſtribution of oxygene between a metal and its oxide is not an uncommon fact in chemiſtry. By keeping a red ſulphate or muriate with iron, they are enabled to recover their firſt ſtate. Mercury kept in a ſolution of corroſive ſublimate becomes changed, as does likewiſe the metallic ſalt itſelf, into the mild muriate of calomel. Mercury undergoes the ſame change in a ſolution of the red muriate of iron, but is not altered in a ſolution of the green muriate of the ſame metal. In the red ſulphate, the mercury is converted into that kind of ſulphate which does not become yellow by the affuſion of water, that is to ſay, in which the oxide is at its minimum. Many other facts of the ſame kind might be mentioned.

It has been aſſerted in this paper, that the oxygenated muriatic acid does not act upon the oxide of Pruſſian blue. The following affords a proof of the truth of this aſſertion: All the known red oxides, whether natural or artificial, colcothar, the iron ore of Elba*, undergo no alteration in this acid; but the native brown oxides, which for the moſt part are mixtures of the black and red oxides, are affected by ſuch treatment.

It is found by means of the oxygenated muriatic acid, that the oxides of the nitrate, the acetite and the muriate of lead are not at their maximum of oxidation. All theſe ſalts, when kept beneath this acid, are decompoſed. A brown or puce-coloured oxide is ſoon depoſited, and even cryſtallized round the ſides of the veſſels: and the nitric acid no longer acts upon the new oxide. In proceſs of time, however, that acid aſſumes a fine roſe colour, bubbles of air riſe from the mixture at the bottom, and at length the nitrate is produced, as ſoon as the oxide, continually ſolicited to union by the nitric acid, has parted with the doſe of oxygene which oppoſed that union.

The muriatic acid does not diſſolve ſuper-oxygenated lead without producing at the ſame time abundance of the oxygenated acid; but in order to procure this oxide in greater quantity, it is only neceſſary to treat the minium of commerce with a weak nitric acid, which ſeparates 13 or 14 per cent. of brown oxide, diſcovered, as is well known, by Scheele. The red lead of Siberia is nothing elſe, as Mr. Macquart has ſhewn, but a natural ſuper-oxidation of this metal. It would be intereſting to know, whether by carrying the calcination beyond the point which affords minium, it might not be poſſible to render the oxide brown. Such a proceſs might perhaps ſupply the want of mangaueſe, for preparing the oxygenated muriatic acid.

Mr. Prouſt defers to a future communication an account of the nature of lead, oxided in a leſs degree than conſtitutes the baſis of the nitrate of this metal.

From the foregoing facts Mr. Prouſt draws the following concluſions:

The oxide which alkalis ſeparate from Pruſſian blue is red, though it originally was black in the green ſulphate which afforded this blue.

---

* The ore of Elba often contains ſulphate of iron. It may be extracted by treatment with nitric acid, and ſubſequent precipitation with ammoniac or cauſtic pot-aſh.

The white pruſſiate is a ſalt which is affected by atmoſpheric air in no other manner than the green ſulphates, muriates and carbonates, or, in a word, moſt of the ſaline combinations which contain iron oxided to the minimum. There is no other difference with reſpect to their ſuper-oxidation, but the greater or leſs time theſe metallic ſalts demand; for he obſerved that the muriates and phoſphates of which the oxide is at the minimum are not perceptibly altered by expoſure to the air.

The pruſſiate of iron is not the only combination of this metal which owes its blue colour to atmoſpheric oxygene. That which is called native Pruſſian blue is merely the phoſphate of iron oxided to a certain point. Mr. Prouſt intends to ſhew artificial phoſphates which are grey, blue, and white, according to the degree of oxidation. We are at preſent well acquainted, adds the author, with the cauſe of thoſe dull greeniſh tinges which frequently appear in the newly-made pruſſiates. It often happens that they are not brightened well by acids, and do not acquire their peculiar lively colour but by expoſure to the air. This fact is well known to manufacturers, who accordingly take care not to waſte their acid in attempting to render ſuch blues perfect.

It has been obſerved, that the pure green ſulphate does not afford a black with the gallic acid. This is very true of it at the firſt moment. But the contact of air ſoon colours the mixture at its ſurface. A few drops of the oxygenated muriatic acid immediately produce the ſame effects throughout the fluid. We ſee therefore that iron does not form ink with the gallic acid, but in proportion as it is oxided. This black colour may likewiſe be deſtroyed by including the black mixture in a bottle with a certain quantity of hepatic water.

From theſe facts it alſo very evidently appears why it is the eſtabliſhed practice to expoſe ſtuffs to the air after the black dye; and why ink newly made and pale becomes black very ſpeedily after it is ſpread upon paper, &c. For in all theſe mixtures the ſulphate of commerce is uſed, which contains little of the red ſulphate, and much of the green. When the gallic acid is poured into ſolutions of the ſulphate and muriate, containing the red calx of iron, ink is inſtantly produced. The baſis of ink and of every black dye is therefore merely the gallate of iron, of which the iron is oxided to the maximum. Laſtly, we cannot but obſerve in all theſe facts that chemiſts have hitherto erred with reſpect to the property of the ordinary ſulphate of iron to become black with the acid of galls, to afford a blue with alkaline pruſſiate, &c. Theſe properties belong excluſively to the combinations of which the oxide contains $\frac{48}{100}$ of oxygene, and not $\frac{27}{100}$ only. I ſhall conclude, ſays Mr. Prouſt, by deducing from theſe experiments the principle I have eſtabliſhed at the commencement of this memoir; namely, that iron, like many other metals, is ſubjected to the law of nature, which preſides at every true combination; that is to ſay, that it unites with two conſtant proportions of oxygene. In this reſpect it does not differ from tin, mercury, lead, and, in a word, almoſt every known combuſtible. He intends ſhortly to explain the nature of that kind of oxide which reſults from the union of oxygene with carbone, in a leſs proportion than is required to conſtitute the carbonic acid.

VI.

An Account of ſome Experiments to determine the Force of Fired Gunpowder. By BENJAMIN *Count of* RUMFORD, *F. R. S. M. R. I. A.**

AFTER obſerving that no human invention, except perhaps the art of printing, appears to have produced ſuch important changes in civil ſociety as the invention of gunpowder; that notwithſtanding the extenſive uſes to which this wonderful agent is applied, it ſeems not hitherto to have been examined with the attention it merits, and that probably this want of inveſtigation may have ariſen from the danger attending the experiments; our author proceeds to relate his own experiments and obſervations.

Several eminent philoſophers and mathematicians have, from time to time, directed their attention to this ſubject. The modern improvements in chemiſtry have greatly elucidated the cauſe and circumſtances attending the exploſion of gunpowder. But the great deſideratum, namely, the real meaſure of the initial expanſive force of this agent, has not yet been determined. Robins, from his experiments, concluded that the elaſtic force of the fluid generated in the combuſtion of gunpowder, is one thouſand times greater than the mean preſſure of the atmoſphere; but the celebrated Daniel Bernouilli determines its force to be not leſs than ten thouſand times the ſame mean preſſure. Count Rumford, being ſtruck with the great difference in theſe reſults, has occaſionally, for many years, endeavoured to aſcertain the truth by experiment. In a paper printed in the year 1781, in the Tranſactions, he gave an account of an experiment, No. 92, by which it appeared that, calculating even upon Mr. Robins's own principle, the force of gunpowder muſt be at leaſt 1308 times greater than the mean preſſure of the atmoſphere; and from that, and many other experiments, he became convinced that it was neceſſary to abandon the methods of that philoſopher, and make others with a very different apparatus.

His firſt attempts were to fire gunpowder in a confined ſpace, thinking that when he had accompliſhed this he ſhould find means of meaſuring its elaſtic force without difficulty. A very ſtrong ſhort gun-barrel was prepared, and attempts made to fire gunpowder in the ſame, by means of a ſmall vent, provided in one inſtance with a valve, and in another experiment lined with gold to prevent corroſion. Theſe attempts were unſucceſsful; the force of the exploſion proving ſufficiently great to enlarge the hole, and ſo rapid as not to allow time for cloſure of the valve. It became neceſſary, therefore, to endeavour to fire the gunpowder by means of heat conveyed through the maſs of the metal itſelf. This was ſucceſsfully performed in a barrel of the beſt forged iron, 3,45 inches long, the diameter of its bore $\frac{7}{10}$ths of an inch, and its ends cloſed up by two ſcrews, each one inch in length, which were firmly and immoveably fixed in their places by ſolder. The vacuity between them in the barrel was conſequently 1,45 inch in length, and conſtituted the chamber of the piece, whoſe capacity was nearly $\frac{6}{10}$ths of a cubic inch. The thickneſs of the metal was equal to the bore of the piece. An hole 0,37 of an inch in diameter was bored through both ſides of the barrel, through the centre of the chamber, and at right angles to its axis. Two tubes of iron, 0,37 of an inch in diameter, the diameter of whoſe bore was $\frac{1}{10}$th of an inch, were firmly fixed in theſe holes with ſolder, in ſuch a manner, that, while their internal openings were exactly oppoſite to each other, and on oppoſite ſides of the chamber, the axes of their bores were in the ſame right line. The ſhorteſt of theſe tubes, which projected 1,3 inch beyond the external ſurface of the barrel, was cloſed at its projecting end; or rather it was not bored quite through its whole length, $\frac{3}{10}$ths of an inch of ſolid metal being left at its end, which was rounded off in the form of a blunt point. The longer tube, which projected 2.7 inches beyond the ſurface of the barrel on the other ſide, and which ſerved for introducing the powder into the chamber, was open; but it could occaſionally be cloſed by a ſtrong ſcrew furniſhed with a collar of oiled leather which was provided for that purpoſe. The method of making uſe of this inſtrument was as follows: The barrel being laid down, or held in a horizontal poſition with the long tube upwards, the charge, which was of the very beſt fine-grained glazed powder, was poured through this tube into the chamber. In doing this, care muſt be taken that the cavity of the ſhort tube be completely filled with powder; and this can beſt be done by pouring in only a ſmall quantity of powder at firſt, and then, by ſtriking the barrel with a hammer, cauſe the powder to deſcend into the ſhort tube. When, by introducing a priming-wire through the long tube, it is found that the ſhort tube is full, it ought to be gently preſſed together, or rammed down by means of the priming-wire, in order to prevent its falling back into the chamber, upon moving the barrel out of the horizontal poſition. The ſhort tube being properly filled, the reſt of the charge may be introduced into the chamber, and the end of the long tube cloſed up by its ſcrew.

---

* Abridged from the Philoſophical Tranſactions for 1797, p. 222.

More effectually to prevent the elaſtic fluid generated in the combuſtion of the charge from finding a paſſage to eſcape by this opening after the charge was introduced into the chamber, the cavity of the long tube was filled up with cold tallow, and the ſcrew that cloſed up its end (which was $\frac{1}{2}$ an inch long, and a little more than $\frac{1}{10}$th of an inch in diameter) was preſſed down againſt its leather collar with the utmoſt force. The manner of ſetting fire to the charge was as follows: A block of wrought iron, about $1\frac{1}{2}$ inch ſquare, with a hole in it capable of receiving nearly the whole of that part of the ſhort tube which projects beyond the barrel, being heated red hot, the end of the ſhort tube was introduced into this hole, where it was ſuffered to remain till the heat, having penetrated the tube, ſet fire to the powder it contained, and the inflammation was from thence communicated to the powder in the chamber.

The reſult of this experiment fully anſwered the expectation of the author. The generated elaſtic fluid was ſo completely confined that no part could make its eſcape. The report of the exploſion was ſo very feeble as ſcarcely to be heard. It certainly could not have been heard at the diſtance of 20 paces. It reſembled the noiſe occaſioned by breaking a very ſmall glaſs tube. The quantity of powder made uſe of in this experiment was not more than 1-8th of what the chamber was capable of containing.

The next attempt was to meaſure the force of the elaſtic vapour thus confined. A hole was bored in the axis of one of the ſcrews, or breech pins, which cloſed up the end of the barrel juſt deſcribed, and a piſton of hardened ſteel was fitted into this hole, which was $\frac{2}{10}$ths of an inch in diameter. The end of the piſton, which projected beyond the end of the barrel, was then cauſed to act upon a heavy weight, ſuſpended as a pendulum to a long iron rod. It was hoped that a deduction of the preſſure of the elaſtic vapour might be made from the length of the arc deſcribed by this pendulum; but the experiment was not found to anſwer, though various alterations and improvements were made in the apparatus before the method was abandoned. It was found almoſt impoſſible to prevent the eſcape of the elaſtic fluid by the ſides of the piſton; and the reſults of apparently ſimilar experiments were exceedingly different, and ſo uncertain, that the Count was often at a loſs to conjecture the reaſons of theſe extraordinary variations.

In order to elucidate theſe in a diſcurſive way, the Count proceeds to remark, that Mr. Robins's two aſſumed poſitions, namely, that the operation of gun-powder is performed by the rarefaction of a permanently elaſtic fluid, and that the whole of any charge is ſet on fire before the ball is ſenſibly moved from its place, are not to be admitted. On the contrary, from his own experiments, the Count is diſpoſed to refer the prodigious expanſion of fired gun-powder to the action of water in the ſtate of ſteam at a very elevated temperature; and he ſhews by various obſervations, ſeveral of which muſt be familiar to every one who is habituated to uſe this agent, that the accenſion of gun-powder is ſo far from being performed in an extremely minute portion of time, that it is in every inſtance gradual and progreſſive. But having found it impoſſible to meaſure the elaſtic force of fired gun-powder with any degree of preciſion by the methods already mentioned, he totally changed his plan of operations, and, inſtead of endeavouring to determine its force by cauſing the generated elaſtic fluid to act upon a moveable body through a determinate ſpace, he contrived an apparatus in which this fluid was made to act by a determinate ſurface againſt a weight, which being increaſed at pleaſure ſhould at laſt be ſuch as would be juſt able to confine it, and in that caſe would juſt counterbalance, and conſequently meaſure its elaſtic force.

A ſolid block of very hard ſtone, four feet four inches ſquare, was placed upon a bed of ſolid maſonry, which deſcended ſix feet below the ſurface of the earth. Upon this block of ſtone, which ſerved as a baſe to the whole machinery, was placed the barrel in which the exploſions were made. It was made of hammered iron 2,78 inches long, and 2,82 inches in diameter at its lower extremity, which was flat in order to reſt upon its ſupporter. Its bore was one quarter of an inch in diameter, 2,13 inches long, meaſured from the upper ſurface of the barrel, and it ended in a very narrow opening below, not more than 0,07 of an inch in diameter, and 1,715 inch long, which formed the vent or paſſage by which the fire is communicated to the charge. This paſſage, however, was not open below, but terminated in a projection from the centre of the bottom of the barrel about 0,45 of an inch in diameter, and 1,3 inch long, which formed the vent tube cloſed below. When this barrel is placed upon its ſtand, which is of gun metal, the vent tube paſſes, through a hole, into a cavity in the ſtand which has a lateral opening. Into this lateral opening a ball of red hot iron is introduced, having a proper cavity within for the reception of the vent tube, which it ſpeedily ignites, and ſets fire to the powder through the ſolid ſubſtance of the tube itſelf. The opening of the bore of the barrel, which is placed in the vertical poſition when in uſe, is cloſed by a ſolid hemiſphere of hardened ſteel, whoſe diameter was 1,16 inch, its plain ſide being downwards. It is confined laterally by three upright cylindrical pins, which allow it to riſe only in the vertical direction. Upon this hemiſphere is placed the weight made uſe of for confining the elaſtic fluid generated from the powder in its combuſtion. This weight, which in ſome of the experiments was a heavy twenty-four pounder placed vertically upon its caſcabel, being fixed to certain timbers, was capable of ſliding up and down in a vertical frame, and could be raiſed and lowered in the intervals between the experiments by a ſtrong lever.

The end of the barrel was covered with gold, in order to prevent as much as poſſible its being corroded by the elaſtic vapour, which, when the weight is not heavy enough to confine it, eſcapes between the end of the barrel and the flat ſurface of the hemiſphere; but even this precaution was not found to be ſufficient to defend the apparatus from injury. The ſharp edge of the barrel, at the mouth of the bore, was worn away almoſt immediately; and the flat ſurface of the hemiſphere, notwithſtanding it was of hardened ſteel, and very highly poliſhed, was ſenſibly corroded. The corroſion of the mouth of the bore, by which the dimenſions of the ſurface, upon which the generated elaſtic fluid acted, were rendered very uncertain, would alone have been ſufficient to have rendered all the Count's attempts to determine the force of fired gun-powder abortive, had he not found means to remedy the evil. The method he purſued for this purpoſe was as follows: Having provided ſome pieces of very good, compact ſole-leather, he cauſed them to be beaten upon an anvil with a heavy hammer, to render them ſtill more compact; and then, by means of a machine made for that purpoſe, cylindrical ſtoppers of the ſame diameter preciſely as the bore of the barrel, and 0,13 of an inch in length, (that is to ſay, the thickneſs of the leather) were formed of it; and one of theſe ſtoppers, which had previouſly been greaſed with tallow, being put into the mouth of the piece after the powder had been introduced, and being forced into the bore till its upper end coincided with the end of the barrel, upon the exploſion taking place, this ſtopper (being preſſed on the one ſide by the generated elaſtic fluid, and on the other by the hemiſphere loaded with the whole weight employed to confine the powder) ſo completely cloſed the bore, that when the force of the powder was not ſufficient to raiſe the weight to ſuch a height that the ſtopper was actually blown out of the piece, not a particle of the elaſtic fluid could make its eſcape. And in thoſe caſes in which the weight was actually raiſed, and the generated elaſtic fluid made its eſcape, as it did not corrode the barrel in any other part but juſt at the very extremity of the bore; the experiment by which the weight was aſcertained, which was juſt able to counterbalance the preſſure of the generated elaſtic fluid, was in no wiſe vitiated either by the increaſed diameter of the bore at its extremity, or by any corroſion of the hemiſphere itſelf; for as long as the bore retained its form and its dimenſions in that part to which the efforts of the elaſtic fluid were confined, that is, in that part of the bore immediately in contact with the lower part of the ſtopper, the experiment could not be affected by any imperfection of the bore either above or below.

The powder made uſe of in theſe experiments was of the beſt quality, being that kind called *poudre de chaſſe* by the French, and very fine grained, and it was all taken from the ſame parcel. Care was taken to dry it very thoroughly, and the air of the room in which it was weighed out for uſe was very dry. The weights employed for weighing the powder were German apothecary grains, 104,8 of which make 100 grains troy. The weights employed to confine the elaſtic vapour generated in the combuſtion of the powder, are reduced from Bavarian pounds, in which they were originally expreſſed, to pounds avoirdupois. The meaſures of length were all taken in Engliſh feet and inches. The experiments were all made in the open air, in the court-yard of the arſenal at Munich, and they were all made in fair weather, and between the hours of nine and twelve in the forenoon, and two and five in the afternoon; but the barrel was always charged, and the extremity of the bore cloſed by its leather ſtopper in the room where the powder was weighed. In placing the barrel upon the block of ſtone, great care was taken to put it exactly under the centre of gravity of the weight employed to confine the generated elaſtic vapour. Upon applying the red-hot ball to the vent tube, and fixing it in its place by its lever, which ſupported it, the exploſion very ſoon followed.

When the force of the generated elaſtic vapour was ſufficient to raiſe the weight, the exploſion was attended by a very ſharp and ſurpriſingly loud report; but when the weight was not raiſed, as alſo when it was only a little moved, but not ſufficiently to permit the leather ſtopper to be driven quite out of the bore, and the elaſtic fluid to make its eſcape, the report was ſcarcely audible at the diſtance of a few paces, and did not at all reſemble the report which commonly attends the exploſion of gunpowder. It was more like the noiſe which attends the breaking of a ſmall glaſs tube, than any thing elſe to which it could be compared: In many of the experiments in which the elaſtic vapour was confined, this feeble report attending the exploſion of the powder was immediately followed by another noiſe totally different from it, which appeared to be occaſioned by the falling back of the weight upon the end of the barrel after it had been a little raiſed, but not ſufficiently to permit the leather ſtopper to be driven quite out of the bore. In ſome of theſe experiments a very ſmall part only of the generated elaſtic fluid made its eſcape; in theſe caſes the report was of a peculiar kind; and though perfectly audible at ſome conſiderable diſtance, yet not at all reſembling the report of a muſket. It was rather a very ſtrong ſudden hiſſing, than a clear, diſtinct, and ſharp report.

Though it could be determined with the utmoſt certainty by the report of the exploſion, whether any part of the generated elaſtic fluid had made its eſcape; yet for ſtill greater precaution, a light collar of very clean cotton wool was placed round the edge of the ſteel hemiſphere, where it repoſed upon the end of the barrel, which could not fail to indicate, by the black colour it acquired, the eſcape of the elaſtic fluid, whenever it was ſtrong enough to raiſe the weight by which it was confined, ſufficiently to force its way out of the barrel.

Though the end of the barrel at the mouth of the bore was covered with a circular plate of gold, in order the better to defend the mouth of the bore againſt the effects of the corroſive vapour; yet, this plate being damaged in the courſe of the experiments (a piece of it being blown away), the remainder of it was removed, and it was never after thought neceſſary to replace it by another. When this plate of gold was taken away, the length of the barrel was of courſe diminiſhed as much as the thickneſs of this plate amounted to, which was about $\frac{1}{400}$th part of an inch; but in order that even this ſmall diminution of the length of the barrel might have no effect on the reſults of the experiments, its bore was deepened $\frac{1}{400}$th of an inch when this plate was removed, ſo that the capacity of the bore remained the ſame as before.

After making uſe of a great variety of expedients, the beſt and moſt convenient method of cloſing the end of the bore, and defending the flat ſurface of the ſteel hemiſphere from the corroding vapours, was found to be this: Firſt, to cover the end of the bore with a circular plate of thin oiled leather; then to lay upon this a very thin circular plate of hammered braſs, and upon this braſs plate the flat ſurface of the hemiſphere. When the elaſtic fluid made its eſcape, a part of the leather was conſtantly found to have been torn away, but never in more places than one; that is to ſay, always on one ſide only.

What was very remarkable, in all thoſe experiments in which the generated elaſtic vapour was completely confined, was the ſmall degree of expanſive force which this vapour appeared to poſſeſs after it had been ſuffered to remain a few minutes, or even only a few ſeconds, confined in the barrel; for upon raiſing the weight by means of its lever, and ſuffering this vapour to eſcape, inſtead of eſcaping with a loud report, it ruſhed out with a hiſſing noiſe, hardly ſo loud or ſo ſharp as the report of a common air-gun; and its effects againſt the leathern ſtopper, by which it aſſiſted in raiſing the weight, were ſo very feeble as not to be ſenſible. Upon examining the barrel, however, this diminution of the force of the generated elaſtic fluid was eaſily explained; for what the Count thinks was undoubtedly in the moment of the exploſion in the form of an elaſtic fluid, was now found transformed into a ſolid body as hard as a ſtone. It may eaſily be imagined how much this unexpected appearance excited his curioſity; but, intent on the proſecution of the main deſign of theſe experiments, the aſcertaining the force of fired gun-powder, he was determined not to permit himſelf to be enticed away from it by any extraordinary or unexpected appearances or accidental diſcoveries, however alluring they might be; and faithful to this reſolution, he poſtponed the examination of this curious phenomenon to a future period, and ſince that time he has not found leiſure to engage in it. He thinks it right, however, to mention ſuch curſory obſervations as he was able in the midſt of his other purſuits to make upon this ſubject.

This matter was very hard, and ſo firmly attached to the inſide of the barrel, and particularly to the inſide of the upper part of the vent tube, that it was always neceſſary, in order to remove it, to make uſe of a drill, and frequently to apply a conſiderable degree of force. It was of a black colour, or rather of a dirty grey, which changed to black upon being expoſed to the air, had a pungent acrid alkaline taſte, and ſmelt like liver of ſulphur. It attracted moiſture from the air with great avidity. Being moiſtened with water, and ſpirit of nitre being poured upon it, a ſtrong effervеſcence enſued, attended with a very offenſive and penetrating ſmell. Nearly the whole quantity of matter of which the powder was compoſed ſeemed to have been transformed into this ſubſtance [*]; for the quantity of elaſtic fluid which eſcaped upon removing the weight was very inconſiderable. But this ſubſtance was no longer gun-powder; it was not even inflammable.

What change had it undergone? demands our author,—What could it have loſt? It is very certain the barrel was conſiderably heated in theſe experiments. Was this occaſioned by the caloric, diſengaged from the powder in its combuſtion, making its eſcape through the iron? And is this a proof of the exiſtence of caloric conſidered as a fluid *ſui generis*; and that it actually enters into the compoſition of inflammable bodies, or of pure air, and is neceſſary to their combuſtion? He dares not take upon him to decide upon ſuch important queſtions. He once thought that the heat acquired by a piece of ordnance in being fired aroſe from the vibration or friction of its parts, occaſioned by the violent blow it received in the exploſion of the powder, but acknowledges fairly that it does not ſeem to be poſſible to account in a ſatisfactory manner for the very conſiderable degree of heat which the barrel acquired in theſe experiments, merely on that ſuppoſition.

---

[*] It is much to be regretted that no experiment was made of the weight of this ſubſtance afforded by a given weight of gunpowder. N.

That this hard ſubſtance found in the barrel, after an experiment in which the generated elaſtic vapour had been completely confined, was actually in a fluid or elaſtic ſtate in the moment of the exploſion, is evident, as he thinks, from hence: that in all thoſe caſes in which the weight was raiſed, and the ſtopper blown out of the bore, nothing was found remaining in the barrel. It was very remarkable, that this hard ſubſtance was not found diſtributed about in all parts of the barrel indifferently, but there was always found to be more of it near the middle of the length of the bore than at either of its extremities, and the upper part of the vent tube in particular was always found quite filled with it. It ſhould ſeem therefore, ſays he, that it attached itſelf to thoſe parts of the barrel which were ſooneſt cooled; and hence the reaſon, moſt probably, why none of it was ever found in the lower part of the vent-tube, where it was kept hot by the red-hot ball by which the powder was ſet on fire.

He found by a particular experiment, that the gunpowder made uſe of, when it was well ſhaken together, occupied rather leſs ſpace in any given meaſure than the ſame weight of water; conſequently when gunpowder is fired in a confined ſpace which it fills, the denſity of the generated elaſtic fluid muſt be at leaſt equal to the denſity of water. The real ſpecific gravity of the ſolid grains of gunpowder, determined by weighing them in air and water, is to the ſpecific gravity of water as 1,868 to 1000. But if a meaſure whoſe capacity is one cubic foot hold 1000 ounces of water, the ſame meaſure will hold juſt 1077 ounces of fine-grained gunpowder, ſuch as was uſed in theſe experiments, that is to ſay, when it is well ſhaken together. When it was moderately ſhaken, together its weight was exactly equal to that of an equal volume, or rather meaſure, of water. But it is evident that the weight of any given meaſure of gunpowder muſt depend much upon the forms and ſizes of its grains. He adds one obſervation more relative to the particular appearances which attended the experiments, in which the elaſtic vapour, generated in the combuſtion of gunpowder, was confined, and that is with regard to a curious effect produced upon the inferior flat ſurface of the leathern ſtopper where it was in contact with the generated elaſtic vapour. Upon removing the ſtopper, its lower flat ſurface appeared entirely covered with an extremely white powder, reſembling very light white aſhes, but which almoſt inſtantaneouſly changed to the moſt perfect black colour upon being expoſed to the air.

The ſudden change of colour in this ſubſtance, upon its being expoſed to the air, led him to ſuſpect that the ſolid matter found in the barrel was not originally black, but that it became black merely in conſequence of its being expoſed to the air. The dirty grey colour it appeared to have, immediately on being drilled out of the cavity of the bore where it had fixed itſelf, ſeems to confirm this ſuſpicion. An experiment made with a very ſtrong glaſs barrel would not only decide this queſtion, but would moſt probably render the experiment peculiarly beautiful and intereſting on other accounts.

The Count thinks a barrel of glaſs might be made ſtrong enough for the experiment, if it could withſtand the ſudden heat, and on the whole ſeems diſpoſed to think the trial worth making.

All the parts of the operation being ready, it was in the autumn of the year 1792 that the firſt experiment was made.

The barrel being charged with 10 grains of powder, its contents when quite full amounting to about 28 grains, and the end of the barrel being covered by a circular piece of oiled leather, and the flat ſide of the hemiſphere being laid down upon this leather, and a heavy cannon, a twenty-four pounder, weighing 8081 pounds avoirdupois, being placed upon its caſcabel in a vertical poſition upon this hemiſphere, in order to confine by its weight the generated elaſtic fluid, the heated iron ball was applied to the end of the vent-tube; and after waiting but a very few moments in anxious expectation of the event, Count Rumford had the ſatisfaction of obſerving that the experiment had ſucceeded. The report of the exploſion was extremely feeble, and ſo little reſembling the uſual report of the exploſion of gunpowder, that the by-ſtanders could not be perſuaded that it was any thing more than a cracking of the barrel, occaſioned merely by its being heated by the red-hot ball: yet, as the Count had been taught by the reſult of former experiments not to expect any other report, and as he found, by putting his hand upon the barrel, that it began to be ſenſibly warm, he was ſoon convinced that the powder muſt have taken fire; and after waiting four or five minutes, upon cauſing the weight which reſted upon the hemiſphere to be raiſed, the confined elaſtic vapour ruſhed out of the barrel. Upon removing the barrel and examining it, its bore was found to be choked up by the ſolid ſubſtance already deſcribed, and from which it was with ſome difficulty that it was freed, and rendered fit for another experiment. The extreme feebleneſs of the report of the exploſion, and the ſmall degree of force with which the generated elaſtic fluid ruſhed out of the barrel upon removing the weight which had confined it, had inſpired the aſſiſtants with no very favourable idea of the importance of theſe experiments. It was ſeen indeed from the beginning by their looks, that they thought the precautions to confine ſo inconſiderable a quantity of gunpowder as the barrel could contain, perfectly ridiculous; but the reſult of the following experiment taught them more reſpect for an agent, of whoſe real force they had conceived ſo very inadequate an idea.

In this ſecond experiment, inſtead of 10 grains of powder, the former charge, the barrel was now quite filled with powder, and the ſteel hemiſphere, with its oiled leather under it, was preſſed down upon the end of the barrel by the ſame weight as was employed for that purpoſe in the firſt experiment, namely, a cannon weighing 8081 pounds. The barrel (which, though ſimilar to it in all reſpects, was not the ſame that has already been deſcribed) was made of the beſt hammered iron, and was of uncommon ſtrength. Its length was $2\frac{3}{8}$ inches; and though its diameter was alſo $2\frac{3}{4}$ inches, the diameter of its bore was no more than $\frac{1}{4}$ of an inch, or leſs than the diameter of a common gooſe quill. The length of its bore was 2.15 inches. Its diameter being $2\frac{3}{4}$ inches, and the diameter of its bore only $\frac{1}{4}$ of an inch, the thickneſs of the metal was $1\frac{1}{4}$ inch; or it was 5 times as thick as the diameter of its bore. The charge of powder was extremely ſmall, amounting to but little more than $\frac{1}{16}$th of a cubic inch; not ſo much as would be required to load a ſmall pocket piſtol, and not one-tenth part of the quantity frequently made uſe of for the charge of a common muſket. This inconſiderable quantity of gunpowder, when it was ſet on fire by the application of the red-hot ball to the vent-tube, exploded with ſuch inconceivable force as to burſt the barrel aſunder in which it was confined, notwithſtanding its enormous ſtrength, and with ſuch a loud report as to alarm the whole neighbourhood. The ſpectators turned pale with affright and aſtoniſhment, and it was ſome time before they could recover themſelves. The barrel was not only completely burſt aſunder, but the two halves of it were thrown upon the ground in different directions; one of them fell cloſe by the Count's feet, as he was ſtanding near the machinery to obſerve more accurately the reſult of the experiment.

From ſome former experiments with ſmall iron wire, Count Rumford deduces, that a cylinder of that metal, whoſe tranſverſe ſection is one inch, would be able to ſuſtain 63466 pounds without being broken. But for greater accuracy, he cauſed ſeveral ſmall pieces to be cut out of the ſolid half of the barrel which was broken. From four experiments with pieces whoſe diameters were reſpectively in thouſandth parts of an inch, fifty, ſixty, ſixty-ſix and ſeventy-ſix parts, which were broken by a direct pull, it was found that the medium weight required to break one inch of this iron was in pounds avoirdupois 63173. The variations in the reſult were ſuch as to render the ſecond figure (expreſſing thouſands) uncertain. Our author computes the reſiſtance of the barrel from the area of the ſurface of fracture, namely, 6½ inches, by uſing this as a ſimple multiplier, which gives 410624½ pounds. And this force being conſidered as applied to a ſurface of half an inch, which is the area of a longitudinal ſection of the bore of the barrel, and reduced into atmoſpheres, by allowing 15 pounds avoirdupois for the medium preſſure of the air upon a ſquare inch, gives 54750 atmoſpheres for the meaſure of the force exerted for overcoming ſuch a reſiſtance. This force, enormous as it may appear, is ſuppoſed by the Count to be ſhort of the real initial force of the elaſtic fluid generated in the combuſtion of gunpowder, becauſe he thinks it probable that the barrel was in fact burſt before the generated elaſtic fluid had exerted all its force. On this head of probabilities I would venture to make a remark, that the iron in the barrel may perhaps be conſidered as not exactly in the ſituation of the pieces which our author broke in his engine by a direct pull. For in theſe laſt it may reaſonably be ſuppoſed, that the whole preſſure of coheſion was equally acted on till the moment of fracture; whereas in the barrel the fracture may, from the ſpring of the metal and other circumſtances, be ſuppoſed to have proceeded from the inner to the outer ſurface by a progreſſion or tearing aſunder, which, however ſwift, muſt have rendered the diviſion more eaſy. But at all events, whatever may be the force of this remark, it can in no reſpect invalidate ſuch concluſions as were drawn from the actual lifting of great weights by raiſing the hemiſpherical cover of the barrel.

A ſet of experiments were inſtituted with an apparatus of the kind here deſcribed, for the purpoſe firſt of determining the expanſive force of the elaſtic vapour generated in the combuſtion of gunpowder in its various ſtates of condenſation, and the ratio of its elaſticity to its denſity; and ſecondly, of meaſuring by one deciſive experiment the utmoſt force of this fluid in its moſt denſe ſtate; that is to ſay, when the powder completely fills the ſpace in which it is fired, and in which the generated fluid is confined. A numerous ſeries of experiments are tabulated in the Count's Memoir.

The dimenſions of the barrel made uſe of in thoſe experiments were as follow:

Diameter of the bore at its muzzle = 0,25 of an inch.

Joint capacities of the bore and of its vent-tube excluſive of the ſpace occupied by the leathern ſtopper = 0,08974 of a cubic inch.

Quantity of powder contained by the barrel and its vent-tube when both were quite full, excluſive of the ſpace occupied by the leathern ſtopper, 25,641 German apothecaries grains, = 24½ grains troy.

The Table, in the original, contains a great number of experiments, in which the weight or piece of ordnance was either not raiſed, or was thrown up with a loud report. Thoſe experiments, in which the weight juſt moved without a report, are obviouſly ſuch as indicate the elaſtic force to have been equal to the preſſure. I have accordingly ſelected theſe only in the following table as ſuitable to form the baſis of computations. The day, hour, and minute when each experiment was made, are likewiſe to be found in the original, though it did not ſeem neceſſary to inſert them in this abridgment. The expreſſion of the weight on atmoſpheres is grounded on the aſſumption, that the mean preſſure of the atmoſphere is equal to fifteen pounds avoirdupois upon a ſquare inch.

TABLE I.—*Experiments on the Force of Fired Gunpowder.*

State of the Atmoſphere.		The Charge of Powder.		Weight employed to confine the elaſtic fluid.		State of the Atmoſphere.		The Charge of Powder.		Weight employed to confine the elaſtic fluid.	
Thermometer. F.	Barometer.	German Apot. Grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmoſpheres.	Thermometer. F.	Barometer.	German Apot. grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmoſpheres.
Deg.	Eng. Inch.	Grs.	Parts.	lbs.		Deg.	Eng. Inch.	Grs.	Parts.	lbs.	
37	28,56	6	234	504,8	685,6	32	28,2	10	390	1387,5	1884,3
57	28,37	1	39	57,4	77,86	32	28,2	11	429	1634	2219
34	28,1	2	78	134,2	182,3	36	28,34	12	460	1895,1	2573,7
48	28,31	3	117	212,24	288,2	42	28,3	13	507	2422	3288,3
50	28,36	4	156	281,57	382,4	43	28,31	14	546	2951	4008
48	28,35	5	195	413,27	561,2	43	28,31	15	585	3477	4722,5
59	28,34	7	273	597,66	811,7	70	28,2	16	624	5220	7090
50	28,32	8	312	857,64	1164,8	68	28,3	18	702	8081	10977
50	28,32	9	351	1142,3	1551,3						

In the laſt experiment, with near eleven thouſand atmoſpheres, the weight was raiſed with a very ſharp report, louder than that of a well-loaded muſket; and in the experiment immediately following with the ſame charge, but with the addition of ſix hundred and nineteen pounds to the weight, the vent-tube of the barrel burſt. This experiment was the 85th of the Table, probably the 85th time of heating the tube.

(To be concluded in the next Number.)

VII.

Obſervations and Experiments on Steel, reſembling that of Damaſcus; with an eaſy Teſt for determining the uniform Quality of Steel before it is employed in Works of Delicacy or Expence.

IN the infancy of ſociety the hardeſt bodies, ſuch as ſtones, and certain kinds of wood, were ſelected and uſed for cutting inſtruments, and ſtill are applied to that purpoſe in ſeveral parts of the world. Theſe materials were ſucceeded by copper, hardened by a mixture of tin, of which numerous weapons yet remain in the cabinets of the curious. And laſtly, ſteel, whether obtained directly from the ore, or by cementation of malleable iron, has deſervedly taken place of every other article, on account of the united qualities of tenacity and hardneſs *. When the ſword was the chief weapon of war, it muſt have been an object of grea intereſt and demand to give to its blade a durable keen edge, and a degree of firmneſs or ſtrength, which, without rendering it unwieldy, ſhould enſure the warrior againſt expoſure to the fatal accident of its breaking in the act of combat. The ſabres of Damaſcus have been famous for ages, and ſtill bear a great price in the Eaſt; but we have no decided account of the manner in which this ſteel is manufactured or made up. Some years ago I was favoured with the poſſeſſion of a true blade of this kind for a few days, which, if my recollection be accurate, had coſt the poſſeſſor twelve guineas at Conſtantinople. I know the ſum was not leſs than this. As I was not permitted to make any experiments upon it, I could only ground my proceſs upon reaſoning from its external appearance and obvious qualities.

It had a dull grey or blueiſh appearance, was ſcarcely harder than common ſteel from the forge, was not eaſily bended, and when bended had no ſpring to recover its figure. Its back was ſmooth, as were alſo two narrow ſloped ſurfaces which formed its edge under an angle of about 40 degrees; but its flat ſides were every where covered with minute waving lines in maſſes in all directions, not croſſing each other, and, for the moſt part, running in the direction of its length. The lines were in general as fine as harpſichord wire, not extremely well defined nor continued; and their diſtinction from each other was effected by no perceptible indentation of the ſurface, but rather by the ſucceſſion of parts differing in the degree of poliſh or brightneſs. No one, upon inſpection of this ſurface, would for a moment have imagined or allowed that it could have been done by engraving or etching, as the falſe blades are damaſked. I was informed that if any part of this blade were made ſmooth by grinding or whetting, the wavy appearance, called the water, could be again produced by means of lemon juice; and that its excellencies were, that it could be depended upon not to break, and that it would cut deeper into a ſoft ſubſtance, ſuch as a pack of wool, or into fleſh, than any other kind of blade.

From theſe circumſtances, as well as from the price, I was induced to think that the blade was compoſed of ſteel and iron, and that the proceſs of forging was ſuch as greatly to enhance the coſt, by the labour and management it might require. For if we ſuppoſe the pieces to be united together at the welding heat, and then forged or drawn out, it is certain that no ſmall degree of ſkill and care would be required to render all the parts ſound, and at the ſame time preſerve the ſteel and iron in poſſeſſion of their characteriſtic properties. Too great a heat would probably render the whole maſs more uniform than is conſiſtent with the ſubſequent production of the water or wavy appearance. In my attempt to imitate this ſteel, I endeavoured to ſubſtitute a mechanical contrivance in the place of this ſuppoſed careful forging.

I cauſed a cylindrical hole of about one inch in diameter to be bored through a piece of caſt iron, the lower part of which could be ſo placed upon an anvil as to cloſe one end of the hole. A forged iron plug was made nearly to fit the cylindrical hole, but conſiderably longer. Equal weights of German ſteel and Swediſh iron, both in filings, were then well

---

* Philoſophical Journal, I. 381.

mixed with oil, and wrapped in a paper, which had before been rolled upon the plug, and conſequently fitted the cylinder. The ends of the paper were neatly folded; and the whole maſs being then put into the caſt-iron cylinder placed upon the anvil, a few blows were given by driving the plug into the hole with a heavy hammer. By this means the maſs of filings, when thruſt out of the cylinder, was compact and manageable. It was then placed in a charcoal fire, and urged to a welding heat by the double bellows. Thence it was taken with the tongs; again haſtily put into the cylinder, and hammered by means of the plug and the heavy hammer. When it was taken out, the whole was found to be conſolidated; but upon forging it into a plate, a conſiderable portion flew off in a crumbly form. The plate, however, was filed up, ſmoothed, and examined.

Its colour preſented nothing remarkable. When weak nitrous acid was poured upon it, it became mottled in conſequence of the numerous ſmall black ſpots which appeared upon the particles of ſteel, while thoſe of iron remained clean. On the nitrous acid being waſhed off, the ſurface appeared wavy like the Damaſcus ſteel, but ſcarcely at all fibrous; doubtleſs becauſe the ſolid had not been drawn out by forging. An attempt was made to harden it by ignition and cooling in water; but it ſtill remained ſoft enough to be cut with the graving tool, the point of which did not indicate any difference in that reſpect between the parts of iron and of ſteel, though it is very probable ſuch a difference did really exiſt.

I infer, therefore, that the Damaſcus ſteel is in fact a mechanical mixture of ſteel and iron; that it is incapable of any conſiderable degree of hardneſs, and conſequently is in no danger of breaking from its brittleneſs; that its tenacity is enſured not only from the admixture of iron, but likewiſe from the facility with which its ſoundneſs may be aſcertained throughout, by the ſame proceſs which exhibits the water or fibrous appearance: and, laſtly, that the edge of a weapon formed of this material muſt be rough, on account of the different reſiſtance which the two ſubſtances afford to the grindſtone, in conſequence of which it will operate as a ſaw, and more readily cut through yielding ſubſtances than ſuch cutting tools as are formed of a more uniform ſubſtance.

This experimental enquiry directed my attention to a method of aſcertaining the uniformity of texture in iron or ſteel, which perhaps may have been noticed by others, but is certainly unknown in moſt manufactories, though I have found it of great utility. If a weak acid, for example the nitrous, which I have uſually taken in a very diluted ſtate, be applied to the face of iron or ſteel previouſly cleaned with the file, or with emery paper, the parts which contain the greateſt portion of carburet of iron (or plumbago) immediately ſhew themſelves by their dark colour. It very frequently happens that articles of conſiderable value, intended to be fabricated in iron or ſteel, are not known to be defective until much expence has been laid out in manufacturing them. A piece of iron, which has a vein of ſteel running through it, as is too often the caſe, will require at leaſt three times the labour and care to turn it in the lathe, which would have been demanded by a piece of greater uniformity. Steel which abounds with ſpots, or veins, or ſpecks called pins, may be faſhioned completely, and will not ſhew its defects, until the final operation when the attempt is made to poliſh it. Other articles, ſuch as meaſuring ſcrews, blades of ſheers, fine circular cutters, &c. either bend in the hardening, from the difference of expanſion, or reſiſt the tool when wrought in the tempered ſtate, or exhibit other incurable defects when they come to be tried; which the teſt by nitrous acid would have indicated before any expence had been incurred. In theſe, and in numberleſs other inſtances, it would have been incomparably more advantageous to have rejected the material upon the firſt trial, rather than have proceeded to the very expenſive proceſs of manufacturing the article, and then finding it of no value. By this ſimple expedient I have ſeen bars of ſteel as full of veins and irregularities as wood, and have been enabled to ſelect the beſt and moſt uniform pieces for works of the greateſt delicacy; whereas, before I thought of this mode of trial, I have very often had the mortification to fail in the laſt ſtage of experimental proceſſes, upon which much coſt and labour had been beſtowed.

VIII.

On the Irritability of the Pollen of Plants. With an Account of a Compoſition for cloſing Wide-mouthed Veſſels.

To Mr. NICHOLSON, *Editor of the Philoſophical Journal.*

SIR,

A FEW ſummers ago a friend of mine chanced to make a curious diſcovery relative to the irritability of the pollen or fecundating duſt of plants; which, as I know not that it has yet met the eye of the public, you are at liberty, if thought ſufficiently intereſting, to communicate through the channel of your Journal.

Whilſt making obſervations with one of Adams's compound microſcopes on the figure of the particles of pollen collected from different plants, he applied a drop of water to a ſmall group (which was placed on the glaſs plate of the ſtage), in order to increaſe the bulk; and, by that means, more accurately determine the figure of the particles. Happening, at the ſame time, to have a ſmall phial of ſpirits of wine at hand, he next tried a drop of that liquid to freſh pollen, when he was agreeably ſurpriſed with ſeeing it produce a quick gyration, as well as a darting of the particles backward and forward in the drop: this motion continued for a few minutes, and then gradually ſubſided; the particles unravelling into a continuous filmy thread, and at other times appearing to burſt and emit a multitude of particles * infinitely ſmaller, and which conglomerated together, leaving the capſular veſſel empty, or only filled with the ſpirit.

The ſpeedy evaporation of the ſpirit appearing at times to prevent the completion of the phenomenon, he afterwards tried, and with better ſucceſs, a little common brandy. The motions, in this caſe, were continued longer, and the appearances were more complete. It will appear that theſe do not proceed from the mere evaporation of the liquid, becauſe thoſe particles which have been once ſaturated, though from ſome cauſe they may not have burſt or unravelled, are incapable of excitation, at leaſt in any ſimilar degree, by the application of more ſpirit.

Different kinds of pollen were made uſe of for this experiment; but that of the "Cactus flagelliformis" was moſtly employed, as affording the moſt ſtriking appearance, on account of the magnitude of its particles.

---

* At leaſt it preſented this appearance at times to my eye; which, I doubt not, has been the occaſion of its obtaining from ſome botaniſts the appellation of the ſpermatic aura of pollen.

I avoid entering into any theoretical obſervations on the aforeſaid phenomenon, until more fully illuſtrated by experiment, and content myſelf with merely announcing it to the curious inveſtigators in the paths of philoſophical botany.

I take this opportunity of communicating another method, or at leaſt another compoſition for cementing wide-mouthed veſſels, in addition to thoſe mentioned in your Journal for September. It is a mixture of ſpermaceti and caoutchouc: the former to be melted in a ladle, and the latter added in ſmall bits, which will be gradually but effectually diſſolved, and the compound forms a cement perfectly air-tight when poured on fluid and ſuffered to cool. It is alſo, I believe, very little, if at all, attacked by acids, and on that account might be of conſiderable uſe in philoſophical laboratories. The quantity of caoutchouc may be varied according to the intended purpoſe, or any hardening ſubſtance, as maſtic, added if required. If this latter communication ſhould prove of any uſe to you perſonally, or to your chemical friends, it will amply gratify

Your obliged reader,

Dec. 1ſt, 1797. AMICUS.

IX.

Experimental Reſearches to aſcertain the Nature of the Proceſs by which the Eye adapts itſelf to produce diſtinct Viſion.

[Continued from page 313.]

IN the former part of this article, reſpecting the adjuſtment of the eye, a conſiderable number of original experiments and remarks were related from communications to the Royal Society. Theſe have for their objects the determination, whether the cryſtalline humour by its ſuppoſed muſcularity, the external muſcles by their greater or leſs preſſure upon the orbit, or the cornea by a variation in its curvature, be each the ſole or chief agent in cauſing the pencils of light from viſible objects to arrive in all caſes at thoſe foci which diſtinct viſion requires. I ſhall now proceed to give the ſubſtance of the other Memoirs publiſhed by the Royal Society upon the ſame ſubject

In the volume for 1795, page 263, I find a paper of Obſervations on the Eyes of Birds, by Mr. Pearce Smith. This gentleman relates, that, in the year 1792, he obſerved, while diſſecting the eyes of birds, an irregular appearance of the ſclerotica in that part which ſurrounds the cornea, and which in them is general y flat. On a more minute examination, it appeared to be ſcales lying over each other, and which appeared capable of motion on each other. On inveſtigating this ſtructure, the ſcales were found to be of bony hardneſs, at leaſt much more ſo than any other part of the ſclerotica. On the inſide of the ſclerotic coat there was no appearance of theſe ſcales. Tendinous fibres were detected ſpreading over the ſcales, terminating in the four recti muſcles of the eye, ſo that upon the contraction of thoſe muſcles the ſcales would be moved.

By reflecting on the probable uſes of this conformation, Mr. Smith deduced, that the internal capacity of the eye will be greateſt when the recti muſcles are not in action; and the ſeveral circles of ſcales being ſuffered to repoſe upon each other in ſucceſſion, are upon the whole, nearer the anterior extremity of the optic axis, than at any other time; that when by voluntary effort thoſe muſcles are made to act, the ſeveral circles of ſcales will be drawn back, and diminiſh the periphery of the eye at their reſpective poſitions in this laſt ſituation; and that conſequently the internal capacity of the organ being rendered leſs, the moſt elaſtic part, namely, the cornea, will yield to the preſſure of the included humour, and become more convex. This he concludes muſt render ſmall objects, near the animal, very diſtinct.

On the relaxation of the muſcles, the original flatneſs of the cornea will be reſtored by the elaſticity (as he affirms) of the ſclerotica; and he proceeds to make remarks on the advantage the animal ſyſtem derives from elaſticity being oppoſed to muſcular force, giving nearly the ſame inſtances as were offered with regard to the elaſticity of the cornea a few months before by Mr. Home *. He does not explain in direct terms the peculiar excellence this imbricated ſyſtem of ſcales may be imagined to poſſeſs, but thinks it particularly neceſſary to birds, which, without it, would, as he ſays, be continually expoſed to daſh themſelves againſt the trees of a thick foreſt. He conceives alſo, that the extraordinary alteration in the focus of the eye of an eagle in almoſt an inſtant of time when it darts from the upper air and ſeizes an object on the ground, and the purſuit and ſeizure of a gnat or ſmall fly by a ſwallow, muſt require the aid of this apparatus, which in them is very diſtinct. But with regard to theſe particulars, or final purpoſes, ſomewhat more of inveſtigation ſeems to be wanting. For it is certain, that no bird will dare to fly rapidly through a thick wood, but either paſs leiſurely from branch to branch, or ſoar above the tops of the trees when ſpeed is the object of their aim. And however conſiderably the bony circles may be ſhewn to add to the range of adjuſtment for near objects, it does not appear that any greater ſuddenneſs of change, in the eagle and other birds of the ſame ſpecies, is wanted, than is within the power of animals not poſſeſſing that ſtructure; becauſe the adjuſtment from remote to near objects is effected in the human, and probably every other eye, in a time incomparably ſhorter than that of an eagle's deſcent upon his prey. Neither do ſwallows purſue gnats or flies, but ſcud through the air with at leaſt twelve times the velocity of thoſe ſmall creatures, which they probably catch by opening their mouths without the leaſt uſe of ſight.

The mention of ſwallows, and the probability of winged animals ſtriking themſelves againſt trees in their flight, brings to my recollection ſome particulars which do not ſeem altogether foreign to the preſent ſubject. In a certain ſtate of the atmoſphere, it is uſual for ſwallows to fly rapidly along near the face of a row of houſes, at the diſtance of three or four inches from the wall. Now I take it for granted, that they cannot poſſibly diſtinguiſh the houſes during their courſe, on account of the rapid angular motion; and I doubt very much whether they ſee objects before them with any preciſion—not to mention the probability that the eye itſelf would be immediately dried if the nictitating membrane were not almoſt conſtantly upon it. With theſe reflections, on an occaſion of this kind, it did not ſeem ſurpriſing that many of theſe birds came ſo near my face, while obſerving them from a window, that they ſcarcely ſeemed to have noticed me. But the moſt remarkable circumſtance was, that they did in fact turn aſide ſo as to avoid ſtriking me; though in ſome inſtances ſo near that I

---

* Philoſ. Tranſ. 1795, p. 20; or this Journal, p. 313.

both heard and felt the action of the air to which they gave motion. On more attentive conſideration, however, it appeared to me, that this motion of the air is the very inſtrument which by its re-action may warn them of the preſence and poſition of an obſtacle the inſtant they approach it; which they may habitually and ſpeedily avoid. For, as the intelligent blind are ſaid to know, by the motion of air and its echo, the ſize of rooms, the width of ſtreets, and other local attributes, which are commonly aſcertained by the ſenſe they are deprived of; and as boats move more ſluggiſhly in ſhallow or contracted channels; and air itſelf cannot be forced with rapidity through long tubes;—in all which circumſtances the re-action muſt be very conſiderable; ſo, on the other hand, it can ſcarcely be doubted, that the object of ſuch re-actions, if capable of perception, would be ſufficiently aware of their preſence or abſence. I ſuppoſe the re-action of the air daſhed againſt my face by the ſwallow, and the noiſe I heard, were as perceptible and as impreſſive to the bird as to myſelf; and from this reaſoning I am leſs diſpoſed to wonder at the reſult of the cruel experiment performed in Italy a few years ago upon bats. As I have ſeen the notice only in a voluminous periodical publication to which I cannot now turn, I muſt ſimply relate, that bats have been thought to poſſeſs a ſixth ſenſe, becauſe, when the eyes of one of thoſe animals were diſſected out, it flew round a room with the ſame ſpeed and preciſion as before, avoiding the walls and obſtacles as readily as if ſtill poſſeſſed of ſight. If my conjectures have any foundation, theſe creatures did never uſe the eye to warn them of impediments to their flight, but had conſtantly attended to the re-action of the fluid in which they moved; which would be very different in the vicinity of an obſtacle compared with that in open ſpace. From the re-action of a direct obſtacle, they would turn to the ſide where the preſſure was leaſt, or the ſpace moſt open; and if the obſtacle were oblique, it may eaſily be imagined that the requiſite deviation from their courſe would be equally obvious.

To return to Mr. Smith's Memoir. He was led from the examination of the eyes of birds to thoſe of quadrupeds, and found by tearing and diſſecting that the recti muſcles terminate in the cornea; in which, and in his inferences, he entirely agrees with Mr. Home in the paper before mentioned *.

The Croonian Lecture, read before the Royal Society in November 1795, was written by Everard Home, Eſq. and contains a proſecution of the enquiry reſpecting viſion, of which an abridgement has already been given.

The explanation of the adjuſtment of the eye by an aſſumed change in the radius of the cornea being different from the theories before formed on that ſubject, it was thought right to put it to the teſt of every experiment which might appear likely to refute or confirm the obſervations already made. The reader has ſeen that a perceptible variation in the figure of this part of the organ of ſight, was aſcertained by obſerving its outline by means of a microſcope. Another method ſuggeſted itſelf; namely, that if the convexity of the cornea were increaſed to a certain degree, it could be meaſured by applying an achromatic microſcope, with a divided eye-glaſs micrometer, to view the image in the virtual focus of its ſurface. The firſt ſtep in this experimental proceſs was to aſcertain, by experiments upon convex mirrors, what difference of curvature could be decidedly obſerved by an apparatus of this kind. Two convex mirrors, one of $\frac{4}{10}$ths of an inch focus, the other $\frac{5}{10}$ths, had their

---

* Philoſ. Tranſ. or this Journal as laſt quoted.

flat ſurfaces made rough and blacked, to prevent an image being ſeen from both ſurfaces. One of theſe was ſtuck upon a piece of wood, directly oppoſite a window, at 12 feet diſtance from it. A board, four feet long, and ſix inches broad, was placed perpendicularly againſt the ſaſh of the window, and its image reflected from the mirror upon the object glaſs of an achromatic microſcope, with a divided eye-glaſs micrometer.

The two images were ſeparated by means of the divided eye-glaſs till their ſurface of contact, which appears like a black line, was rendered as ſmall as poſſible. When this effect was produced on the images from the mirror of $\frac{4}{10}$ths of an inch focus, that mirror was removed and the other put in its place. The contact of the two images, which before appeared like a line, had now acquired conſiderable breadth, correſponding exactly to the difference between the convexities of the mirrors.

When the ſame experiment was repeated upon the eye, there was a perceptible, though extremely ſmall, change in the micrometer at firſt when the eye was freſh. This was not however ſeen afterwards, and the eye very ſoon became ſo much fatigued, that it was neceſſary to deſiſt. It was found that every time the eye adapted itſelf to different diſtances it became neceſſary to alter the diſtance of the object-glaſs of the microſcope from the cornea.

This experiment was repeated on four different days with the ſame reſult; namely, a change in the micrometer at firſt, which in the ſubſequent trials could not be detected. Two ſuppoſitions offered themſelves as likely to account for this effect, namely, a motion of the head forwards, or an action of the muſcles of the head itſelf; but, as the author remarks, this effect ought in the firſt caſe to have been more frequent, the greater the fatigue of the eye; and the latter circumſtance would have produced a contrary reſult. It appeared, therefore, that the effect on the micrometer did really ariſe from a change in the cornea, though too ſmall to be detected with certainty in this way.

With two other mirrors, of which the focal diſtances were $\frac{4}{10}$ths and $\frac{408}{1000}$ths of an inch, the difference between the ſize of the images was juſt viſible in the micrometer; but it did not appear probable that the ſame difference would have been viſible had the mirror not been perfectly at reſt. From the unſteadineſs of the eye, it might therefore be reaſonably ſuppoſed that a change of this magnitude might take place in the cornea without being diſtinctly ſeen.

To give an idea of the ſhort time that a part can remain nicely adjuſted by muſcular action, the author points out an experiment which any one may make upon himſelf. Let him take a glaſs ſpirit level, and reſt one end of it on a table, ſupporting the other with his hand, and endeavour to keep the air bubble in the middle. If the hand is very ſteady, the bubble may be kept nearly in its place, but not exactly ſo; for it will undulate in correſponence with the action of the muſcles, making up for want of ſteadineſs, by ſhort motions, in contrary directions.

From theſe experiments, the change in the curvature of the cornea could not be more than $\frac{1}{125}$th part of an inch, as any greater quantity would probably have been diſtinctly ſeen in the micrometer. This, however, is ſtill more than was aſcertained by the former experiments, which made it to exceed $\frac{1}{800}$th part of an inch.

This change in the cornea, at firſt view of the ſubject, appeared ſufficient to account for the adjuſtment of the eye, and may probably be ſufficient when the lens is removed, but not when the eye is entire. The author therefore proceeded to examine what alteration the figure of the human eye might be ſuſceptible of when air was thrown into its cavity, through the optic nerve, ſo as to diſtend its coats. From the experiments it was found that the axis of viſion was lengthened a ſmall degree in the eye of a boy ſix years old, forty-five minutes after death, while the tranſverſe diameter and axis from the optic nerve were ſhortened. A ſimilar though leſs effect was obſerved in the eye of a man 25 years old, one hour after death; but no alteration took place, by the like treatment, in the eye of a man 50 years old, 20 minutes after death. The object of theſe experiments does not clearly appear, as there is no ſuſpicion of an action of this kind in the living ſubject; but they appear to ſhew that the cornea is the part moſt variable from elaſticity; and when the preſſure is made laterally, and from without, the elongation muſt be ſtill greater, the action of the ſtraight muſcles being the moſt advantageous that could be imagined for this purpoſe.

This lateral preſſure, Mr. H. obſerves, will not only elongate the eye, and increaſe the convexity of the cornea, but will produce an effect upon the cryſtalline lens and ciliary proceſſes, puſhing them forward in proportion as the cornea is ſtretched. For, as theſe proceſſes form a complete ſeptum between the vitreous and aqueous humours, the cavity of the aqueous humour will be always of the ſame ſize, and the cornea and lens at the ſame diſtance from each other; in the accurate production of which effects, he ſuppoſes it likely that the ciliary proceſſes may operate by muſcular action; an opinion which other facts in the courſe of the lecture tend to confirm.

The reſult of this enquiry, which was not carried on, as he remarks, in ſupport of any particular theory, but with the ſole view of diſcovering the truth, appears to be, that the adjuſtment of the eye is produced by three different changes in that organ; an increaſe of curvature in the cornea, an elongation of the axis of viſion, and a motion of the cryſtalline lens. Theſe changes, in a great meaſure, depend upon the contraction of the four ſtraight muſcles of the eye. Mr. Ramſden, from computations grounded on the principles of optics and general ſtate of the facts, eſtimates, that the increaſe of curvature of the cornea may be capable of producing one-third of the effect, and that the change of place of the lens, and elongation of the axis of viſion, ſufficiently account for the other two-thirds of the quantity of adjuſtment neceſſary to make up the whole.

After this explanation of the mode by which the axis of viſion can be elongated, and the convexity of the cornea increaſed in the human eye, for the purpoſe of its adjuſtment, Mr. Home was deſirous of applying theſe obſervations to the eyes of other animals.

Quadrupeds in general muſt have their eyes fitted to ſee very near objects, as many of them collect their food with their mouths, in which action the objects are brought very cloſe to the eye. Birds are under the ſame circumſtances in a ſtill greater degree with reſpect to their food; but from their mode of life, they alſo require the power of ſeeing objects at a great diſtance from the eye. Fiſhes, from the nature of the medium in which they live, muſt have ſome other mode of adjuſting the eye than that of a change in the cornea, becauſe that ſubſtance is poſſeſſed of the ſame refractive power as the ſurrounding fluid.

Quadrupeds have three modes of procuring their food; one by their fore-paws only, which they uſe like hands, as in the monkey tribe; the ſecond by their fore-paws and mouth, as the lion and cat tribe; the third by the mouth only, as all ruminating animals. Theſe three different modes require the food being brought from different diſtances from the eye, and it is curious that the muſcles of the eye are different in all the three tribes.

In the monkey tribe the muſcles of the eye are exactly the ſame as in the human. In the lion tribe they are double in number, and the four intermediate muſcles are loſt in the ſclerotic coat at a greater diſtance from the cornea than the others. In the ruminating tribe there are four muſcles, as in the human eye; but there is alſo a muſcle ſurrounding the eye-ball attached to the bottom of the orbit, round the hole through which the optic nerve paſſes, and loſt upon the ſclerotic coat immediately before the broadeſt diameter of the globe of the eye. The upper portion of this muſcle is rather the longeſt, the inſertion being nearly in a circular line at right angles at the axis of viſion, but not to the axis of the eye from the entrance of the optic nerve.

In quadrupeds in general, the ball of the eye is broader in proportion to its depth than in the human ſubject. In the bull the proportion is $1\frac{10}{10}$ inch to $1\frac{6}{10}$. The cornea is larger and more prominent, its real thickneſs not eaſy to be aſcertained, becauſe, like that of the human eye, it readily imbibes moiſture after death. When dried, it is thinner than the ſclerotic coat in the ſame ſtate. In ruminating animals, the cornea, though circular, has an oval appearance, ariſing from an opake portion which is covered by a membrane. The ciliary proceſſes, as in the human eye, are connected with the choroide coat; but they are larger, and are united at their origin with the iris.

This ſtructure of the eye in quadrupeds, as far as it differs from that of the human eye, appears calculated to increaſe the power of adjuſting it to near objects; and from the mode of life which theſe animals purſue, ſuch additional powers appear neceſſary to enable them to procure their food with eaſe.

That birds, procuring their food by their beak, muſt require an adjuſtment to ſee very near objects, and that a degree of preciſion and facility, with regard to remote objects, is no leſs neceſſary from their ſituation during flight, are obvious circumſtances. Mr. Home mentions ſome inſtances, in which birds are ſuppoſed to have ſeen diſtant objects with peculiar diſtinctneſs. It is related, that vultures and other birds very ſpeedily repair to the place where a dead animal or other prey is expoſed to them; and though there is nothing, perhaps, in theſe facts which ſuppoſe an acuteneſs of ſight greater than that of other animals, yet it cannot be doubted, upon the whole, that theſe animals ſee with very great preciſion at vaſt diſtances.

The eyes of birds are larger in proportion than thoſe of other animals, and broader in proportion to their depth, with a more prominent cornea than in the quadruped. The cornea in the gooſe is not united to the ſclerotic coat by the terminating of one abruptly in the other; but the two edges are bevelled off and laid over each other for nearly $\frac{1}{10}$th of an inch. This circumſtance, as alſo the bony rim or apparatus ſurrounding the baſis of the cornea, which is peculiar to this claſs of animals, and deſcribed by Mr. Smith, was well known to Haller. Mr. Home deſcribes it more particularly, and ſtates its uſe in the focal adjuſtment to be nearly the ſame as that deſcribed by Mr. Smith. The ciliary proceſſes are larger and longer in birds than in other animals whoſe eyes are of the ſame ſize; and the marſupium or membranous proceſs peculiar to the claſs of birds, is ſhewn by him to be very ſimilar to that of the ciliary proceſſes; but ſtronger in all its parts, and, like them, connected with the cryſtalline lens at the one extremity, and at the other with the bottom of the eye. By experiments on this membrane he aſcertained, that it is capable of muſcular contraction: for, when the marſupium and cryſtalline lens of a gooſe's eye were expoſed immediately after death, and the lens was puſhed forwards ſo as to elongate that membrane from $\frac{7}{40}$ths to $\frac{10}{40}$ths of an inch, it repeatedly recovered its original dimenſion when the preſſure was taken off. But when the parts had been left until all remains of life were gone, and the tenſion was then made as before to $\frac{10}{40}$ths of an inch, it contracted only to $\frac{8}{40}$ths; whence it was concluded, that the difference between the contractions was the effect of muſcular action. It was obſervable, however, that in ſo minute a quantity it was eaſy to be deceived. Another experiment was therefore inſtituted upon the well-known fact, that in the act of dying, the muſcles are found to contract to their utmoſt where there is no reſiſtance to prevent ſuch action; and that this is alſo found to take place in the greateſt degree when the animal is killed by any violence committed upon the brain or ſpinal marrow. The cryſtalline lens of a turkey's eye was extracted, and immediately afterwards the turkey was killed by wounding the ſpinal marrow. The two eyes were taken out and put into ſpirits. In the one the marſupium had nothing to prevent its contracting to the utmoſt; while in the other the lens being in its natural ſituation could not allow of any unuſual contraction. Some days afterwards the two eyes were examined: in the perfect eye the marſupium meaſured $\frac{4}{20}$ths of an inch, and its different folds were ſemi-tranſparent; in the imperfect eye the marſupium meaſured $\frac{3}{20}$ths of an inch, and the folds were much more opake. Here then was a difference of $\frac{1}{20}$th of an inch in the length of the two marſupiums: which could ariſe from no other cauſe than the one having contracted ſo much more than the other, which contraction muſt be conſidered to be muſcular.

On a review of the peculiarities in the eyes of birds the author infers, that they tend to facilitate the lengthening of the axis of viſion, and increaſing the convexity of the cornea. The bony rim to which the muſcles are attached, confines their effect to the broadeſt part of the eye; and as their action throws the cornea forward, the anterior rim of the bony edge yields to adapt itſelf to that change. The ciliary proceſſes are long, and admit of the lens being moved forward during the adjuſtment for very near objects, which is performed with more facility than in other animals, while their action ſerves to bring it back to its place. The marſupium ſerves alſo to draw it backwards, and, by ſuſtaining part of the preſſure from behind, renders the cornea flatter, while the anterior edge of the bony rim is adapted to it in this ſtate. It may be ſaid that no ſuch great change is neceſſary for viſion with parallel rays; but Mr. Home remarks, that where viſion is to be very diſtinct, a certain nicety of adjuſtment becomes neceſſary, and the action of the marſupium is probably intended for that purpoſe.

The ſubject of viſion in birds is concluded by an obſervation, that one of the moſt beautiful illuſtrations of the combination of muſcular and elaſtic ſubſtances is ſeen in the motion of the nictitating membrane. This membrane is elaſtic, and is connected by means of a tendon with two muſcles ſituated upon the poſterior part of the eye-ball. The action of theſe muſcles brings the membrane over the cornea; and the inſtant they ceaſe to contract, the elaſticity of the membrane draws it back again.

The eyes of fiſhes have ſeveral peculiarities, and in many reſpects their ſtructure differs from that which is obſerved in the quadruped and bird.

The muſcles of the eye, that correſpond to the ſtraight muſcles in the quadruped, are four in number: they are however differently placed; they do not ſurround the eye-ball, but two of them are on that ſide of the orbit next to the noſe of the fiſh, the other two on the oppoſite ſide; their attachment to the eye is cloſe to the edge of the cornea; they do not paſs round the eye-ball towards the poſterior part, as in other animals, but are connected with the bones of the head at ſome diſtance from the eye on each ſide, ſo that they cannot at all compreſs the eye laterally; they can only pull it backward by the combined effect of their actions.

The bottom of the orbit on which the eye-ball reſts is ſolid and adapted to it, there being no fat interpoſed between them as in other animals; and where the eye is removed to a great diſtance from the ſkull, and that cannot be the caſe, there is a ſtrong cartilage projecting from the ſkull to the bottom of the eye, and that end of it next to the eye is concave, and fitted to the portion of the eye-ball directly oppoſite the cornea, juſt above the entrance of the optic nerve. This is conſidered as a fixed point upon which the eye moves; but it will alſo, from the ſituation of the muſcles, allow the eye to be forced back upon it, and the whole eye to be flattened.

The ſhape of the eye differs conſiderably in different fiſhes; but in all of them the tranſverſe diameter is the longeſt. In the haddock the proportion is $\frac{11}{10}$ths to $\frac{8}{10}$ths of an inch, and in ſome fiſhes it differs much more.

The ſize of the eye does not correſpond with that of the fiſh; the ſalmon's eye being ſmaller than the haddock's.

The ſclerotic coat is in ſome fiſhes membranous[*], in ſome partly bone[†], in others entirely ſo[‡]; but in general the poſterior part is membranous, although the lateral parts are bone[§].

The cornea is in general flat, not always circular in its ſhape, is very thin, made up of laminæ, and does not loſe its tranſparency in ſpirits, appearing like talc[‖]. In others it is more convex, as in fiſh of prey; this appears to adapt it to the ſpherical cryſtalline lens, which in them lies directly behind it[¶]. The tunica conjunctiva forms the anterior layer of the cornea[**], and in ſome fiſhes is quite detached.

(*The Subſtance of Mr. Home's Concluding Lecture upon the Eye will be given in a future Number.*)

NEW PUBLICATIONS.

Philoſophical Tranſactions of the Royal Society of London, for the Year 1797, Part II. Quarto, 541 pages, excluſive of the title, contents, liſt of preſents, and index, with nine plates. Sold by Elmſly, London.

THIS part contains the following papers: 1. On the Action of Nitre upon Gold and Platina, by Smithſon Tennant, Eſq. F.R.S. 2. Experiments to determine the Force of Fired Gun-powder, by Benjamin Count of Rumford, F.R.S. M.R.I.A. (ſee Philoſ. Journal, i. p. 459). 3. A third Catalogue of the comparative Brightneſs of the Stars; with an introductory Account of the Index to Mr. Flamſted's Obſervations of the fixed Stars contained in the Second Volume of the Hiſtoria Cæleſtis; to which are added ſeveral uſeful Reſults derived from that Index, by William Herſchell, LL. D. F. R. S. 4. An Account of the Means employed to obtain an overflowing Well, in a Letter to the Right Hon. Sir Joſeph Banks, K. B. P. R. S. from Mr. Benjamin Vulliamy. 5. Obſervations of the changeable Brightneſs of the Satellites of Jupiter, and of the Variation in their apparent Magnitude; with a Determination of the Time of their rotatory Motions on their Axis. To which is added a Meaſure of the Diameter of the Second Satellite, and an Eſtimate of the comparative Size of all the four, by William Herſchell, LL. D. F. R. S. 6. Farther Experiments and Obſervations on the Affections and Properties of Light, by Henry Brougham jun. Eſq. 7. On Gouty and Urinary Concretions, by William Hyde Wollaſton, M. D. F. R. S. 8. Experiments on Carbonated Hydrogenous Gas, with a View to determine whether Carbon be a Simple or a Compound Subſtance, by Mr. William Henry. 9. Obſervations and Experiments on the Colour of Blood, by William Charles Wells, M. D. F. R. S. 10. An Account of the Trigonometrical Survey carried on in the Years 1795 and 1796, by Order of the Marquis Cornwallis, Maſter General of the Ordnance, by Colonel Edward Williams, Captain William Mudge, and Mr. Iſaac Dalby.

---

[*] Haddock. [†] Sword-fiſh. [‡] Devil-fiſh. [§] Mackarel. [‖] Sword-fiſh. [¶] Pike. [**] Haddock.

A Lecture introductory to a Courſe of Popular Inſtruction on the Conſtitution and Management of the Human Body, by Thomas Beddoes, M. D. octavo, 72 pages, price 1s. 6d. Printed for Cottle in Briſtol, and ſold in London by Johnſon, 1797.

The author of this pamphlet was accidentally informed, by a practitioner in ſurgery, in the courſe of the year juſt expired, that he was deſirous of giving a courſe of anatomical lectures in Briſtol. To furniſh individuals with ſo much knowledge of themſelves as ſhould enable them to guard againſt habitual ſicklineſs, and a variety of ſerious diſorders, had long been an object of Dr. Beddoes's contemplation. He, therefore, propoſed that the courſe ſhould be modelled according to this idea. He remarked that a diſtinct exhibition of the larger lines of anatomy and phyſiology would be alſo the mode of inſtruction beſt adapted to young ſtudents in medicine; much obſervation of lectures having convinced him that extreme minuteneſs is only perplexing to beginners. This, joined to ſome other conſiderations, prevailed. Meſſ. Bowles and Smith undertook the courſe, the intention of which is, to exhibit the ſtructure of the body in a manner neither ſuperficial nor tedious; to explain the functions of the parts as far as they have hitherto been inveſtigated; to illuſtrate, by ſpecimens, the principal deviations of theſe parts from their healthy conformation, and to interſperſe ſuch reflections as may be uſeful in phyſical education, and the whole conduct of life. Dr. Beddoes purpoſes to contribute his utmoſt aſſiſtance to the deſign, in whatever way that aſſiſtance ſhall, upon reflection, appear moſt likely to be effectual. This introductory lecture conſtitutes part of his exertions in a plan ſo truly calculated to promote individual happineſs, and public welfare. He is deſirous that its publication may produce ſimilar undertakings elſewhere, and juſtly ſuppoſes that a communication of the fact, that theſe lectures are attended by an audience more than twice as numerous as the friends of that deſign expected, will tend to promote this purpoſe.

After this conciſe ſtatement of an undertaking to which every friend of mankind muſt wiſh ſucceſs, I muſt decline analyſing the lecture, and content myſelf with obſerving, that the ſubject, which itſelf is highly intereſting to all deſcriptions of men, is treated in a very perſpicuous and impreſſive manner.

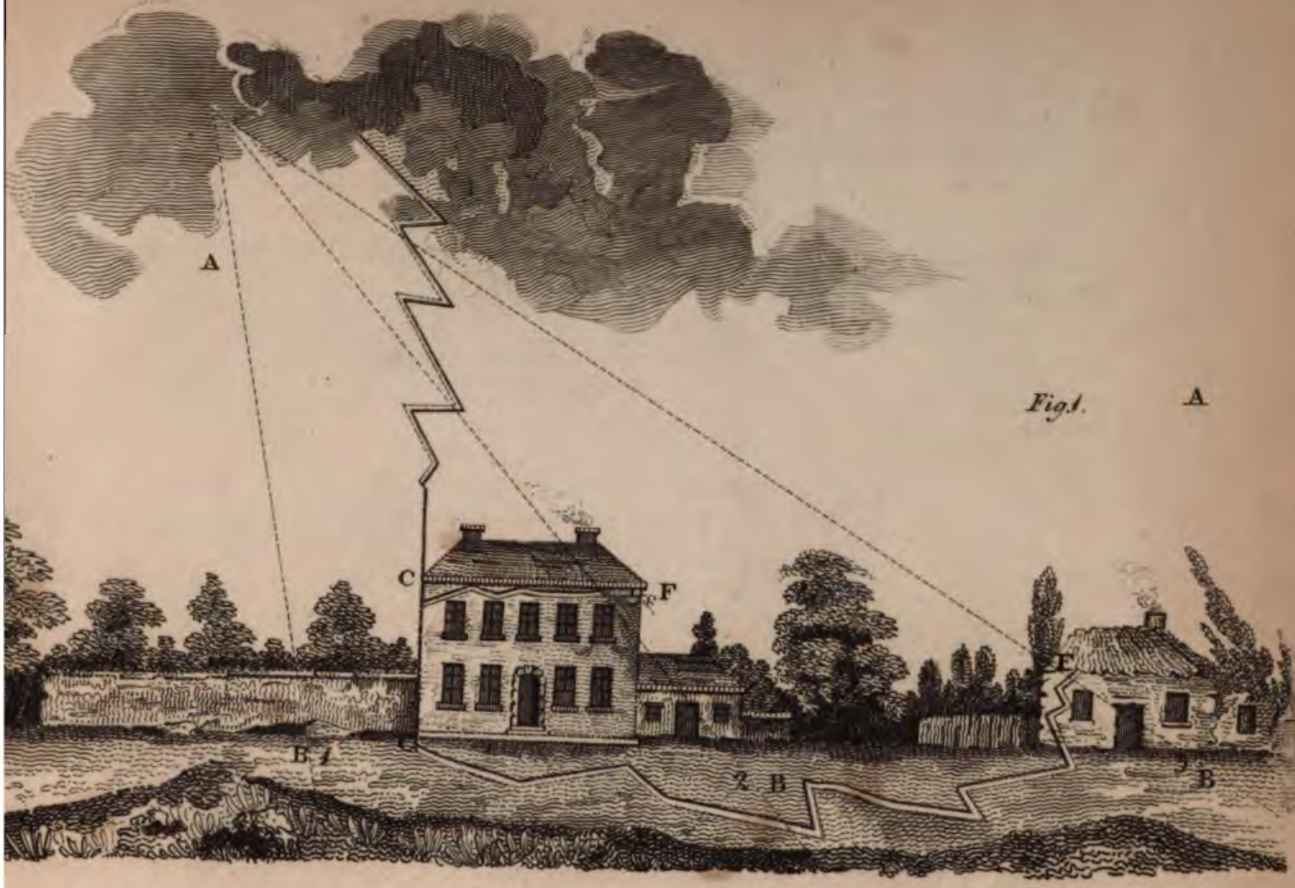


Fig. 1.

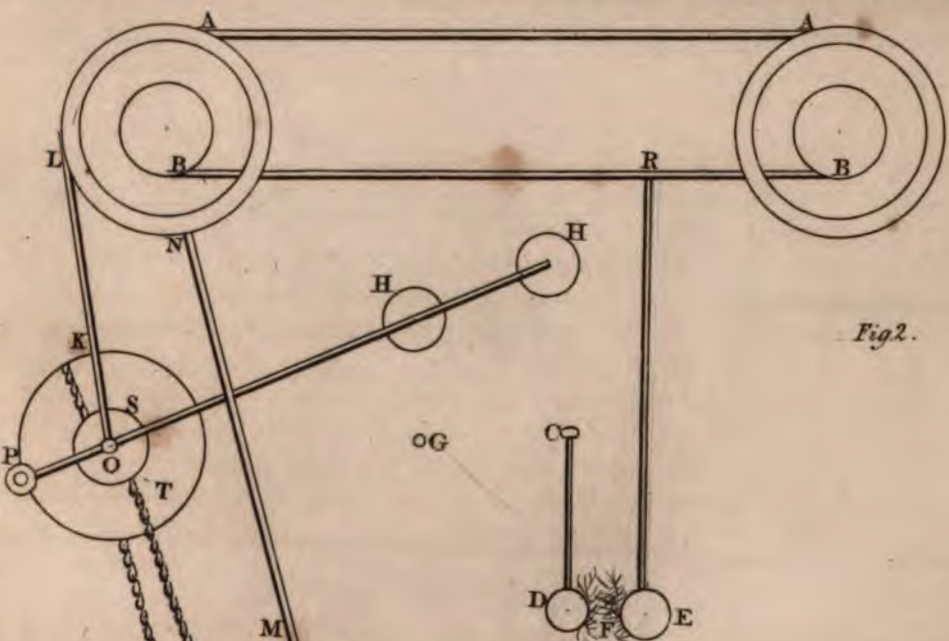


Fig. 2.

THE NEW Y
PUBLIC LIB
ASTOR AND

Air Pumps by James Sadler Esq^r.

Fig. 3

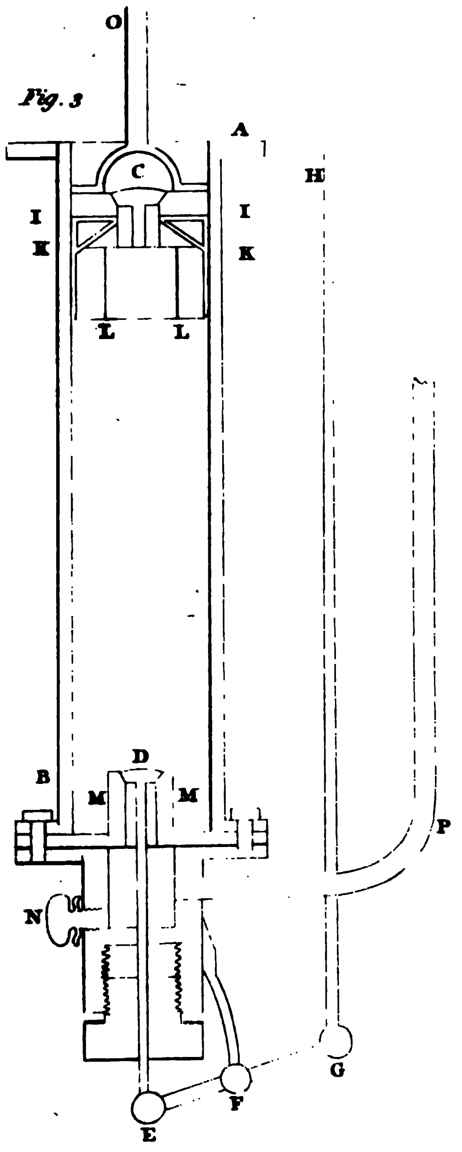


Fig. 2

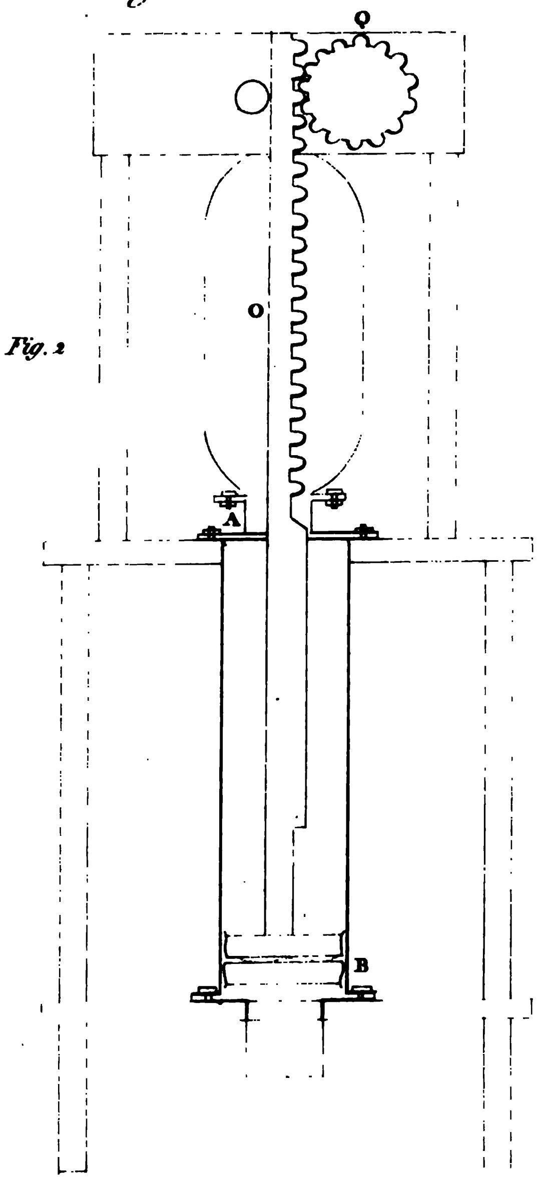
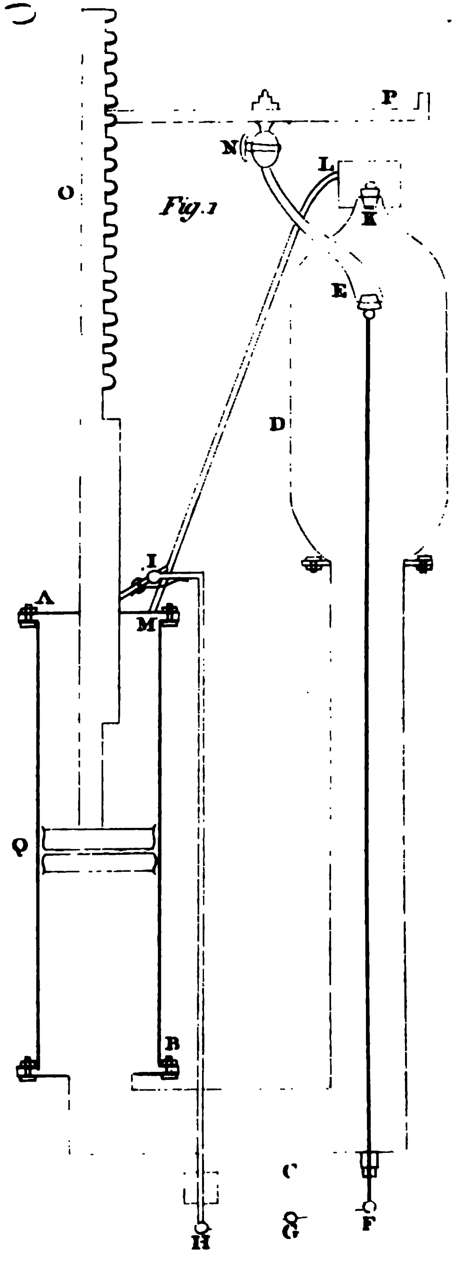


Fig. 1



THE NEW YORK
PUBLIC LIBRARY

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY 1798.

ARTICLE I.

An Attempt to accommodate the Diſputes among the Chemiſts concerning Phlogiſton. In a Letter from Dr. MITCHILL *of New York to* JOSEPH PRIESTLEY, *LL.D. F. R. S. &c. &c. Dated November* 20, 1797 *.

ON reviewing the ſtate of philoſophical controverſy as carried on both in Europe and America between the Phlogiſtians and their opponents; it has of late appeared to me, that much of the difficulty which attends the ſubject ariſes, as in abundance of other caſes, from the want of a preciſe language and of a right underſtanding of each other's meaning. This was ſo evident to me in the preſent caſe, that I informed my audience of it in one of my public lectures in Columbia College, and added my belief, that due attention to terms, their application and uſe, would have great influence in bringing the diſpute to a termination.

Having ſubjected water heated to the temperature of ſteam in an eolipile, and directed the ſteam iſſuing from it to the ſurface of red-hot charcoal; the coal brightened, and a greater flame was obſerved near the ſpot againſt which the ſteam was made to play. Here was an occurrence oppoſing the common obſervation of mankind, that water will always extinguiſh fire by reaſon of its own incombuſtibility. Water kept at or below a certain temperature will extinguiſh fire, and ſo will oil; but if water be raiſed to a heat ſufficiently high, it will alſo burn or undergo decompoſition like oil. As far as I could judge from the phenomena before me, water in proper circumſtances underwent a true combuſtion; and was inflammable for the ſame reaſon that oil was, becauſe it contained a ſomething that would burn; and this ſomething ſeemed to be exactly ſimilar to that which made oil capable of exhibiting flame. It ſtruck me inſtantly, that the inflammability of this vapour proceeding from burning fat, from heated alcohol, from camphor, ether, coal, and a multitude of other ſubſtances, gave evidence of their poſſeſſing a principle enabling them to burn with flame after the ſame manner that water did. If there was this ſimilarity or indeed identity of the inflammable radical among them, there appeared to be no more propriety in calling that radical *hydrogene* than in terming it *olegene*, *alcohologene*, *etherigene*, *coalegene*[*], &c. To give the radical ſubſtance enabling oil, alcohol, ether and coal to burn with flame a name derived from water, becauſe it enabled water to burn with blaze too, appeared to me partial, illogical and wrong, inaſmuch as it conſtantly and unneceſſarily brought water and its properties to mind whenever any thing was thought of that contained hydrogene; and by this unhappy aſſociation, beſides the difficulty which attends the ſubject in point of fact, vaſtly greater difficulty was made to ſurround it by reaſon of the ill choſen and badly aſſorted terms employed in talking about it.

---

* Communicated by the Author. N.

I had entertained no doubt, for two years, that *hydrogene* was an improper word for a nomenclature of ſcience, and deſerved to be ſtruck out of the liſt: but as I was engaged in reforming another article of that arrangement, I choſe not then to meddle with it; and I am glad I did not; ſince the prolonged diſputes between the parties afford more weighty cauſes for an alteration of terms at this day than exiſted at any former time.

The circumſtance common to all the proceſſes I have mentioned, is "burning with flame or blaze," which, wherever it occurs, ſeems to indicate the preſence of what has been called hydrogene. According to my preſent conception of the matter, this principle or ſubſtance common to ſo many bodies, and enabling them to undergo inflammation, may in ſtrict propriety be called *phlogiſton*. I always thought *phlogiſton* a well conceived word, and have objected to it, not on account of the impropriety of the term as ſuch, but becauſe of the vague and unſatisfactory way in which it was uſed. If a definite ſignification can be affixed to it, I think the adoption of it will be ſtill a great acquiſition to philoſophical language, and have a tendency to ſettle at leaſt half the controverſy which divides the chemiſts.

I propoſe, then, to expunge *hydrogene* and ſubſtitute *phlogiſton* in its place. *Phlogiſton* will thus be the radical term, and ſtrictly mean the thing in combuſtible bodies which forms blaze or ignited vapour. The union of this with mere caloric will make *phlogiſtous* or inflammable air, the air which burns with blaze. The combination of phlogiſton with oxygene will conſtitute water or the *oxyd of phlogiſton*, one of the products of inflammation, and, like fixed air and other compounds formed during the ſame proceſs, incombuſtible in common temperatures and circumſtances afterwards. And the cauſe of this ſlowneſs to burn, of water and the other compounds which combuſtion furniſhes, is owing to the large doſe of oxygene with which they are charged, giving them little or no appetite for more. If this baſe be united to a yet larger quantity of oxygene, it will form the acid of phlogiſton, or water ſoured by exceſs of oxygene, as *perhaps* (though I do not believe it) in what is termed the pyro-lignic and pyro-mucic acids, and perhaps in ſome other caſes: but the readineſs with which phlogiſton parts with its ſurpluſage of oxygene, turns back to water, and preſerves itſelf in that oxydated form (as proved by the operation of ſharp-pointed filaments under water in effecting the ſeparation) ſhews that nature, in enabling the principle of inflammability to combine with oxygene, diſqualifies the latter in moſt caſes from becoming an acid with the former; unleſs it ſhould be found (and in this I have no faith) that the formation of the native acids of vegetables is a proceſs of this kind. Should this latter conjecture turn out to be the fact, there would be inſtances enough of *phlogiſtic* and *phlogiſtous acids.*

---

[*] So in the original: doubtleſs by overſight. N.

The nomenclature will then ſtand thus:

1.	2.	3.	4.
Phlogiſton for Hydrogene.	Phologiſtous gas. for Hydrogenous or inflammable air.	Oxyd of phlogiſton for Water or oxyd of hydrogene.	Gaſeous oxyd of phlogiſton for Aqueous gas, or watery ſteam.

5.	6.	7.
Phlogiſtous and phlogiſtic acids. Probably no ſuch things; the hydrogene or phlogiſton not being capable of combining with oxygene beyond the degree of oxyd.	Phlogiſtates and phlogiſtites. Probably no ſuch combination.	Phlogiſtures and phlogiſturets; of ſulphur, coal, phoſphorus, iron and zinc, inſtead of The common ſulphur, &c. of the laboratories and ſhops, &c. &c.

On the decompoſition of fat and oil by fire, it is known that a large quantity of water is formed, and this probably by an union of the baſe of vital air with phlogiſton or hydrogene. The like obtains in the inflammation of alcohol, camphor, ether and coal, part of the phlogiſton or hydrogene of which apparently turns to water by junction with oxygene. And the principle which in the firſt inſtance readily exhibits the blazing appearance is changed by and during that operation to a ſomething much more difficult to inflame by any after-proceſs.

If hydrogene or phogiſton is the material which inflames in the ſubſtances already mentioned, there is preſumptive evidence, upon the face of the ſubject, of its exiſtence alſo in the common ſulphur, phoſphorus, zinc, and iron of the laboratories. I do not mean to ſay it is a *neceſſarily* conſtituent part of either of thoſe bodies; for I believe they may exiſt without it, or at leaſt they may be conceived to exiſt abſtractedly from it. They therefore ſtand very well in the nomenclature as ſimple ſubſtances. But if theſe ſubſtances, ſuch as we commonly get them after expoſure to the common atmoſphere in ordinary temperatures, are taken for ſimple or pure elementary bodies, the perſons who conſider them ſo fall into a great miſtake. In their uſual forms they are all incorporated with hydrogene or phlogiſton, and from it derive their capacity to burn with flame. This will be the more clearly ſeen by conſidering them more particularly one by one.

1. Of Sulphur. The Phlogiſtians ſay ſulphur is compoſed of phlogiſton united to vitriolic acid; conſequently if any thing takes away that ingredient from the acid, this will turn to brimſtone. The antiphlogiſtians affirm ſulphur to be a ſimple body, uncombined chemically with any thing; and that it becomes ſulphuric acid by junction with oxygene. Now both parties have reaſoned in a manner that does not by any means ſatisfy me. They have viewed the combuſtion of ſulphur in the abſtract, rather than taken it up as it is. The fact is, that the acid formed in the combuſtion of ſulphur is not the ſolid cryſtallized mat-

ter of the glacial oil of vitriol, but a ſolution of theſe cryſtals in water. The exiſtence of vitriolic acid in a fluid form implies neceſſarily the co-exiſtence of water. The formation of the water in the inflammation of ſulphur appears to have been paſſed over by both parties, though the interpretation of this part of the proceſs ſeems to me to furniſh the means of reconcilement. Thus, while the pure ſulphur combines with one portion of oxygene to make the acid, the hydrogene or phlogiſton unites with another parcel of it to form the water in which the acid diſſolves. Common brimſtone then is not a ſimple ſubſtance, but is a *phlogiſture of ſulphur*. And this is confirmed by the fact that, where combuſtion is reſtrained, the ſulphur may be reſolved into hepatic gas; the phlogiſton turning with caloric into inflammable air and diſſolving ſome of the ſulphur. The formation of common hepatic gas ſeems to evince the ſame thing; for while the potaſh ſeizes the ſulphur, the hydrogene or phlogiſton is ſet looſe, turns with caloric to hepatic gas, and ſnatches, as it departs, a portion of the ſulphur from the alkali. Thus it appears that the two ſyſtems are reconcileable with each other. When the Old Chemiſts talk of phlogiſton, they ſhould define it to be that thing which burns with flame, and when united to oxygene forms water. When the new ones make experiments on ſulphur, they ſhould remember that the common material called by that name is not the abſtract, pure, uncombined elementary thing they intend in their nomenclature.

2. In like manner the phenomena attending the inflammation of phoſphorus ſeem to have been as negligently interpreted. Phlogiſton added to phoſphoric acid was ſaid by ſome to conſtitute phoſphorus; while oxygene added to phoſphorus made phoſphoric acid in the opinion of others. But theſe were a kind of chemical theorems, true only in the abſtract experiments. We find that phoſphorus burns *with flame*, and *water* is exhibited during the proceſs. All that needs be ſaid about it is, that in common circumſtances phoſphorus, though capable of exiſting *per ſe*, has a very ſtrong attraction for hydrogene or phlogiſton; and in ordinary caſes attaches more or leſs of it to itſelf. During its inflammation part of the oxygene, as in the caſe of the ſulphur, combines with the phlogiſton into water; and another part of it joins the oxygene to conſtitute the acid. In eſtimating the *whole* of the proceſs, the candid partiſans of both ſides will allow that the ſubſtance under conſideration parts with its phlogiſton and borrows oxygene, and thus water and the acid diſſolved in it are formed.

Where is the harm of owning that common phoſphorus contains a portion of hydrogene united with it? It does not invalidate the modern theory: but it ſhews that the objections of the ancient doctrine were not frivolous, as they have by ſome been deemed to be; but on the contrary very ſubſtantial, and not capable of reconcilement upon any other plan, that I know, than the one herein ſuggeſted.

3. Zinc may be abſtractedly conſidered as a ſimple body, and with propriety placed as ſuch in the catalogue. Commonly, however, it is preſented to us in cloſe connection with hydrogene, for which its attraction is ſo ſtrong that they commonly appear in the form of a *phlogiſture of zinc*. When that compoſition is employed for experiments, it is very eaſy to conceive how, when ſuch zinc is expoſed to a ſufficient heat in an open fire, the phlogiſton diſlodged, and immediately becoming phlogiſtous gas or inflammable air, ſhall take upon itſelf the form of flame and conſtitute water; while the oxygene combines with the metal into a white oxyde, the flowers of zinc. So if the ſame compound be diſſolved in ſulphuric

acid, the phlogiſton diſplaced will turn to inflammable air, while the acid and the zinc form white vitriol. In this way ſome of the phlogiſtous or inflammable gas may be accounted for, as extricated from the metallic preparation: and at the ſame time I ſee no objection to deriving the reſt of the great quantity afforded by this proceſs, from the decompoſition of part of the water or oxyd of phlogiſton. To accommodate matters then, the advocates of the Lavoiſierian theory ſhould concede that zinc, in common circumſtances, is aſſociated with hydrogene or phlogiſton. And the diſciples of Stahl ſhould on their ſide allow that zinc cannot be conſidered as a pure metal, while alloyed or blended with phlogiſton or any other foreign ingredient. The material they have all worked upon is not the uncombined metal, but a phlogiſture of zinc.

4. Some ſorts of iron treated by heat alone afford phlogiſtous or inflammable air. The ſame metal may be made to burn with flame, and, when treated with ſulphuric acid, affords much phlogiſtous air. What then is the thing commonly called iron? Is it a pure and unmixed ſubſtance? Or is it a compound of elementary iron with hydrogene or phlogiſton? The facts enumerated lead concluſively to the latter opinion. The Phlogiſtians are right then, when they ſay common iron is a compound: and they are right when they ſay the inflammable air obtained from it is nearly pure phlogiſton. And the Antiphlogiſtians are juſtifiable in placing in their enumeration of ſimple bodies, ſuch a thing as elementary iron is or may be imagined to be; and in aſcribing the production of hydrogenous or phlogiſtic gas to a decompoſition of part of the water. The compound called iron then gives out ſomething, and takes in ſomething, in all the common proceſſes. And the modern chemiſts ſhould correct the miſtake they appear generally to have fallen into, of taking it for granted, that was a ſimple ſubſtance which in fact is a chemical compoſition of iron with hydrogene. And thus finery cinder, which evidently differs from hæmatites or any pure oxyd of iron, may be a triple compound of iron, hydrogene or phlogiſton, and oxygene; which juſt about correſponds with your idea that it conſiſts of iron and water.

It will not follow from all this, that becauſe phlogiſton or hydrogene ſo generally exiſts in combination with zinc and iron, it muſt be an ingredient in all metals. For gold, arſenic, ſilver, platina, mercury, copper, tin, lead, biſmuth, cobalt, antimony, and manganeſe are capable of exiſting without it, and accordingly do not commonly burn with flame, nor afford inflammable air by ſolution in acids; though if ever they exhibit in any of their ſtates blaze by burning, or phlogiſtous air with acids, this will only evince the exiſtence of hydrogene in them in ſuch caſes. Both parties may thus allow that ſome metals contain phlogiſton, and ſome do not. Nor will charcoal, as has ſometimes happened, be confounded with phlogiſton according to this view of the matter; though hydrogene is often blended with it. If coal at any time affords inflammable air, this is no evidence of the converſion of that ſubſtance into phlogiſtous gas, but merely a proof that the coal when ſubmitted to experiment was combined with the baſis of inflammable air, which it could part with and ſtill remain coal, though in that caſe incapable of burning with flame; but, in a ſufficient expoſure to heat and oxygenous air, taking on without blaze the form of carbonic acid gas. It may be conceded then on both ſides, that, though phlogiſton or hydrogene may exiſt with coal, nevertheleſs coal can exiſt without phlogiſton.

I know not in what manner theſe conſiderations may impreſs your mind. I have flattered myſelf they have a tendency to accommodate and reconcile the gentlemen of both ſides to each other, and to the truth. But in this if I have deceived myſelf, I have only to appeal to your known experience and candour. It does however ſeem to me capable of getting through with much of the diſcuſſion not yet ſolved between Mr. Kirwan and the French philoſophers; and between yourſelf and Meſſrs. Adet and Maclean; and I ſuſpect the celebrated experiments of Mrs. Fulhame are in no wiſe repugnant to this mode of interpretation.

Permit me to expreſs my ſatisfaction, before I conclude, that you have not given up the points before the philoſophical world, without due examination. I no more like unanimous deciſions in haſte upon a queſtion of ſcience than of legiſlation. The ſure way to introduce errors both into laws and into philoſophical performances, is to aſſent to new bills and projects upon the credit and authority of the propoſer, without compelling them to undergo the amendments of debate. And I cannot help conſidering your refuſal to agree to *all* that the New Nomenclature Men have aſſerted and recommended, as a fortunate event in philoſophy; as I am ſure, with reſpect to myſelf, the oppoſition you have made has cauſed me to conſider the ſubjects in diſpute with greatly more attention than I otherwiſe ſhould have done. With much reſpect I beg leave to aſſure you that I am, &c.

SAM. L. MITCHILL.

Annotations upon the preceding Letter.

Steam in an eolipile.] It is well known that ignited charcoal in the gun-barrel decompoſes water, and that the products are hydrogene and carbonic acid gas. In this caſe the carbone is burned, and the hydrogene unburned. Are we to ſuppoſe that the water was firſt decompoſed in Dr. Mitchill's experiment, and then recombined by a ſecond combuſtion of the hydrogene? or was the effect any thing more than an increaſed energy of combuſtion by the greater afflux of atmoſpheric air mechanically impelled by the ſteam? Dr. Lewis * found that the ſteam from the eolipile always extinguiſhed his fires, unleſs the vapour was made to paſs through a portion of the atmoſphere. Whence I infer that the quantities of heat carried off and rendered latent in giving elaſticity to the hydrogene and carbonic acid are ſo great as ſpeedily to diminiſh the temperature, and terminate the combuſtion of the charcoal; unleſs, as in the experiment of the gun-barrel, there be a ſufficient ſupply of heat from without.

by this unhappy aſſociation.] The argument of aſſociation of ideas ſeems to militate ſtrongly againſt the reception of the word phlogiſton in a new ſenſe.

Burning with flame or blaze.] Though the author's arguments in favour of hydrogene, as the excluſive cauſe of flame, may be hypothetically applied to all the inſtances in which this appearance is ſeen; yet the contrary poſition, namely, that bodies, whether mechanically divided, ſuch as the powder of reſin or coal, or chemically, as zinc in the elaſtic ſtate during ſublimation, may be ſet on fire and exhibit the appearance of an ignited tranſparent fluid, ſuch as flame, ſeems at leaſt equally probable. It cannot therefore be ſtated as matter of fact, that where flame is there muſt neceſſarily be hydrogene.

---

* Philoſoph. Commerce of Arts, p. 21.

In the decompoſition of fat and oil by fire, &c.] The difference between the leading aſſumption in Profeſſor Mitchill's letter, and the theory formerly maintained by Mr. Kirwan, and ſtated by him in the Introduction to his Work upon Phlogiſton, appears to be, that the former does not ſuppoſe his phlogiſton, or principle which affords flame, to be eſſential to the metallic ſtate, but merely, as I conceive it, accidental in that claſs of combuſtible ſubſtances. He follows Mr. Kirwan, by firſt directing his attention to the univerſality of hydrogene in organized bodies, and then endeavouring to ſhew that it exiſts in ſulphur, phoſphorus, zinc, and iron, in their ordinary combuſtible ſtate. It has not yet been ſhewn that aqueous ſulphuric acid can be produced by the combination of ſulphur and oxigene, without the preſence of water ready formed. The ſame obſervation more ſtrongly applies to phoſphorus, which Lavoiſier converted into the ſolid acid by the burning glaſs in a veſſel of oxigene gas over mercury. In the obſervations on zinc and on iron, we look in vain for the experimentum crucis, which muſt be exhibited before the ſuppoſed fact that metals contain hydrogene can be admitted.

I have ventured to make theſe remarks on the Profeſſor's ingenious communication, without aiming at the eſtabliſhment of poſitions contrary to thoſe he has offered. The queſtions he treats muſt be decided by plain facts, if ſuch can be found. Till this be done, they remain among the maſs of unknown propoſitions, concerning which, we ought neither to affirm nor deny.

II.

Experiments on the Compoſition and Proportion of Carbon in Bitumens and Mineral Coal.

By RICHARD KIRWAN, *Eſq. F. R. S. L. & E. M. R. I. A. &c.*

AN exact knowledge of the component parts of the different ſpecies of mineral coal, and alſo of bitumens (ſubſtances which moſt of them contain) forms an object of ſome importance, not only to the naturaliſt whoſe views are merely ſpeculative, but to the practical œconomiſt, who wiſhes to extract from each ſpecies all the advantages it is capable of yielding, and to be enabled to compare the various kinds afforded by different countries, in order to obtain and employ that which ſhall on the compariſon appear to him beſt ſuited to his intentions.

In effect, coals are not only applicable to the more uſual purpoſes of combuſtion—an uſe, ſimple as it may appear, attended, according to their various ſpecies, with a conſiderable difference of calefactive power both in intenſity and duration, but alſo to the production of varniſhes, much more advantageouſly applicable in many inſtances than thoſe extracted from the Vegetable Kingdom, as Lord Dundonald has diſcovered, and abundantly proved*, and alſo of that charred reſiduum called coak, the only one that can be reſorted to in many caſes, and, in moſt, ſuperior to vegetable charcoal.

* Upon the moſt minute enquiry why coal-varniſh is not more commonly employed in paying the bottoms of ſhips, I have been informed the principal reaſon is, that it ſucceeds too well; the ſhips not requiring ſuch frequent repair. K.

Coals and bitumens are, however, ſubſtances that reſiſt the uſual modes of analyſis; they elude the action of aqueous, acid, alkaline, or ſpirituous menſtruums; and diſtillation, the only mode hitherto uſed, confounds and varies their natural contents.

Reflecting on theſe obſtacles to an exact diſcrimination of bitumens and coals, and of the various kinds of theſe laſt, it occurred to me, that partly by combuſtion and partly by their efficacy in decompoſing nitre, the ſecret of their internal compoſition might poſſibly be unveiled.

1. Combuſtion. I have obſerved that all the ſpecies of ſolid bitumen properly ſo called, when laid on a red hot iron, burn with a large bright flame, ſmoke and ſoot, leaving none or ſcarce any coaly reſiduum, and only a little aſhes:

That the ſofter bitumens, as maltha, burn in the ſame manner, leaving no coal, but only a little aſhes, and requiring no increaſe of heat for their entire conſumption:

That aſphalt burns with flame and ſoot, but melts and ſwells, and requires for its entire conſumption an increaſe of heat, leaving ſcarce any coal, and but little aſhes.

It is moreover well known that liquid bitumens contain inflammable air and carbon: that they abſorb atmoſpheric air when long expoſed to it and light: that, in conſequence of this abſorption, they are thickened, blackened, and condenſed, firſt into mineral tar, then into mineral pitch or maltha, and laſtly into aſphalt: that almoſt all ſpecies of mineral coal yield more or leſs of both ſpecies of bitumen in diſtillation, leaving a ſhining coaly reſiduum; but that the proportion is variable in every ſpecies according to the degree of heat applied; that the reſiduum always obſtinately retains a proportion of bitumen, and that conſequently diſtillation, in addition to its other imperfections, is an inſufficient medium whereby to diſcern the proportion of carbon and bitumen, and conſequently to diſcriminate the various ſorts of mineral coal from each other.

2. Decompoſition of nitre. It has long ago been remarked by the juſtly celebrated Macquer *, that nitre detonates with no oily inflammable matter, until ſuch matter is reduced to a coal, and then only in proportion to the carbonaceous matter it contains. An obſervation the truth of which will fully appear in the ſubſequent experiments.

Hence it occurred to me that, ſince in the act of detonation nitre is always totally or partially decompoſed, and ſince, where carbonaceous compounds are employed, this decompoſition ariſes ſolely from the mere carbonaceous parts, and, every thing elſe being equal, is proportioned to the quantity of mere carbon they contain; and ſince moſt ſpecies of coals are compounds of mere carbon and bitumen, as appears by the products of their diſtillation; it ſhould follow, that, by the decompoſition of nitre, the quantity of mere carbon in a given quantity of every ſpecies of coal may be diſcovered; and this being known, that of bitumen may be inferred; and, the other uneſſential ingredients being detected by incineration, the whole contents of coaly ſubſtances might be aſcertained.

The compoſition of bitumens alſo, as far as relates to their proportion of carbon and oil, may be evidenced in the ſame manner; and here it is to be obſerved, that the bitumens I here conſider are thoſe that are found in a dry or ſolid ſtate, and that theſe contain a larger proportion of carbon than the liquid bitumens; for, though theſe laſt alſo contain carbon, it being an eſſential component part of all oils, yet this portion does not extricate or educe

---

* Diction. Chym. ſecond edition, 481.

any air from nitre, nor conſequently contribute to its decompoſition, as the ſubſequent experiments ſufficiently evince; but is conſumed partly by the pure air ſpontaneouſly emitted by nitre during its ignition, and partly by the ambient atmoſpheric air.

Nay, when mineral coal is employed in the decompoſition of nitre, the ſhare which the mere carbonaceous part of the bitumen contained in it contributes to the decompoſition will be found ſo ſmall that it merits no conſideration in the general account.

The firſt ſtep towards carrying this analytic plan into execution muſt therefore be, to determine the quantity of pure carbon neceſſary to decompoſe a given quantity of pure nitre. But here many practical difficulties occur, which ſhall preſently be mentioned. The moſt perfect method of obviating them was that employed by the ever memorable Lavoiſier: he mixed the pureſt nitre with charcoal alſo purged of the inflammable as well as other airs and water which it uſually abſorbs, in the proportion which, after ſeveral trials, he found requiſite for the entire decompoſition of that ſalt, rammed them into a copper tube, fired them, and continued the inflammation under water; by which means the charcoal was acted on ſolely by the air educed from the nitre to the entire excluſion of the external air, and this air was educed ſolely by the ignited charcoal to the entire excluſion of external heat; advantages that cannot be procured by the uſual mode of effecting this decompoſition. Thus he found the proportion of charcoal neceſſary for the entire decompoſition of nitre, to be as 1 to 7,57, or, in other words, that 13,21 parts charcoal decompoſe 100 of nitre[*]: and yet even in this experiment I find a ſmall inaccuracy, as he did not take the water employed in mixing the nitre and charcoal into the account; and hence, and for ſome other reaſons, the detail of which would lead me too far, I think the proportion ſhould be as 1 to 7,868 nearly, or that 12,709 charcoal decompoſe 100 of nitre; but the difference is of little importance.

This mode of experimenting, however, is inapplicable on the preſent occaſion; the different ſpecies of mineral coal being not ſo readily inflammable as to carry on the combuſtion in this manner. Hence I contented myſelf with the common manner only, uſing ſuch precautions as to render its reſults tolerably uniform, and repeating each experiment ſeveral times.

I examined the purity of the nitre I employed by nitrated ſilver, and found by the quantity of ſalited ſilver produced, that 480 grains of the nitre contained 3,5 grains of common ſalt (135 grains of muriated ſilver, indicating 100 of common ſalt): hence the conſtant quantity of nitre I uſed was 483,5 grains, except in the experiments on bitumens, as I had not enough of them to expend in ſo large a quantity of nitre.

The nitre was heated barely to redneſs, before any coal was projected on it, in a wind furnace and a very large crucible; upon this uniform degree of heat much of the uniformity of different experiments on the ſame ſpecies of coal depends.

In my firſt experiments the coals were reduced to a very fine powder, and then projected on the ignited nitre; but I obſerved that by this method much more of each ſpecies of coal was requiſite to alkalize the ſtandard quantity of nitre, than when it was reduced to a coarſe powder about the ſize of a pin's head, or ſomewhat larger; and the reaſon is, that by the force of the exploſion much of the finer powder is carried off, without having been in contact with the nitre. Hence in the experiments of Mr. Hielm on the quantities of charcoal of different woods requiſite to alkalize 100 parts of nitre, we find theſe quantities to bear for the moſt part ſome analogy to their ſpecific gravities, being generally ſmaller when the ſpecific gravity of the charcoal is lighter; thus *:

---

[*] 11 Mem. Sçav. Etrang. 626.

Grains requiſite to alkalize 100 grains of nitre.

	Specific gravity.	Firſt experiment.	Second ditto.
Oak-coal	0.332	35	30
Birch-coal	0.542	22	22
Pine-coal	0.280	29	20
Fir-coal	0.441	33	25
Coak	0.744	19	

Another circumſtance of great importance towards procuring juſt and uniform reſults is, that the projections of coal ſhould ſucceed each other without delay as ſoon as the flame ceaſes; for, as ignited nitre gives out pure air ſpontaneouſly, and ſo much the more as it is more heated, the acid will be decompoſed, and the nitre alkalized, by a quantity of coal ſo much the ſmaller as the intervals of projection are longer. From inattention, perhaps, to this and the laſt-mentioned particular, as well as from various conditions of common charcoal, which ſeldom contains leſs than $\frac{1}{12}$, and often $\frac{1}{8}$ of its weight of moiſture and abſorbed air, proceeded the various reſults of different chemiſts, with reſpect to the proportion of it neceſſary to alkalize nitre.

It is almoſt ſuperfluous to add, that the charcoal ſhould be projected in very ſmall portions. I ſeldom projected more than one or two grains at a time: each operation laſted from 20 to 25 minutes nearly.

There is always ſome portion of nitre undecompoſed, being protected by the ſurrounding alkali. This error is unavoidable, but very ſmall. Even the poſition of the crucible in the furnace is not indifferent; for, if it be near the flue, more coal muſt be employed, which I attribute to the torrent of air, which, in that caſe, affects and carries away more than when the crucible is nearer to the anterior part of the furnace.

It may, perhaps, be ſuſpected that this and ſome other incidental errors may be avoided by previouſly mixing the nitre and coal, and projecting the mixture in ſmall portions into a red-hot crucible; but not to mention that this method ſuppoſes the due proportion of theſe two ſubſtances to be known, which cannot be till after the experiment, and that alſo every atom of theſe ſubſtances is in perfect contact with the other ſubſtance, elſe they cannot act on each other—independently, I ſay, of theſe unfounded ſuppoſitions, this mode of experimenting is ſtill more fallacious than the former, as during theſe projections a conſiderable proportion of the nitre is ſcattered and diſperſed, and may be ſeen adhering to the ſides of the crucible. This loſs being repeated at every projection, becomes at laſt intolerable.

I now proceed to relate the experiments themſelves, conducted in the manner I have mentioned. The different ſpecies of coal and bitumen, whoſe compoſition I have thus examined, were Kilkenny coal, Maltha, Aſphalt, Lancaſhire cannel, Slaty, Scotch cannel, Whitehaven, Wigan, Swanſey, and Leitrim; ſelecting of each ſort the pureſt ſpecimens, free from pyrites and viſible ſtony matter.

---

* Schwed. Abhandl. 1781, 188.

Kilkenny Coal.

ITS colour is black, and, when freſh broken, frequently violet.

Luſtre 4, Metallic. Tranſparency 0*.

Fracture foliated, the courſe of the lamellæ variouſly and confuſedly directed. Fragments rather ſharp, and often diſcovering between the diſtinct concretions whitiſh illinitious.

Hardneſs 7. Specific gravity 1,526.

Does not burn until wholly ignited, and then ſlowly conſumes without caking or emitting flame or ſmoke. 266 grains of it expoſed to a heat of 27° Wedgwood in a crucible for five hours, did not loſe their luſtre until almoſt $\frac{9}{10}$ of them had diſappeared, and at laſt left reddiſh aſhes amounting to 7,13 grains, nearly 2,7 per cent. Projecting this coal in fine powder on 480 grains of pure ignited nitre, I found the ſalt required 65 grains of the coal to alkalize it, but only 50 grains when in coarſe powder; and in a third experiment, when the crucible was farther from the flue of the ſurface, only 49 grains; ſo that I look upon 50 grains as being in round numbers neareſt to the truth. That is, in the proportion of one part of Kilkenny coal to 9,6 of nitre: or, 100 parts of nitre require for their decompoſition 10,416 of Kilkenny coal.

This proportion of coal is much ſmaller than that of charcoal in Mr. Lavoiſier's experiment, which we have ſeen to be as 1 to 7,57, or as 13,21 to 100, which I attribute to the advantageous mode in which his experiment was inſtituted, as already explained; whereas in mine, and the uſual way, the decompoſition of nitre is promoted by the external heat applied, as well as by the coal, and conſequently leſs of the coal is employed.

From the experiments of Scheele one might be led to infer, that the proportions of charcoal and nitre neceſſary to the alkalization of this latter, approach ſtill nearer to each other

---

* The figures made uſe of by this author to denote degrees of luſtre, tranſparency, hardneſs, and ſharpneſs of fragments, are explained in the following Table, which I have conſtructed from his ſection upon the diſtinctive characters of minerals.

Numbers or Degrees.	Luſtre.	Tranſparency.	Fragments.
0	None.	None.	Perfectly blunt.
1	Exceedingly weak; or a few ſhining particles.	Only at the edges.	Leſs blunt.
2	Gloſſy; not more than of ſilk.	Tranſmits light, but not ſufficient to diſcern objects.	Sharp.
3	Shining; as of cryſtals or of metals not much poliſhed.	Tranſmits light enough to ſhew objects imperfectly through the maſs.	More ſharp.
4	Brighteſt; as of diamonds or poliſhed metals.	Objects clearly diſtinguiſhed through the maſs.	Moſt ſharp; as of glaſs.

On account of the higher range of numbers, degrees of hardneſs could not conveniently be included in the above Table. No. 3, denotes the hardneſs of chalk; 4, a ſuperior hardneſs, but yet what yields to the nail; 5, that which will not yield to the nail, but eaſily and without grittineſs to the knife; 6, that which yields more difficultly to the knife; 7, that which ſcarcely yields to the knife; 8, that which cannot be ſcraped by a knife, but does not give fire with ſteel; 9, that which gives a few feeble ſparks with ſteels, as baſalt; 10, that which gives plentiful lively ſparks, as flint. N.

than in Lavoiſier's ſtatement, and conſequently much nearer than in mine: for, in his Eſſay on Plumbago, he tells us that five parts nitre are ſufficient to conſume one of charcoal, and conſequently it ſhould ſeem that one part charcoal ſhould decompoſe no more than five of nitre. The conſequence however is not juſt, for undoubtedly five parts nitre would conſume one of charcoal, but it does not thence follow that they would not conſume ſtill more. On the other hand, he found that ten parts nitre were neceſſary for the conſumption of one part of plumbago; whence it follows, that one part of plumbago decompoſes ten of nitre, otherwiſe nine parts nitre would ſuffice to conſume it, and the tenth would have been unneceſſary, as it acts only as it is decompoſed. Now this proportion approaches very nearly to my reſults; namely, one of charcoal to 9,6 of nitre.

Hence, and ſince Kilkenny coal in the preceding experiments ſhewed no ſign of its containing any thing bituminous, I take it for granted that it conſiſts almoſt entirely of pure carbon; and ſince 50 grains of it alkalize 480 grains of pure ignited nitre, that in all the ſubſequent experiments on other ſpecies of coals or bitumens free from ſulphur and iron, the decompoſition of this ſtandard quantity of nitre will indicate, in the quantity of coal neceſſary for that decompoſition, the preſence of 50 parts of mere carbon.

Before I proceed to the recital of other experiments, I muſt mention another circumſtance that occurs in making them, which is, that, after the inflammation ceaſes, a hiſſing noiſe is perceived for a long time, and is increaſed on adding freſh quantities of coal, even when the nitre is ſeemingly decompoſed. This ſeemed to me to ariſe from the decompoſition of the nitrous air, or mephitized nitrous acid, of which a portion is always retained by the alkali; and conſequently I paid no attention to it, but always ceaſed adding coal when the inflammation totally ceaſed.

Maltha.

ITS colour is dark-brown, or black.

Luſtre 0. Tranſparency 0. Fracture uneven, tough. Specific gravity 2,070.

It feels ſomewhat greaſy, yields to compreſſion, has a heavy ſmell, acquires a poliſh when ſcraped, does not adhere to the tongue, or ſtain the fingers; its flame high and bright, leaving no coal, but only a little aſhes.

Having but a ſmall quantity of this ſubſtance, I on this occaſion uſed only 240 grains of nitre: when it was heated to redneſs, I threw on it one grain of vegetable pitch; it immediately inflamed, but floated quietly on the ſurface of the nitre, and decrepitated like common ſalt from the moiſture it contained: the flame was partly white from the action of the air ſpontaneouſly emitted by the nitre, and partly yellowiſh from the action of the ambient atmoſpheric air, but ſteady, and unattended with thoſe turbulent guſhes that attend the decompoſition of nitre by carbonaceous ſubſtances.

I then gradually projected on it 55 grains of maltha, which was all I had: this burned juſt as the pitch, but attended with a blacker ſmoke; yet the nitre was ſo far from being alkalized, that, to produce this effect, I was obliged to throw on it 29 grains of cannel coal.

Now 33,5 grains of cannel coal, if it alone had been uſed, would ſuffice to alkalize 40 grains of nitre: it will preſently be ſeen, therefore, the 55 grains of maltha and the one grain of pitch contained no more carbon than 33,5 — 29 = 4,5 grains. Therefore 100 grains of maltha contain no more than 8 grains of carbon. And as theſe 8 grains of carbon provoked no turbulent eruption of air from the nitre, it is plain they did not contribute to its decompoſition, but were taken up by the air it ſpontaneouſly emitted, and partly by the ambient atmoſpheric air.

Aſphalt.

ITS colour is greyiſh black. Luſtre 2.3. greaſy. Tranſparency 0.

Fracture perfectly conchoidal. Hardneſs from 7 to 8, very brittle. Specific gravity from 1,07 to 1,165 by my trials. It feels ſmooth, but not greaſy; has no ſmell, except while pounding; does not ſtain the fingers; when heated it melts, ſwells, and at laſt inflames; but it requires, for inflammation, a higher heat than maltha does, and leaves no coal, and ſcarce any aſhes.

Of this bitumen I found 161 grains requiſite to alkalize the ſtandard quantity of nitre: it viſibly educed air from the nitre; for there were eruptions from time to time. I ſuppoſe when the more oily part was conſumed, and the carbonaceous laid bare, much of the flame was alſo yellowiſh. Hence 161 grains of aſphalt contain only 50 of mere carbon, that is, nearly 31 per cent.

Mr. Thory, burning it in a low heat, found it to leave about of its weight of coal after melting, ſwelling and inflaming as uſual[*]: however, his aſphalt was not perfectly pure, as he obtained ſulphur from it.

Cannel Coal.

ITS colour is black. Luſtre 2. Common when freſh broken, often barely 1. Tranſparency 0.

Croſs fracture conchoidal. Fragments rather ſharp. Hardneſs from 7 to 8. Specific gravity by my trials 1,232, by Dr. Watſon's 1,273; does not ſtain the fingers, eaſily kindles without melting, and burns with a large bright flame, but of ſhort duration, leaving a large coaly reſiduum; does not cake. 240 grains of it, heated until all the coaly part was conſumed, left 7,5 grains of reddiſh brown aſhes, moſtly argillaceous, that is, 3,12 per cent.

66,5 grains of it were ſufficient to alkalize the ſtandard quantity of nitre. It burned with a large bright flame, except the laſt portion, which was yellowiſh, the pure air of the nitre being then exhauſted. Hence 66,5 grains contained 50 of pure carbon, and 2,08 of aſhes; then deducting 52,08 from 66,5, we find the quantity of bitumen equal 14,42. Then 100 parts of it contain 75,2 of carbon, 21,68 bitumen of the ſort called maltha, and 3,1 of aſhes.

I take this bitumen to be maltha, from its quick inflammability, and the ſhort duration and brightneſs of its flame, both which properties indicate the moſt inflammable of the bitumens, and whoſe flame is leaſt durable from its refuſal to cake (caking being a property ariſing from the fuſion of aſphalt), and the difficult combuſtibility of the carbonaceous ſubſtance that remains after the ceſſation of its flame—qualities that counter-indicate aſphalt.

Slaty Cannel Coal.

THAT which I employed was from Ayrſhire in Scotland, the only one of this ſort imported to Dublin.

Its colour is black.

---

[*] 6 Crell's Chym. Jour. 62.

Its luſtre 2. Common. Tranſparency 0.

Its fracture partly ſlaty, partly imperfectly conchoidal; fragments ſharp.

Its hardneſs from 5 to 8. Specific gravity 1,426 by my trials.

It burns like the compact cannel, but ceaſes ſooner to flame; does not cake—leaves a ſtony reſiduum. 240 grains of it, treated as before mentioned, leave 50 of reddiſh grey aſhes, equal 20,83 per cent. From the ſmell that iſſues from it during ignition, I am led to think it contains ſome portion of ſulphur.

To alkalize 480 grains of nitre, 105 grains of this coal were employed. It burned like the former with a large white continued flame, except the laſt portions. Hence this quantity contained 50 grains of mere carbon; and ſince it alſo contained 20,83 of aſhes, the remainder, namely 34,15, muſt have been bitumen. Then 100 parts of it contain 47,62 of carbon, 32,52 of bitumen, and about 20 of aſhes. Some deduction however from theſe quantities of carbon and bitumen may be made by reaſon of the ſmall proportion of ſulphur contained in it. This bitumen I take to be maltha, and not aſphalt, for the reaſons I mentioned in treating of compact cannel.

It is from a coal of this ſort that Lord Dundonald extracts his tar, as maltha eaſily diſtils; but it is probably of a better kind, as this ſtony kind exiſts moſtly in Ayrſhire.

'By his Lordſhip's mode of diſtillation, however, much ſeems to be loſt during the internal combuſtion. I ſhould think the Prince of Naſſau Saarbruck's method in this reſpect more advantageous. Mr. Sage tells us, that, by diſtillation, he obtained from cannel coal $\frac{1}{4}$ of its weight of tar[*]; but Mr. Faujas, who uſes Lord Dundonald's method, obtains from the coal of Decire, which ſeems to be of this kind, only 4 per cent. of tar[†]. Faujas alſo obſerved, that this tar is gradually converted into aſphalt by long expoſure to the air, which confirms the difference I have eſtabliſhed between the two bitumens.

Whitehaven Coal.

ITS colour is black.

Its luſtre 3. Greaſy. Tranſparency 0.

Its fracture plane foliated. Its fragments 2, often diſcovering quadrangular or cubic diſtinct concretions, ſometimes interſected with browniſh red flakes.

Its hardneſs 6, very brittle. Specific gravity 1,257 by my trials. Stains the fingers, particularly when moiſt.

It burns at firſt with a clear flame, and for a long time; but at laſt cakes. 240 grains of it, after five hours ſtrong heat, left only 4 grains of reddiſh aſhes, or about 1,7 per cent.

The ſtandard quantity of nitre was alkalized by 88 grains of this coal. Hence 100 grains of it contained nearly 57 of mere carbon, 41,3 of a mixture of maltha and aſphalt, and 1,7 of aſhes. That it contains both maltha and aſphalt is evident from its flame and caking. The proportion I cannot exactly aſcertain, but moſt probably the aſphalt predominates.

Wigan Coal.

ITS colour is black.

Its luſtre 3. Greaſy. Tranſparency 0.

Its fracture plane foliated. The lamellæ, ſome uniformly, ſome promiſcuouſly directed. In the groſs often ſlaty; forms ſeparate concretions, often with bright yellowiſh illinitions.

---

[*] Roz. Journ. p. 387.

[†] Roz. Journ. p. 188.

Its hardneſs 6. Specific gravity 1,268 by my trials. It burns with a bright flame and quicker than the foregoing, and is leſs apt to cake. 328 grains of it, expoſed as the former to a ſtrong heat, left 5,13 grains of aſhes, that is, 1,57 per cent. 81 grains of it decompoſed 480 grains of nitre. Hence 100 grains of it contain 61,73 of carbon, 36,7 of a mixture of maltha and aſphalt, and 1,57 of aſhes.

It ſeems to contain a larger proportion of maltha, with reſpect to its quantity of aſphalt, than Whitehaven coal does.

Swanſey Coal.

ITS colour is black.

Its luſtre 2. Tranſparency 0.

Its fracture foliated; but ſome lamellæ being at right angles with the other, give it a fibrous or ſtriated appearance. Fragments 2.

Its hardneſs 5, very brittle. Specific gravity 1,357 by my trials.

It burns more ſlowly than the former, and cakes.

240 grains of it, treated as the former kinds, left 8 grains of yellowiſh red aſhes; that is, equal 3,33 per cent.

Of this coal 68 grains were requiſite to decompoſe 480 grains of nitre. Then 100 grains of it contain 73,53 of carbon, 23,14 of a mixture of maltha and aſphalt, and 3,33 of aſhes. The aſphalt ſeems to predominate.

Leitrim Coal.

ITS colour is black.

Its luſtre when freſh broken 3. Tranſparency 0.

Its fracture foliated. Its fragments 2.

Its hardneſs 6, very brittle. Specific gravity 1,351 by my trials. It ſlightly cakes.

240 grains of it left, after three hours expoſure to heat, 12,5 grains of reddiſh grey aſhes, that is, equal 5,2 per cent.

The decompoſition of the ſtandard quantity of nitre required 70 grains of this coal. Hence 100 grains of it contain 71,43 of carbon, 23,37 of a mixture of maltha and aſphalt, and 5,2 of aſhes.

Newcaſtle Coal.

I HAD none of this kind of coal; but, according to Dr. Watſon's experiment, it left on diſtillation a coaly reſiduum amounting to 58 per cent. and hence contained about 40 of a mixture of aſphalt and maltha, in which the former appears to predominate. Hence it much reſembles the Whitehaven coal; but it evidently contains ſulphur alſo, which that of Whitehaven ſeldom does.

A ſynoptical View of the Contents of Bitumens and different Sorts of Mineral Coal.

100 parts	Carbon.	Bitumen.	Aſhes.	Specific gravity.
Maltha	8	—	—	2,070 *
Aſphalt	31	68	—	1,117
Kilkenny	97,3	—	3,7	1,526
Compact cannel	75,2	21,68 maltha	3,1	1,232
Slaty cannel	47,62	32,52 maltha	20	1,426
Whitehaven	57	41,3 mixt	1,7	1,257
Wigan	61,73	36,7 mixt	1,57	1,268
Swanſey	73,53	23,14 mixt	3,33	1,357
Leitrim	71,43	23,37 mixt	5,20	1,351
Newcaſtle	58	40 mixt	—	1,271

To theſe reſults I ſhall add a few more taken from a Treatiſe on Pit Coal, lately publiſhed by Signior Fabroni. The Italian coals were examined by himſelf; the French and German by other chemiſts—All by diſtillation.

100 parts	Carbon.	Bitumen.	Aſhes.	Specific gravity.
Coals of Halles	86	12	—	—
of Tudertino	25	75	—	—
of Cortolla	45	43	12	1,403
of Macinaia	60	37	3	1,411
Stony of ditto	12,5	37,5	50	1,666
of Mocaio	32	35	33	1,403

Theſe coals contain very little aſphalt, but chiefly maltha.

Moſt coals afford a volatile alkali by diſtillation:—this ſeems to me to be rather a product of the operation ariſing from the union of hydrogene and mephitic air, and thus the alkaline baſis of the ammoniacs found on volcanos ſeems to have been formed. Coals alſo afford an acid, commonly the marine, or if pyritous, alſo the vitriolic, more rarely the ſuccinous.

According to Mr. Jars, 100 parts of the beſt Engliſh coal give when charred 63 of coaks †; but Hielm found the reſiduum of the beſt Engliſh coals diſtilled, to amount to 73 per cent. and Dr. Watſon found the reſiduum of Newcaſtle coal to amount only to 58 per cent. Theſe reſults neceſſarily differ according to the degree of heat applied, the duration of the combuſtion, and the variable admiſſion of air. It is plain the bitumen is never totally expelled, at leaſt not until moſt of the carbon is conſumed; but much more of it is expelled by combuſtion than by diſtillation. Watſon, p. 27 and 28 ‡.

---

* Probably 1,07.

† 1 Jars, 319.

‡ This paper is taken from the Second Volume of Elements of Mineralogy, lately publiſhed.

III. *Obſervations*

III.

Obſervations on the beſt Methods of producing Artificial Cold. By Mr. RICHARD WALKER *.

HAVING already inveſtigated the means of producing artificial cold, and at the concluſion of my laſt paper (on the Congelation of Quickſilver) diſmiſſed that part of the ſubject, the beſt method of making uſe of thoſe means naturally becomes a deſideratum; to that therefore I have lately given my attention, and flatter myſelf that the following obſervations may be conſidered as an uſeful Appendix to my former papers. The freezing point of quickſilver being now as determined a point on the ſcale of a thermometer, viz. 39°, as the freezing point of water; and as this metal, exhibited in its ſolid ſtate, affords an intereſting as well as curious phenomenon, I ſhall apply what I have to ſay principally to that object.

Frequent occaſions having occurred to me of obſerving the ſuperiority of ſnow in experiments of this kind to ſalts, even in their fitteſt ſtate, that is, freſh cryſtalliſed, and reduced to very fine powder, I reſolved upon adopting a kind of artificial ſnow.

The firſt method which naturally preſented itſelf was by condenſing ſteam into hoar froſt. This anſwered the purpoſe, as might be expected, exceedingly well; but the difficulty and expence of materials in collecting a ſufficient quantity determined me to relinquiſh this mode for another, by which I can eaſily and expeditiouſly procure ice in the fitteſt form for experiments of this kind; the method I mean is, by firſt freezing water in a tube, and afterwards grinding it into very fine powder. Thus poſſeſſed of the power of making ice, and afterwards reducing it to a kind of ſnow, the congelation of quickſilver becomes a very eaſy and certain proceſs; for, by the uſe of a very ſimple apparatus, (Plate XX. fig. 1.) quickſilver may be frozen perfectly ſolid in a few minutes, wherever the temperature of the air does not exceed 85°: thus, one ounce of nitrous acid is to be poured into the tube *b* of the veſſel, obſerving not to wet the ſide of the tube above with it; a circular piece of writing-paper, of a proper ſize, is to be placed over the acid, reſting upon the ſhoulder of the tube, and the paper bruſhed over with ſome melted white wax. Thus prepared, the veſſel is to be inverted, and filled with a mixture of diluted nitrous acid, phoſphorated ſoda, and nitrous ammoniac, in proper proportions for this temperature *, and tied over ſecurely, firſt with waxed paper, and upon that a wet bladder.

The veſſel being then turned upright, and placed in a ſhallow veſſel, viz. a ſaucer or plate, an ounce and a half of rain or diſtilled water is to be poured into the tube, which is to be covered with a ſtopper or cork, and, as ſoon as frozen ſolid, ground to very fine powder, an aſſiſtant holding it firmly and ſteadily the while; obſerving occaſionally to work the inſtrument in different directions up and down, that no lumps may be formed. When the whole of the ice is thus reduced to powder, and the lumps, if any, broken, the frigorific mixture is to be let out quickly, by cutting or untying the ſtring, and removing the bladder, &c. which confines it; a communication made by forcing a rod of glaſs or wood through the partition; and the whole mixed expeditiouſly together.

---

* Philoſophical Tranſactions, 1795.

† I have, by a very accurate preparation of this mixture, ſunk a thermometer from 85 degrees (temperature of the veſſel and materials) to + 2 degrees.

In this climate a mixture much leſs expenſive will be ſufficient; viz. that compoſed of diluted nitrous acid, Glauber's ſalt, ſal-ammoniac and nitre; a mixture of this kind ſinking a thermometer in the warmeſt weather to near 0°. At the temperature of 70 degrees, or a little higher, the quantity of diluted nitrous acid may be about one-fourth leſs than is mentioned in the table for 50 degrees.

Theſe methods are the moſt expeditious, and attended with the leaſt trouble; but as ice may be uſed with equal certainty, and with much leſs expence, I ſhall give a particular detail of an experiment made with the uſe of it, firſt mentioning a preparatory experiment, to which I was immediately led by the recollection that Sir Charles Blagden, in his paper "on the Point of Congelation," (Phil. Tranſ. vol. lxxviii.) had found that common ſal-ammoniac and common ſalt mixed with ſnow produced a cold of — 12 degrees, whereas the latter uſed alone with ſnow produces only — 5 degrees. I uſed a mixed powder of equal parts of common ſal-ammoniac and nitre with the common ſalt, by which the thermometer ſunk to — 18 degrees; and when I uſed nitrous ammoniac with common ſalt, to — 25 degrees. This cold I could not increaſe by the addition of any other ſalts, nor could I equal it by any other combination of ſalts. Thoſe I tried were, Glauber's ſalt, ſalt of tartar, ſoda, and ſal catharticus amarus. By ſeveral trials, I found the beſt proportions to be, ſnow or pounded ice twelve parts, common ſalt five parts, and of nitrous ammoniac, or a powder of equal parts, ſal-ammoniac and nitre mixed, five parts; or one-third of common ſalt, when I uſed that alone with ſnow or pounded ice.

My apparatus then (Dec. 28. laſt) conſiſted of two veſſels (fig. 3 and 4.); an inſtrument (fig. 6.) to grind or rather ſcrape the ice to powder; a kind of ſpatula (I uſe a marrow ſpoon) to ſtir the powder occaſionally; a thermometer (fig. 8.) and a ſmall thermometer glaſs, with the bulb three-fourths full of quickſilver (fig. 7.) I filled the veſſel fig. 3, holding when inverted two pints, *ſtratum ſuper ſtratum*, with pounded ice, common ſalt, and a powder conſiſting of equal parts ſal-ammoniac and nitre mixed together; by firſt putting in ſix ounces of pounded ice, then two ounces and a half of common ſalt, and, after ſtirring theſe well together, two ounces and a half of the mixed ſalts, mixing the whole well together: this was repeated in the ſame manner until the veſſel was quite full. It was then tied over ſecurely with a wet bladder, turned upright, and one ounce and a half of rain water poured into the tube through a funnel, the tube covered with a cork, and the veſſel left undiſturbed till the water was frozen perfectly ſolid. The inſtrument for grinding it was then put in to acquire cold, whilſt the veſſel fig. 4, holding a pint, was filled in the ſame manner, with the ſame proportions of materials; a bladder tied over it, ſet upright, and one ounce of fuming nitrous acid poured in to be cooled. The ice was then ground to powder; and when finiſhed, the nitrous acid being found to have acquired a ſufficient degree of cold, viz. — 13 degrees, the frigorific mixture of ice and ſalts was let out of the veſſel which contained the nitrous acid, and the powdered ice (ſtill ſurrounded by its frigorific mixture) added to the acid as quick as poſſible; when the thermometer ſunk to near — 50 degrees, and the mixture ſoon froze the quickſilver in the glaſs bulb. In this experiment 18 minutes were required to freeze the water perfectly ſolid, and 15 to reduce the ice by moderate labour to very fine powder. The experiment was over in 55 minutes, and the temperature of the preparatory cooling mixture then found to be — 10 degrees.

I had a ſpirit thermometer by me; but a mercurial thermometer being much more ſenſible, and conſequently deſcending much quicker, I prefer it in experiments made merely to freeze quickſilver—knowing from experience how the congelation is going on, from the irregular deſcent of the mercury when a few degrees below its freezing point, and from having uſually found that the quickſilver in the thermometer glaſs begins to freeze as ſoon as the mercurial thermometer reaches — 40°.

Whenever I have occaſion to uſe ice in ſummer for this purpoſe, I uſually pound together firſt ſome ice and ſalt in a ſtone mortar, about two parts of the former to one of the latter; throw this away, and wipe the peſtle and mortar perfectly dry: the mortar being thus cooled, the ice may afterwards be pounded ſmall without melting.

And as a mixture made of ſnow or ice in powder and ſalts does not give out its greateſt cold till it is become partially liquid by the action of the ice and ſalts on each other, it is neceſſary that the whole be ſtirred well together, till it is become of an uniformly moiſt pulpy conſiſtence; eſpecially ſince, in becoming liquid, the mixture ſhrinks ſo much, that, if this be not attended to, the veſſel will not be near full, and conſequently the upper part of the tube not ſurrounded, as it ought to be, by the frigorific mixture. The diſſolution of the ice and ſalts may, if required, be haſtened, by adding occaſionally a little water; but then the cold produced will be leſs intenſe, and not ſo durable.

That particular form of the veſſel in which the ice is made and reduced to powder is choſen, becauſe it ſubjects the powdered ice in the tube to the conſtant action of the freezing mixture, without which it would be leſs fit, particularly in warm weather, for the intended uſe; and becauſe in it the ice is not liable to be impregnated with the ſalts of the mixture, by which it would be utterly ſpoiled; and that for cooling the nitrous acid, and making the ſecond mixture in, becauſe it is ſteady, and is beſides inſulated, as it were, from the external warm air, and ſurrounded in its ſtead by an atmoſphere much colder.

It is ſcarcely neceſſary to add, that when ſnow which has never thawed can be procured, it may be cooled in this apparatus by a mixture of ſnow (inſtead of the pounded ice) and the ſalts, and the trouble of reducing the ice into powder ſaved.

I prefer the red fuming nitrous acid, becauſe, as I have obſerved in a former paper, it requires no dilution. Being under the neceſſity at one time of uſing the pale nitrous acid, I found it required to be diluted with one-fifth its weight of water. The beſt and only way of trying or reducing any acid to the proper ſtrength, is by adding ſnow, as Mr. Cavendiſh directs, or the powdered ice to it, until the thermometer ceaſes to riſe; then cool the acid to the ſame temperature of the ſnow again; add more ſnow, which will make the thermometer riſe again, though leſs; cool it again, and repeat this until the addition of ſnow or powdered ice will not make the thermometer riſe: to be very accurate, it ſhould be reduced in this manner to the proper ſtrength, at the temperature, whatever it be, at which the nitrous acid and ſnow, or powdered ice, are to be mixed together when cooled.

In the courſe of my experiments I have endeavoured to aſcertain the comparative powers of ice to produce cold with nitrous acid in the different forms I have had occaſion to uſe it. The reſult is, that freſh ſnow ſunk a thermometer to — 32 degrees, ground ice to — 34 degrees, and the moſt rare frozen vapour to below — 35 degrees; the veſſel and materials each time being + 30 degrees.

The

The veſſels for theſe mixtures, particularly that in which the quickſilver is to be frozen, ſhould be thin, and made of the beſt conductors of heat; firſt, becauſe thin veſſels rob the mixture of leſs cold at mixing, that is, if two mixtures of the ſame kind are made, one in a thin, the other in a thick veſſel, the former will be coldeſt; ſecondly, becauſe the air is a ſufficiently bad conductor; and thirdly, for the very obvious reaſon that the cold is tranſmitted through them quicker.

For theſe reaſons, and from the difficulty I have found in procuring veſſels of glaſs, which are undoubtedly fitteſt for experiments of this kind, I have uſed tin, which is readily had in any form, and, if coated with wax, is ſufficiently ſecured againſt the action of the acids.

I give the inſide ſuch a coating by pouring melted white wax into the veſſel, previouſly clean and dry, and turning it about by hand, ſo as to leave no point of the metal uncovered for the acid to act on, pouring the ſurplus away.

In the experiments above deſcribed, I uſed a ſingle veſſel for cooling the nitrous acid; a cupping-glaſs (repreſented by the dotted line at *b* fig. 4.) being cemented into the tin, and thereby forming that part in which the nitrous acid was firſt cooled, and the mixture afterwards made in which the quickſilver was frozen; but from the trouble and impediments ariſing from letting out the mixture, and clearing the bottom from the lumps of ice, &c. adhering to it, I was led to the addition of the other part (fig. 5.) by which all theſe difficulties are got rid of: and it is beſides a much more comfortable and neat way of conducting it; the upper part, which contains the nitrous acid, being lifted off, and placed on the table immediately before the powdered ice is added.

The whole of this apparatus may be of tin; that part only (when the cooling mixtures are made without uſing any corroſive acid) in which the acid mixture is to be made being previouſly coated in the manner above mentioned; or a thin glaſs tumbler, of a proper ſize, may be cemented in.

I have occaſionally uſed a thin glaſs tumbler for the mixture in which the quickſilver is to be frozen, immerſing it, with the acid, in a frigorific mixture, till the acid is ſufficiently cooled; then adding the ground ice to it, previouſly removing the tumbler out of the frigorific mixture, as in the experiment above mentioned: this ſimplifies the apparatus, but is leſs convenient on many accounts.

The ſcale of this apparatus may be diminiſhed or increaſed at the will of the operator; for there is no doubt that a ſmall quantity of quickſilver may be frozen at any time with one-fourth of this quantity, with an apparatus of this kind, by any one converſant in ſuch experiments.

I have frequently frozen quickſilver by mixing together at 0°, three drams of ground ice with two drams of nitrous acid.

Whenever the intention is, as in theſe experiments, to cool the materials to nearly the ſame temperature with the frigorific mixture in which they are immerſed, the proportion of the frigorific mixture to the intended mixture (or materials to be cooled) ſhould not be leſs than twelve to one; a greater diſproportion is ſtill better.

By attending to the directions particularly mentioned in the experiment made on December 28, a thermometer may be always diſpenſed with; the proportions of the materials to be cooled being exactly adjuſted, and, when they are to be mixed, preciſely determined by the time employed in grinding the ice to powder. The proportions of ſnow or pounded ice and ſalt or ſalts may be gueſſed ſufficiently near without weighing, unleſs in very nice experiments.

Imagining that a recapitulation of the different mixtures deſcribed in my former paper for producing artificial cold, brought into one view, might not be unuſeful, I have ſubjoined a table of the ſalts, their powers of producing cold with the different liquids, and the proportions of each, according to a careful repetition of each; the temperature being 50 degrees.

SALTS.	Liquor.		Temperature of Cold produced.
* Sal-ammoniac 5, nitre 5	Water	16	— + 10°
Sal-ammoniac 5, nitre 5, Glauber's ſalts 8	——	16	— + 4
* Nitrous ammoniac 1	——	1	— + 4
Nitrous ammoniac 1, ſal ſoda 1	——	1	— 7
Glauber's ſalt 3	D. nitr. acid	2	— 3
Glauber's ſalt 6, ſal-ammoniac 4, nitre 2	——	4	— 10
Glauber's ſalt 6, nitrous ammoniac 5	——	4	— 14
Phoſphorated ſoda 9	——	4	— 12
Phoſphorated ſoda 9, nitrous ammoniac 6	——	4	— 21
Glauber's ſalt 8	Marine acid	5	— 0
Glauber's ſalt 5	D. vitr. acid	4	— + 3

N. B. I have choſen the temperature of 50 degrees, becauſe the materials may at any time, by immerſion in water drawn from a ſpring, be cooled nearly to that temperature, and the experiment for freezing with any of theſe mixtures commence there.

At a higher temperature than 50 the quantity of the ſalts muſt be increaſed, and the effect will be proportionably greater: at a lower temperature diminiſhed, when the effect will be proportionably leſs.

It muſt be obſerved, that, to produce the greateſt effect by any frigorific mixture, the ſalts ſhould be freſh cryſtallized †, not damp, and newly reduced to very fine powder; the veſſel in which they are made, very thin, and juſt large enough to contain the mixture; and the materials mixed intimately together as quickly as poſſible, the proper proportions at any temperature (thoſe in the table being adjuſted for the temperature of 50° only) having been previouſly

---

* The ſalts from each of theſe may be recovered by evaporating the mixture to dryneſs, and uſed again repeatedly.

N. B. The figures after each ſalt, and after the liquor, ſignify the proportion of parts by Troy weight to be uſed; the trouble of weighing the water may be ſaved by obſerving, that a full ounce of it by wine meaſure, correſponds exactly with one ounce of it by Troy weight: likewiſe it muſt be noticed, when more kinds of ſalts than one are uſed, to add them to the liquor one after the other in the order they ſtand in the table, beginning on the left hand, and ſtirring the mixture well between each addition. D. nitr. acid is red fuming nitrous acid two parts, and rain or diſtilled water one part by weight, well agitated together and become cool. D. vitr. acid is ſtrong vitriolic acid, and rain or diſtilled water equal parts by weight, thoroughly mixed (very cautiouſly) and cooled.

† Soda, phoſphorated ſoda, and Glauber's ſalt, are beſt cryſtallized afreſh, becauſe their effect, eſpecially the two laſt, in the acids, depends upon the quantity of water they contain in a ſolid ſtate.

previouſly tried, by adding the powdered ſalts gradually to the liquid till the thermometer ceaſed to ſink; obſerving to produce the full effect of one ſalt before a ſecond is added, and likewiſe of the ſecond before a third is added. Neither ſoda, phoſphorated ſoda, nor Glauber's ſalt, ſhould be mixed with nitrous ammoniac, or the powder compoſed of ſal ammoniac and nitre, unleſs at a low temperature, that is, below 0; but pounded and kept apart.

In the experiments alluded to in the table, the precaution of freſh-cryſtallizing the ſalts was not obſerved, becauſe I choſe to give the ordinary effects only. I therefore then uſed ſalts in their common ſtate, taking care, however, to chooſe ſuch as had not in the leaſt effloreſced.

Since it is always uſeful, and generally abſolutely neceſſary, to know how much room in a veſſel the ſeveral materials take up ſeparately; and when mixed, it will be right to obſerve, that ſnow or ice in powder, at near 0°, occupy in meaſure nearly two thirds more than their weight; that is, one ounce weight of water will, when in the form of ſnow, or ice ground to powder, nearly fill a veſſel which holds three ounces wine meaſure; powdered ſalts, nearly double their weight; ſtrong nitrous acid, about three-fourths its weight; and a mixture made of ſalts and diluted nitrous acid; meaſures rather leſs than two-thirds of the weight of the ingredients. Without a previous knowledge of this, it is impoſſible to adjuſt the ſize of the veſſels to the mixtures which are to be made; becauſe, in moſt nice experiments of this kind, the height to which a veſſel will be filled is indiſpenſably neceſſary to be known beforehand.

The long continuance of the late froſt having afforded me opportunities of repeating theſe experiments in various ways, I ſhall mention briefly the reſult of ſuch as appear to me to be material.

I have found, that ice may be ground ſo fine as to be equal to frozen vapour; and the harder it is frozen, the finer it is ground, but with more labour.

That quickſilver may be frozen by cooling the nitrous acid only, ſaving the trouble and inconvenience of cooling the ſnow likewiſe; either by adding ſnow at + 32 degrees, to nitrous acid at — 29 degrees; or ſnow at + 25 degrees, to nitrous acid at — 20 degrees; or ſnow at + 20 degrees, to nitrous acid at — 12 degrees. Moſt winters offer an opportunity of doing it in this way; the nitrous acid may be cooled in a mixture of ſnow and nitrous acid.

That it may likewiſe be frozen by mixing expeditiouſly together ſnow and nitrous acid, when the temperature of each is + 7 degrees.

Or by mixing ground ice and nitrous acid at + 10 degrees.

Hence it follows, that the cold of this climate offers occaſionally opportunities of freezing quickſilver, without previouſly cooling by art the materials to be mixed; for I have once ſeen the thermometer at + 6 degrees—and others, I believe, have ſeen it lower.

I expected an opportunity would have offered this winter, but the loweſt point I ſaw my thermometer at this ſeaſon was only + 10 degrees. At this temperature I mixed nitrous acid (cooled out-of-doors to the temperature of the air) and ſnow on January 23d laſt; but the cold produced was not quite ſufficient to freeze the quickſilver, although very near it, as indicated by a thermometer. From what I have obſerved ſince theſe latter experiments were made, I think it may be reaſonably expected that powdered ice and nitrous acid at + 14 degrees, or ſnow at + 10 degrees, will ſucceed, if mixed expeditiouſly.

Strong ſpirit of vitriol, whoſe ſpecific gravity is 1,848, required to be diluted with half its weight of water, and produced with ſnow at the temperature of + 30 degrees, about eight degrees leſs than with nitrous acid, ſinking the thermometer to — 24 degrees; four parts of the diluted vitriolic acid required at that temperature ſix parts of ſnow.

It perhaps will be remarked, that I have taken no notice before of the vitriolic acid. The reaſon is, becauſe, the freezing point of quickſilver being 39, it may be frozen tolerably hard by a mixture of nitrous acid with ſnow or ground ice, though the utmoſt degree of cold this acid can produce with ſnow is — 46°, which degree of cold may be produced by mixing the ſnow or ground ice and nitrous acid at 0°.

If it be required to make it perfectly ſolid and hard, a mixture of equal parts of the diluted vitriolic acid and nitrous acid ſhould be uſed with the powdered ice; but then the materials ſhould not be leſs than — 10° before mixing.

If a ſtill greater could be required than a mixture of this kind can give, which is about — 56 degrees, the diluted vitriolic acid alone ſhould be uſed with ſnow or powdered ice, and the temperature at which the materials are to be mixed not leſs than — 20 degrees.

Select, according to the intention, either of the three following mixtures:

Firſt, ſnow or pounded ice two parts, and common ſalt one part, which produces a cold of — 5 degrees.

Second, ſnow or pounded ice twelve parts, common ſalt five parts, and a powder, conſiſting of equal parts of common ſal-ammoniac and nitre mixed, five parts, which produces a cold of — 18 degrees.

Third, ſnow or pounded ice twelve parts, common ſalt five parts, and nitrous ammoniac in powder five parts, which produces a cold of — 25 degrees.

The proportions which I have found to be the beſt for mixing the ſnow or powdered ice with the different acids at different temperatures are theſe: viz. at + 30 degrees, ſeven of the former to four of the nitrous acid; at + 5 degrees, (with a trifling allowance, if any, for a few degrees above or below) three to two; at — 12 degrees, four to three, with the mixed acids, and at — 20 degrees, with the diluted vitriolic acid, equal parts.

If it be required to prepare the materials in a frigorific mixture without the uſe of ice, a mixture of the proper ſtrength may be choſen from the table.

It is immaterial, when the exact proportions of each are known, whether the powdered ice be added to the acid, or the acid poured upon that, provided the powdered ice be kept ſtirred to prevent lumps forming, and the materials be mixed as quick as poſſible. But when the proportion is not known, it is better to be provided with more powdered ice than is expected to be wanted, and add it to the acid by degrees, until the greateſt effect is produced, as ſhewn by a thermometer.

The conſiſtence is a pretty ſure guide to thoſe accuſtomed to mixtures of this kind, viz. when freſh additions of ſnow or ice do not readily diſſolve in the acid, though well ſtirred, and the mixture acquires a thickiſh flocculent appearance.

Snow or powdered ice that have ever been ſubjected to a cold leſs than freezing are ſpoiled, or rendered much leſs fit for experiments of this kind.

I prefer the method of adding the powdered ice or ſnow to the acid in a ſeparate veſſel, principally becauſe the ſize of that veſſel may be exactly adjuſted to the quantity of mixture it is to contain.

A mixture made of diluted nitrous acid, phoſphorated ſoda, and nitrous ammoniac, (by much the moſt powerful of any compounded of ſalts with acids,) prepared with the greateſt accuracy, is not quite equal to a mixture of ſnow and nitrous acid, each mixed at + 30 degrees, although very nearly ſo.

Though quickſilver may be frozen by ſalts diſſolved in acids, it is neceſſary that the materials be cooled previouſly to mixing, much lower than when ſnow or ground ice are uſed.

If it be required to mix the powdered ſalts and acids at a low temperature, the beſt method is this:—Put firſt the nitrous ammoniac into the tube of ſuch an apparatus as fig. 1, ſhaking it down level, gently preſſing the upper ſurface ſmooth; then the phoſphorated ſoda or Glauber's ſalt: cover this with a circular piece of writing paper, and pour a little melted white wax upon it, and when cold pour upon this the diluted nitrous acid: immerſe this in a frigorific mixture till it is ſufficiently cold, as found by dipping the thermometer into the liquor occaſionally; force a communication through, and ſtir the whole thoroughly together, contriving that the upper ſtratum of ſalt, that is, the phoſphorated ſoda or Glauber's ſalt, be mixed with the liquor firſt, and then the nitrous ammoniac; the powdered ſalts do not require ſtirring whilſt cooling, like ſnow, for, however hard they are frozen, they will readily diſſolve in the acid: care muſt be taken that the partition be perfect between the ſalts and the liquor, and that in this and every inſtance where the materials are to be cooled they be immerſed below the ſurface of the frigorific mixture. The ſtrength of the red fuming nitrous acid uſed in theſe experiments I found to be 1,510, and that of the vitriolic acid 1,848.

I have thought it better, for the ſake of brevity, not to uſe in this, as in my former papers, the new chemical names, eſpecially as the old ones are more generally known.

Theſe experiments were chiefly made in a warm room, not far from the fire-ſide.

I have now finiſhed my propoſed plan reſpecting the beſt modes of conducting experiments on cold; in which it will appear, that I have reduced the congelation of quickſilver, in any climate, at any ſeaſon, to a certain and almoſt as eaſy a proceſs as that I originally ſet out with for the freezing of water, (Phil. Tranſ. vol. LXXVII.) viz. by previouſly cooling the materials in one mixture, to produce the effect in a ſecond. It may very likely appear to ſome, that I have been too minute in a few particulars; yet, as perhaps experiments of this kind, all circumſtances conſidered, are inferior to few in the delicacy required to make them ſucceed completely, I truſt I ſhall be excuſed by thoſe who chooſe to repeat them, particularly ſuch as are not in the habit of making experiments of this kind—eſpecially if it ſecure them from an unſucceſsful attempt, and that perhaps without being able to account for it.

Oxford, March 1, 1795.

It is very well known, that vitriolic ether will produce ſufficient cold by evaporation to freeze water. This circumſtance is noticed by many; and ſeveral different methods have been propoſed, particularly one by Mr. Cavallo, with a very ingenious apparatus for the purpoſe (Phil. Tranſ. vol. LXXI.); nevertheleſs, as I am upon the ſame ſubject, and the following experiments differ, as well in the effect produced, as in the particular mode of conducting them, from any I have met with, I have ventured to mention them.

June 29, 1792, temperature of the air 71 degrees, I ſunk a thermometer (the bulb being covered with fine lint tied over it and clipped cloſe round) by dipping it in ether, and ſanning it to 26 degrees; then by expoſing the thermometer to the briſk thorough air of an open window, to 20 degrees; and again, by uſing ſome of the ſame ether, but which had been purified by agitating it with eight times its weight of water, applied exactly as in the laſt experiment, the thermometer ſunk to 12 degrees. Water tried in the ſame manner at the ſame temperature, ſunk the thermometer to 56 degrees.

A whirling motion was given the thermometer during each experiment.

The lint was renewed for each experiment, and the bulb required to be dipped into the ether thrice; the firſt time ſufficiently to ſoak it; after which the thermometer was held at the window till it ceaſed to ſink; then a ſecond quick immerſion, and likewiſe a third, expoſing the thermometer in like manner after each immerſion.

In this manner, a little water in a ſmall tube may be frozen preſently, by good ether not *purified*, at any time, eſpecially if a ſmall wire be uſed to ſcratch or ſcrape the ſides of the tube below the ſurface of the water.

During the warmeſt weather of laſt ſummer, I frequently froze water in this way.

Explanation of Plate XX.

FIG. 1. is a veſſel in one piece, open at the bottom; *a a* the body, holding, when inverted, two pints; *b* the tube, holding five ounces; the lower or ſmaller part (formed by a contraction or leſſening of the tube in diameter, merely for the purpoſe of leaving a ſmall ſhoulder for a temporary partition), holding rather leſs than one-fifth of the whole.

Fig. 2. is a veſſel conſiſting of two parts; *a a* the body, holding two pints; *b* the tube, holding five ounces; which, together with the lid *c*, forms a cover to take off and on the veſſel.

N. B. This veſſel may, if preferred, be uſed inſtead of fig. 1. the parts correſponding with it, except in not being open at bottom, and the continuation of the tube upwards juſt ſufficient to ſerve for a handle.

Fig. 3. is a veſſel in one piece, open at the bottom, holding, when inverted, two pints; *b* the tube, holding four ounces and a half.

Fig. 4. a veſſel open at bottom, holding, inverted, one pint.

Fig. 5. a cover to fig. 4. *a a* the body, fitting exactly over, and *b* the cup-part (holding three ounces) fitting exactly within the correſponding parts of fig. 4.

Fig. 6. the inſtrument for grinding the ice into powder; it works upon a ſhort centre-point, and has the edge bevilled contrary ways on each ſide the point, ſo as to follow. The fineneſs of the powder is regulated by the degree of preſſure uſed. The handle is wood, the reſt metal; *a* is a ſliding cover fitting on the tube in which the ice is ground, to exclude the external air, and to keep the inſtrument ſteady; *b* is the ſhoulder or guard, to prevent the point of the inſtrument from touching ſo as to endanger injuring the bottom of the tube. It ſhould be made ſo as to fit without grating the inſide of the tube in uſing.

The tubes of each of the veſſels ſhould be ſomewhat ſhorter than the veſſel, ſo as not quite to reach the bottom of it.

Fig. 7. a thermometer-glaſs, with the bulb three-fourths full of quickſilver.

Fig. 8. a thermometer with the lower part of the ſcale-board turned up with a hinge, for the convenience of taking the temperature of ſmall quantities, or of mixtures in which mineral acids form a part.

N. B. Theſe veſſels are repreſented as in glaſs, that being undoubtedly fitteſt for purpoſes in which corroſive acids are to be uſed.

IV.

The Deſcription of a New Portable Electrical Machine. Invented by the Rev. W. PEARSON, *of Lincoln.*

AN electrical machine ſufficiently portable, and at the ſame time poſſeſſing power adequate to all the purpoſes of medical electricity, has long been a deſideratum among thoſe profeſſional gentlemen who attend the infirm, and are perſuaded that electricity has a ſanative influence in many diſorders of the human body. The electrophore and medical bottle, as alſo the prepared ribbon and apparatus attached thereto, do both poſſeſs the former property, but are found to be greatly deficient in the latter. Mr. Colman [*], in his ingenious treatiſe on the means proper to be uſed for reſtoring ſuſpended reſpiration, propoſes an electrical machine (ſuch as is both portable, and likewiſe poſſeſſes power ſufficient to give a ſhock of 35 inches of coated ſurface at about a quarter of an inch ſtriking diſtance) to conſtitute one important article of his improved apparatus for the recovery of perſons apparently drowned or ſuffocated; but ſeems to lament that we have no machine anſwering ſuch a deſcription. A conſideration of ſuch magnitude as the want of an inſtrument which promiſes to benefit ſociety in ſo critical a moment as is that of the *apparent extinction of the vital principle*, has turned the mind of the writer of this article frequently upon the ſubject, as having ſome relation to his clerical function: he has in theory projected various conſtructions of a portable machine, which mature conſideration convinced him would be objectionable in practice. For to anſwer all the purpoſes requiſite for conſtituting an electrical machine ſuitable for Mr. Colman's purpoſe, it appears neceſſary not only that it have the two deſired properties of portability and power, but alſo that its power be guarded ſo as not to be annihilated by the moiſture when uſed in the open air; that it be capable of being uſed without a table or ſtand in caſes of neceſſity; that it be rendered fit for uſe upon the ſhorteſt notice; that when uſed it charge the coated ſurface without conſiderable loſs of time; and laſtly, that it be not liable to be eaſily broken.

The writer, however, flatters himſelf he has at length hit upon a conſtruction which comprehends all theſe properties, and which he recommends to the notice of inſtrument-makers, from a conviction that it is practicable. Indeed he has conſtructed more than one machine, which anſwer his moſt ſanguine expectation, and which therefore he will the more circumſtantially deſcribe.

Fig. 1. Plate XXI. repreſents a view of the machine with the eye placed directly over it at a ſmall diſtance; A B C D is a ſmall mahogany box 9½ by 7½ inches within, and 6 inches

deep, made of half-inch board: upon the bottom of the box lies a plate of glaſs, alſo 9½ by 7½ inches, cemented to the wood at the four corners by common electrical cement; this plate is coated on both ſurfaces with tin-foil 7 inches by 5, as repreſented at Q in figure 2 (except that the ſlips of tinfoil at H and I are paſted or gummed upon the *under* ſide only), and is uſed as a ſubſtitute for a jar of 35 inches of coated ſurface, it being of no conſequence what the ſhape of the glaſs be, provided it have a proper quantity of coating. M E N is a cylinder of glaſs of four inches diameter, the body of which is 7½ inches long: at M and N are braſs or * boxen caps cemented as in other machines, upon the ſmall cylindrical ends of which the cylinder revolves by means of a ſimple winch C, which may be taken off or put on at pleaſure, either by ſcrewing, or by being inſerted upon a ſquare ſhoulder. The central points of revolution of N and M are at about 2½ inches from the top of the box, the firſt inſerted into a circular hole in one end of the box, and the other *let down* in an open place made down through the other, which has a detached piece of ſimilar wood to fit it, and to keep the cylinder in its place. L is the cuſhion, and E the ſilk placed in the uſual way; D is a ſcrew with a milled head, which, by the aſſiſtance of a tapped nut, placed faſt in the inſide of the box at 4 inches from the bottom, preſſes againſt the elaſtic part of the *ſupport* of the cuſhion which is hid from the eye: this ſupport, which may be of elaſtic wood, coated above D with tinfoil, or of any elaſtic metal, is ſcrewed faſt to the back of the box near the bottom in the inſide. The chain at D is hung on the ſcrew at pleaſure, as the wood is found to be in a good or bad conducting ſtate.

---

[*] As it is two years ſince I peruſed this treatiſe, and I have not ſeen it ſince, I cannot be certain of the exact words of the title, nor whether the name be Colman or Coleman. W. P.

F is a piece of light wood turned very ſmooth in a lathe, and neatly covered with tinfoil, its ends being rounded. This piece, which I ſhall call the collector, is furniſhed with about a dozen fixed pins of braſs, projecting againſt the ſide of the cylinder, as repreſented in the figure, as near as may be without touching it. The collector is 6 inches long, and ſomewhat more than an inch in diameter, and is ſupported by two ſolid glaſs pieces, C C, of an inch and three quarters in length, and nearly half an inch in diameter, cemented into the ſide of the box at 4½ inches from the bottom, and each at an inch from the end of the collector: by this means the collector becomes inſulated, and has a chain to be hooked on an eye of wire fixed on any part thereof, ſo as to fall occaſionally upon the upper inſulated coating of the plate of glaſs in the bottom of the box: the chain, however, muſt not be ſo long as to extend beyond the coating by any motion of the box.

B is a glaſs tube † of about one-eighth of an inch bore, and about 3 or 3½ inches long, with a ring of braſs or horn, that has a male ſcrew, cemented upon the middle of it, to ſcrew into another ring that has a female ſcrew, and is cemented or otherwiſe faſtened into the

---

[*] When caps are made of wood, they muſt be turned ſo as to admit the glaſs neck of the cylinder into a ſpace or cavity which it ſhall nearly fit. By this diſpoſition, the glaſs is cemented to the wood at both ſurfaces, and the cap itſelf will be ſtronger. W. P.

[†] Mr. Nicholſon may have been aware, that glaſs-tubes of a ſmall bore, however thick, are liable to be broken by a ſhock paſſing through them, when unguarded; and alſo that a lining of any kind will uſually prevent ſuch an effect. Mr. Fell of Ulverſton, in Lancaſhire, an excellent electrician, has tubes placed over the wires of a very large battery, lined with paper or tinfoil, which do not break with a diſcharge that melts pretty thick braſs wire. The tubes are uſed as a guard againſt any accidental touch when the battery is charged. W. P.

front ſide of the box at 4½ inches from the bottom. Through this tube (when coated with varniſh or ſubſtance of any kind within, to prevent its breaking by a diſcharge) paſſes a braſs wire as thick as will eaſily move in it, about 4½ inches long, tapped about an inch at each end and a little ſmoothed; on each end of this wire is a tapped nut which ſcrew back or forwards to or from the ends of the tube, ſo as to hold the wire in any ſituation that may be required with regard to the diſtance of its ends from thoſe of the tube. The wire has alſo two braſs balls, one of which has its diameter leſs than that of the tube, that it may move through the circular hole into the box, to prevent its falling on the glaſs plate, which it might do if ſcrewed off and on within the box.

O O are the directors with handles of glaſs, or baked wood varniſhed, each ſix inches long, beſides the balls and wires; the balls are faſt upon the wires, and the wires ſcrew into ſockets on the ends of the handles.

There is yet one part of the apparatus which it was not neceſſary to exhibit in the plate, viz. the inſulating ſtool; this is made exactly of a ſize to cover the box, namely 10½ inches by 8½, and has four feet of glaſs, or baked wood varniſhed, almoſt 6 inches long. Underneath that part of the ſtool which covers the collector ſlides a little drawer, fig. 3. by means of the end-pieces P P, which drawer contains the tube, with its appendages, milled nut, chains, handles of the directors, and amalgam, and muſt be taken from the ſtool when uſed. The three compartments of the drawer, by being made to fit the directors and tube, prevent their ſhaking in carriage. When packed, the four feet of the ſtool go exactly into each corner, and the edge of the drawer juſt within the ſide of the box, by which means the ſtool is kept in its place as a cover; and laſtly the wires of the directors paſs through the ſtool, which is of inch plank, at the middle near the two ends, and, by ſcrewing into the fixed nuts in the edges of the box's ends at K and K, until the balls touch the wood of the ſtool, fix the whole ſo firm that a handle, ſuch as is uſed in a cheſt of drawers, fixed on the centre of the ſtool, ſerves for carrying the whole apparatus by, which has the appearance of a lady's ſmall travelling-box, and weighs eight pounds. After having been thus prolix in the deſcription of the ſeparate parts of this ſmall machine, I come next to ſay a few words about the uſe of it.——As it is neceſſary before uſing any machine to have it perfectly dry, and as the ſeparate parts require to be ſucceſſively placed before the fire, this machine will be found to poſſeſs an advantage even in the preparation, which greatly conduces to expedition; for after the box is diſengaged from the ſtool, and has the tube ſcrewed into its place (both which may be done in one minute), the box may be held in the hand before the fire, where all its parts will be alike dried at the ſame time; or it may be placed on the end A or ſide D at any convenient diſtance from the fire on the floor, as occaſion may require, whilſt the amalgamated leather is preparing.

The machine being freed from moiſture and duſt, and amalgam applied to the cylinder, the ſhock is adminiſtered in the following manner, viz. Let the ſmall chain fall from the collector upon the upper coating of the glaſs plate, and place the inner ball at the required ſtriking diſtance, ſuppoſe a quarter of an inch, as the caſe may require, and fix it there by means of the adjuſting nuts, and the quantity of the charge will be limited in the ſame manner as by Lane's electrometer; then connect one chain with the conducting wire which paſſes through the tube, and the other with a braſs ring connected with the ſlip of the under coating at H (as appears in figure 1, or at I, as is moſt convenient), and hook the oppoſite ends upon the wires of the directors, whereby a ſhock may be ſent through any particular part of the body, as well as if the medical jar were uſed with Lane's electrometer. It is immaterial whether the machine, in this operation, be held on a table or ſtand, the operator's knee when ſitting, the bed of a patient, or even the ground by the ſide of a river in caſe of neceſſity where the patient is apparently drowned: nay, it may be held, by a perſon ſtanding, in one arm, whilſt the other hand turns the winch.

When the ſpark only is required, the ſmall chain is removed from the collector, and alſo the long chain, connected with the hook at H, is taken away; the inner ball is then fixed ſo as to touch the collector, by means of the adjuſting nuts; in which caſe the collector, the wire in the tube, and the ſecond long chain connected therewith, form together one conductor. After the exterior end of this chain is attached to the wire of one of the directors, the ſpark may be directed *into* any particular part of the body; when the eye is the part affected, a pointed wire muſt be uſed inſtead of the wire carrying the ball, which muſt be made to ſcrew into the ſocket of the director.

When a ſpark is taken *out of* the body, the patient muſt ſtand on the ſtool, and hold the chain, connected as before, in his hand; and then the operator, or aſſiſtant, may take ſparks from him, as in other machines.

If the patient be nervous, or afraid of the ſpark, it would be ſafer to hook the chain to the collector for fear of breaking the tube, and hold it at a diſtance from the edge of the box in a perpendicular direction. The ſparks taken from an inſulated body are ſtronger than thoſe ſent into any perſon by reaſon of the ſmallneſs of the conductor, which is perhaps the greateſt defect of the machine: this defect may, however, be remedied by placing any rounded metallic ſubſtance, ſuch as a large candleſtick, upon the ſtool near the machine, and connecting the chain with it, ſo as to make it a part of the conductor, and then the ſparks will become more denſe.

There is yet another way of adminiſtering the electric fluid, which cannot properly be called adminiſtering either the ſpark or the ſhock; in which caſe the ſenſation is not ſo momentary as is that cauſed by the ſhock, but more pungent than that of the ſimple ſpark; it may be called the interrupted ſhock, and is thus effected: Let the ſhort chain fall from the collector upon the coating of the plate, and connect one of the long ones with the hook at H or I, and ſuffer it to be extended on the table; then, the inner ball touching the collector, turn half a dozen rotations before the finger be preſented to the outer ball; and becauſe the circle is not completed between the two coated ſurfaces of the plate by *perfect* conductors, a continuous ſtream of denſe ſparks will iſſue into the finger, cauſing a more pungent ſenſation than would be felt from a ſimple ſpark of a much more powerful machine. I ſhould ſuppoſe the ſtimulant power of this mode of electrifying any particular limb to be very great, and I underſtand it has been practiſed by ſome electricians, by means of a charged jar where the circle is not completed by perfect conductors.

If a ſmall pocket-jar, in a caſe, be added to this machine, it will be capable of producing either a negative or poſitive charge in it; for if the jar held by the ball connected with its inner coating be preſented ſo as to receive a poſitive charge on its outer coating from the outer ball of the machine, the inner coating will of courſe be negatively charged; and if it be placed upon the ſtool, or inverted goblet, till the hand has taken hold of the outſide, the negative charge will be retained.

Thus this little machine is capable of affording much amuſement, and even of trying experiments with, beſides its being adapted for medical purpoſes; and, which is not the ſmalleſt recommendation, may be manufactured at a ſmall expence, compared with the larger machines. But, ſmall as it is, I have frequently fired ardent ſpirits with it, and have ſeen its power ſuch as to cauſe 35 inches of coating to diſcharge at a quarter of an inch ſtriking diſtance at every third revolution; and at fourteen or fifteen revolutions it will, when in good order, diſcharge at half an inch. A ſecond machine of this conſtruction, which I lately made for a friend, contains 42 inches of coated ſurface, with a cylinder of the ſame ſize as the one already deſcribed, and is better adapted for making experiments, the box being larger, and the inſulation more perfect; but is not ſo portable.

Before the former machine was completed, I apprehended that the proximity of the wood to the collector would take off much of the fluid; but it does not appear that this is the caſe at all in giving ſhocks, nor even in taking ſparks, unleſs the diſtance of the body to be electrified from the ball uſed be greater than the diſtance from the collector to the wood, or rather from the adjuſting ſcrews to the wood, for here the tendency to eſcape ſeems to be the greateſt. On this account the parts of the box where the fluid has the greateſt tendency to eſcape, are varniſhed with ſeveral coats of ſealing-wax melted in ſpirits of wine. How far baked wood would be preferable to mahogany in its working ſtate for the front and ends of the box, I have not attempted to aſcertain.

As this machine is calculated to keep out the humidity of the air, when the hole for the tube is corked, and all the glaſs, or inſulating parts, well varniſhed with proper electrical varniſh; the exciting power of the cylinder, when packed in good order, will remain but little impaired for ſeveral hours, and ſometimes days if kept in a dry room, without freſh amalgam. And what may appear rather remarkable, as the ſtool is near the cylinder when packed, if the chains and tube be firſt properly fixed, and the drawer removed, a ſhock may be adminiſtered with the ſtool ſcrewed on, as well as when off, and to all appearance in as powerful a manner: hence the ſtool may be uſed as a preſervative againſt humidity in the open air, in caſes of neceſſity.

After what has been ſaid of the conſtruction and uſes of this little machine, it may be aſked: "Has its efficacy ever been brought to the teſt?" I anſwer, Yes. A gentleman, who is a near relative of the writer of this article, was ſeized with a paralytic affection, during breakfaſt, on September 15, 1797, which totally deprived him of the uſe of any part of his left ſide, but which was not attended with pain. The beſt medical aſſiſtance was recurred to for nearly a fortnight, without producing any apparent alteration. The appetite and tendency to ſleep remained nearly as uſual; but the diſtortion of one ſide of the mouth rendered the ſpeech ſo inarticulate as to be nearly unintelligible. On the 27th of the ſame month I was permitted to adminiſter partial ſhocks through different parts of the affected ſide, which I began at the ſtriking diſtance of one-eighth of an inch with the machine already deſcribed, while the patient was extended on a ſofa; the firſt two or three ſhocks ſent through the hand, were ſcarcely if at all felt; by and by a ſenſation was perceived; a few larger ſhocks at one-ſixth were then adminiſtered, which convulſed the hand a little, and were ſenſibly felt; a great many more, at the ſame ſtriking diſtance, were then ſent through moſt of the joints of the diſeaſed ſide; laſtly, a few more, at the diſtance of one-fourth of an inch, were directed from the ſhoulder to the hand, and from the knee to the foot. After being thus electrified, the patient felt ſome pain in the knee, where the largeſt ſhocks entered, and upon trial could raiſe the leg a little from the ground; but the hand and arm were motionleſs. On the morning of the 28th, the patient, after a good ſleep, could raiſe the leg as before; and, to my great ſatisfaction, ſuch an alteration had taken place, that by a motion proceeding apparently from the ſhoulder, the arm was carried back and forward, as if ſuſpended by a pin; but the hand appeared no more than a dead weight. Partial ſhocks of one-fourth of an inch were adminiſtered as on the preceding evening, after which a pain ſimilar to what had been before felt in the knee was now felt in the ſhoulder; the knee became ſomewhat ſtronger, and the arm more active in its motion, but the hand remained apparently dead. In the evening of the 28th, the patient was in bed, and electrified as before, after which the thumb and firſt finger could be moved in a ſmall degree. This rapid advance on the firſt application of electricity, could not, I think, be attributed to the effects of medical aſſiſtance, which had been omitted to be uſed a few days before, except that a ſeton remained (which ſtill remains); nor yet to what is called the "vis medicatrix naturæ," for this uſually operates ſlowly. Shocks have continued to be uſed ſometimes from one extremity to the other, in general twice a day, ever ſince, to the preſent time, January 13, 1798, whilſt the ſtriking diſtance has been gradually enlarged to nearly 4-10ths of an inch, and the increaſe of ſtrength and activity has been hitherto regular. The patient at this time can walk into the ſtreet, or up and down ſtairs, without the aſſiſtance of a ſtick, and can handle any thing of ſmall weight in his weaker hand: in ſhort, he is happily become capable of ſuperintending the affairs of his family, and, I truſt, has reaſon to hope for ſtill greater ſtrength by perſevering in the means hitherto uſed. Other inſtances could be adduced in which this machine has been uſed with as good effect as a larger one in diſeaſes of the human body; but a particular detail would enlarge the bounds of this article, which perhaps will be deemed already ſufficiently long.

Lincoln, January 13, 1798.

V.

A Memoir concerning a remarkable Phenomenon in Meteorology. Read to the Society of Naturaliſts of Geneva. By M. De Saussure, *October* 1797 *.

As ſoon as I had rendered my hygrometer ſufficiently perfect to compare one inſtrument with another, and at the ſame time ſo delicate as to immediately ſhew the changes in the atmoſphere, I was in hope that I might avail myſelf of it to foretell the changes of the weather. I expected that the hygrometer would move towards *dry* on the approaching fine weather, or towards *moiſt* when wet weather was at hand; and it is true that, in general, the north-eaſt wind, which uſually with us accompanies fine weather, does cauſe it to move towards *dry*; and, on the other hand, that it indicates humidity during the ſeaſons of rain. But I have ſince obſerved a very remarkable exception; namely, that the times of greateſt dryneſs are generally the precurſors of rain.

---

* This paper was communicated to the Editor of La Decade Philoſophique, &c by Felix Deſportes, Reſident of the French Republic at Geneva. I have tranſlated it from this laſt work, No. 4. N.

' Ever ſince I made this obſervation, my thoughts have been frequently directed to account for it; and it was not till lately at Plombieres that I found a ſatisfactory cauſe, which I ſhall make the ſubject of the preſent Memoir.

In order to give every poſſible degree of certainty to my obſervations, I kept my inſtruments defended not only from the direct rays of the ſun, but from every reflection; and I obſerved their ſtation daily at the ſame hours, but more particularly at four in the afternoon, when the greateſt dryneſs uſually prevails.

During the two months that I remained at Plombieres, the greateſt dryneſs I obſerved was on the 2d of Auguſt. The hygrometer indicated 68,5°, the thermometer being at 22,5°. Three or four days before, the hygrometer at the ſame hour had ſtood higher or nearer to humid, that is to ſay, 86°, or 87°, though the thermometer was near a degree higher, namely 23,1°, which muſt have proportionally raiſed the hygrometer. It rained in the evening of the ſame day on which it had been drieſt, namely, on the 2d of Auguſt. At the ſame time that the hygrometer deſcended, the barometer alſo fell near two lines. Now I have proved, in my Eſſays on Hygrometry, and by ſeveral experiments, that in an air which is rarefied the hygrometer deſcends, and denotes a greater degree of dryneſs. I therefore attributed this extraordinary dryneſs to the rarefaction of the air.

On my return from Plombieres, I purſued my obſervations with the ſame care. On the 29th of Auguſt of this preſent year, at 20 minutes after 4 in the evening, I obſerved the hygrometer at 74°, while the thermometer ſtood at 22,5°. On the following day, at 50 minutes after one, I found the thermometer exactly at the ſame degree as the evening before, namely, 22,5°, the hygrometer being at 59,5°, that is to ſay, 14,5° higher than the evening before. I noted this moment, as affording a valuable obſervation, on account of the identity of height in the thermometer. It was therefore evident that the dryneſs of the air had been increaſed, not by its heat, but by ſome other agent, ſuch as its rarity. In fact, the barometer had deſcended more than half a line. A wind from the ſouth-weſt prevailed at the ſame time, and it rained early the next morning.

But the moſt ſtriking inſtance is to be found in my Eſſay on Hygrometry. I made this obſervation at Chamouni on the 23d of July 1781. The hygrometer ſtood at 41,2°, the thermometer indicating at the ſame time 20,2°. But this degree of heat was far from ſufficient to produce ſuch a degree of dryneſs; for, by calculating from the table at page 87 of my Eſſay on Hygrometry, we find that the difference of 4½ degrees of heat between that day and the preceding could not depreſs the hygrometer more than 9 degrees, inſtead of 20, which it really deſcended. The exceſs, namely 11 degrees, muſt therefore be attributed to another cauſe, which I conclude to be the rarefaction of the air. The air may be rarefied either by the fall of the barometer, or the direction of the wind. In fact, the ſouth and ſouth-weſt winds coming from lower countries than ours, and from the ſea, muſt neceſſarily riſe, and thence become rarefied, and conſequently, as I have ſaid, cauſe the hygrometer to advance towards *dry*. It is probable likewiſe that the ſingular elevation of the valley of Chamouni above the level of the ſea, was one of the principal cauſes of the dryneſs which predominated in that valley on the 26th of July 1781.

We may therefore conceive the reaſon of this phenomenon, which at firſt ſight appears ſo ſtrange; namely, that extraordinary dryneſs ſhould precede rain, and that the hygrometer ſhould become, in this manner, an aſſiſtant to the barometer, and afford one of the moſt certain indications of change of weather. It is to be obſerved, that it rained at Chamouni the next morning.

Nature has exhibited ſigns of dryneſs which do not deceive the inhabitants of the country, and ſerve to predict ſtorms long before hand; ſuch as the flaccidity or drooping of plants with large and thin leaves; ſuch as the gourd and the beet in our gardens, the petaſite in the fields, and the cacalia in the mountains. It is obſerved, that the leaves of theſe plants droop and incline towards the ground on the approach of ſtormy weather; and that on the contrary they ſpring up, and aſſume an appearance of vigour, when the dew or rain has reſtored the elaſticity and natural freſhneſs to their fibres. I muſt add, that the heavy rain in the month of September laſt was preceded by a wind of extraordinary dryneſs: and as we are deſirous of knowing the purpoſe, or at leaſt the uſe, of each of the laws of nature, I would remark, that theſe great droughts which precede ſtorms ſeem intended to put vegetables into a proper ſtate to obtain the greateſt advantage from the rains, on which their growth is to depend. A dry air relaxes and empties their veſſels, and gives them the power of abſorbing the rain-water which ſucceeds. This water finds the air through which it falls loaded with carbonic acid and other exhalations which give fertility to plants.

It is in fact obſerved, that the rain of ſtorms which ſucceed uncommonly dry weather gives to vegetables a peculiar growth and ſtrength, much greater than are found to ſucceed other rains or long continued wet weather.

Hence we perceive, that the more attention we pay to natural phenomena, the more reaſon we find to admire the order and uniformity which prevail in the laws to which they are ſubject.

VI.

On the various Denominations given to the Alkali of Tartar. By a Correſpondent.

To Mr. NICHOLSON, Editor of "The Journal of Natural Philoſophy, &c."

SIR,

AMONGST the numerous innovations which have taken place within theſe few years, nothing perhaps has undergone ſuch an infinite variety of modifications as the chemical nomenclature: ſo far indeed has the rage for novelty been carried in this reſpect, that every perſon who, without a demonſtrable neceſſity for the change, propoſes at preſent either a new ſyſtem, or an alteration in any one already received, muſt juſtly incur the cenſure of increaſing the confuſion which ſuch an endleſs mutation of names has already produced. I ſhall however beg leave to mention a circumſtance which has frequently appeared to me to be a remarkable one. We have no tolerable appellation univerſally received in our own language for a ſubſtance which is indiſputably in more ordinary uſe in the laboratory, and more frequently ſpoken of in chemical writings, than any other—the pure alkali of tartar. Let us only ſee what are the appellations which have been given to it.

Kali, the name applied to this ſalt by the London College of Phyſicians, is not only improper, as being already that of a genus of plants, but has in fact been long uſed as the diſtinctive appellation of the other fixed alkali, in conſequence of its extraction from thoſe vegetables.

Fixed vegetable alkaline ſalt, the deſignation of this ſubſtance in the Edinburgh Pharmacopœia, to ſay nothing of its being rather a deſcription than a name, is no leſs objectionable on account of its impropriety. This alkali does not ſeem to be any more a product of vegetation than ſoda.

Potaſſe, the barbarous corruption of our word *pot-aſh* by the French chemiſts, has with them at leaſt one advantage; it does not ſignify any thing elſe: but when re-tranſlated by us into *pot-aſh*, it certainly becomes in the higheſt degree objectionable, as tending to confound a ſimple ſubſtance in a ſtate of chemical purity, with a heterogeneous compound of very different qualities.

Spodium, the appellation propoſed by Dr. Hopſon, is alſo inappropriate. This ſalt is certainly not formed by the proceſs of incineration any more than the other fixed alkali is: indeed barilla, from which the greater part of the ſoda uſed in this country is obtained, is as immediately in the ſtate of a cinder as any ſubſtance in commerce.

Tartarin, or, better perhaps, *tartarine*, is a word, which, if not every thing we could wiſh, has ſeveral great advantages. 1ſt. It ſignifies nothing elſe. 2dly. It is perfectly appropriate. Tartar is a ſubſtance which has been long and univerſally known, and which contains this alkali only in combination with an acid which is deſtructible by fire; ſo that the pureſt ſpecimen of its carbonate, which is commonly met with in the ſhops, is obtained from it by mere combuſtion; on which account it has been long called by ſeveral compound names analogous to the one here ſpoken of, as *ſalt of tartar*, *alkali of tartar*, &c. 3dly. The name itſelf recalls to the mind one of the readieſt and moſt ordinary means of diſtinguiſhing the ſolution of this ſalt from that of ſoda by the teſt of the acid of tartar. And 4thly. It has already been uſed throughout the whole of a work of ſome importance, which is in the hands of every chemiſt;—a circumſtance perhaps eſſential to the eſtabliſhment of any new chemical appellation. As the Latin, on account of its ſaving the neceſſity of auxiliary particles, and for ſome other reaſons, ſeems better adapted to ſome of the purpoſes of a chemical nomenclature than the modern languages, the feminine *tartarina* may ſerve well enough in this caſe. The word propoſed in the French nomenclature will not by this means become abſolutely uſeleſs:—the claſſical chemiſt may ſtill label his common pot-aſh "*Potaſſa Ruſſica*," and its titles, if not of equal antiquity, will not perhaps be inferior in elegance to thoſe of its next-door-neighbour, the "*Cineres Perlati*."

But as it is rather my intention to point out the difficulty than to propoſe the remedy, I ſhall add no more on the ſubject. The wiſh of every one muſt be, that by the univerſal reception of ſome unobjectionable appellation, we may be enabled to ſpeak of this ſubſtance without impropriety, or the appearance of affectation.

I am, Sir, your obedient ſervant,

PHILONOMUS.

January 18, 1798.

VII.

An Account of ſome Experiments to determine the Force of fired Gunpowder. By BENJAMIN *Count of* RUMFORD, *F.R.S. M.R.I.A.*

[Concluded from page 468.]

AFTER tabulating the experiments, of which an abſtract was given in our laſt number, the Count proceeds to aſcertain the law, according to which the elaſticities increaſe in proportion to the quantities of powder uſed for the charge. For the ſake of brevity, as well as becauſe the ſubject appears to require ſtill further inveſtigation than he has beſtowed upon it, I ſhall refer the reader to the paper itſelf, inſtead of attempting to give an abridged account of this part, and ſhall proceed to relate the other experimental reſults contained in his paper.

After having ſhewn the extreme force of fired gunpowder, he adverts to an objection which may be made againſt his deductions. How does it happen that fire-arms and artillery of all kinds, which certainly are not calculated to withſtand ſo enormous a force, are not always burſt when they are uſed? Inſtead of anſwering this queſtion, by aſking how it happened that the extremely ſtrong barrel uſed in his experiment could be burſt by the force of gunpowder, if this force be not in fact much greater than it has ever been ſuppoſed to be, he proceeds to ſhew that the combuſtion of gunpowder, inſtead of being inſtantaneous, as Mr. Robins's theory ſuppoſes, is much leſs rapid than has hitherto been apprehended; an obſervation, which, if eſtabliſhed, is certainly ſufficient to anſwer the objection.

He remarks, that it is a well known fact, that on the diſcharge of fire-arms of all kinds, cannon and mortars as well as muſkets, there is always a conſiderable quantity of unconſumed grains of gunpowder blown out of them; and what is very remarkable, as it leads directly to a diſcovery of the cauſe of this effect, theſe unconſumed grains are not merely blown out of the muzzles of fire-arms, but come out alſo by their vents or touch-holes, where the fire enters to inflame the charge, as many perſons who have had the misfortune to ſtand with their faces near the touch-hole of a muſket, when it has been diſcharged, have found to their coſt.

It appears extremely improbable to our author, if not abſolutely impoſſible, that a grain of gunpowder actually in the chamber of the piece, and completely ſurrounded by flame, ſhould, by the action of that very flame, be blown out of it without being at the ſame time ſet on fire. And, if this be true, he conſiders it as a moſt deciſive proof not only that the combuſtion of gunpowder is leſs rapid than it has generally been thought to be, but that a grain of gunpowder actually on fire, and burning with the utmoſt violence over the whole of its ſurface, may be projected with ſuch a velocity into a cold atmoſphere, as to extinguiſh the fire, and ſuffer the remains of the grain to fall to the ground unchanged, and as inflammable as before.

This extraordinary fact was aſcertained beyond all poſſibility of doubt by the Count's experiments. Having procured from a powder-mill in the neighbourhood of the city of Munich a quantity of gunpowder, all of the ſame maſs, but formed into grains of very different ſizes, ſome as ſmall as the grains of the fineſt Battle powder, he placed a number of vertical ſcreens of very thin paper, one behind another, at the diſtance of 12 inches from each other; and loading a common muſket repeatedly with this powder, ſometimes without and ſometimes with a wad, he fired it againſt the foremoſt ſcreen, and obſerved the quantity and effects of the unconſumed grains of powder which impinged againſt it.

The ſcreens were ſo contrived, by means of double frames united by hinges, that the paper could be changed with very little trouble, and it was actually changed after every experiment.

The diſtance from the muzzle of the gun to the firſt ſcreen was not always the ſame; in ſome of the experiments it was only 8 feet, in others it was 10, and in ſome 12 feet.

The charge of powder was varied in a great number of different ways; but the moſt intereſting experiments were made with one ſingle large grain of powder, propelled by ſmaller and larger charges of very fine grained powder.

Theſe large grains never failed to reach the ſcreen; and though they ſometimes appeared to have been broken into ſeveral pieces by the force of the exploſion, yet they frequently reached the ſcreen entire; and ſometimes paſſed through all the ſcreens (five in number) without being broken.

When they were propelled by large charges, and conſequently with great velocity, they were ſeldom on fire when they arrived at the firſt ſcreen, which was evident not only from their not ſetting fire to the paper (which they ſometimes did), but alſo from their being found ſticking in a ſoft board, againſt which they ſtruck, after having paſſed through all the five ſcreens; or leaving viſible marks of their having been impinged againſt it, and being broken to pieces and diſperſed by the blow. Theſe pieces were often found lying on the ground; and from their forms and dimenſions, as well as from other appearances, it was often quite evident that the little globe of powder had been on fire, and that its diameter had been diminiſhed by the combuſtion before the fire was put out, on the globe being projected into the cold atmoſphere. The holes made in the ſcreen by the little globe in its paſſage through them, ſeemed alſo to indicate that its diameter had been diminiſhed.

That theſe globes or large grains of powder were always ſet on fire by the combuſtion of the charge, can hardly be doubted. This certainly happened in many of the experiments; for they arrived at the ſcreens on fire, and ſet fire to the paper: and in the experiments in which they were projected with ſmall velocities, they were often ſeen to paſs through the air on fire; and when this was the caſe, no veſtige was to be found.

They ſometimes paſſed on fire through ſeveral of the foremoſt ſcreens without ſetting them on fire, and ſet fire to one or more of the hindmoſt, and then went on and impinged againſt the board, which was placed at the diſtance of 12 inches behind the laſt ſcreen.

The Count then proceeds to mention another experiment, in which the progreſſive combuſtion of gunpowder was ſhewn in a manner ſtill more ſtriking and not leſs concluſive.

A ſmall piece of red-hot iron being dropped down into the chamber of a common horſe-piſtol, and the piſtol being elevated to an angle of about 45 degrees, upon dropping down into its barrel, one of the ſmall globes of powder (of the ſize of a pea), it took fire, and was projected into the atmoſphere by the elaſtic fluid generated in its own combuſtion, leaving a very beautiful train of light behind it, and diſappearing all at once like a falling ſtar.

This amuſing experiment was repeated very often, and with globes of different ſizes.

When very ſmall ones were uſed ſingly, they were commonly conſumed entirely before they came out of the barrel of the piſtol; but when ſeveral of them were uſed together, ſome, if not all of them, were commonly projected into the atmoſphere on fire.

As the ſlowneſs of the combuſtion of gunpowder is undoubtedly the cauſe which has prevented its enormous and almoſt incredible force from being diſcovered, our author deduces, as an evident conſequence, that the readieſt way to increaſe its effects, is to contrive matters ſo as to accelerate its inflammation and combuſtion. This may be done in various ways; but, in his opinion, the moſt ſimple and moſt effectual manner of doing it would be to ſet fire to the charge of powder, by ſhooting (through a ſmall opening) the flame of a ſmaller charge into the midſt of it.

He contrived an inſtrument on this principle for firing cannon three or four years ago, and it was found, on repeated trials, to be uſeful, convenient in practice, and not liable to accidents. It likewiſe ſuperſedes the neceſſity of uſing priming, of vent-tubes, port-fires, and matches; and on that account he imagined it might be of uſe in the Britiſh navy. Whether it has been found to be ſo or not he has not yet heard.

Another infallible method of increaſing very conſiderably the effect of gunpowder in fire-arms of all ſorts and dimenſions, would be to cauſe the bullet to fit the bore exactly, or without windage, in that part of the bore at leaſt where the bullet reſts on the charge; for, when the bullet does not completely cloſe the opening of the chamber, not only much of the elaſtic fluid, generated in the firſt moment of the combuſtion of the charge, eſcapes by the ſide of the bullet; but, what is of ſtill greater importance, a conſiderable part of the unconſumed powder is blown out of the chamber along with it in a ſtate of actual combuſtion, and, getting before the bullet, continues to burn on as it paſſes through the whole length of the bore; by which the motion of the bullet is much impeded.

The loſs of force which ariſes from this cauſe, is in ſome caſes almoſt incredible; and it is by no means difficult to contrive matters ſo as to render it very apparent, and alſo to prevent it.

If a common horſe-piſtol be fired with a looſe ball, and ſo ſmall a charge of powder that the ball ſhall not be able to penetrate a deal board ſo deep as to ſtick in it when fired againſt it from the diſtance of ſix feet; the ſame ball, diſcharged from the ſame piſtol with the ſame charge of powder, may be made to paſs quite through one deal board, and bury itſelf in a ſecond placed behind it, merely by preventing the loſs of force which ariſes from what is called windage, as he found more than once by actual experiment.

He has in his poſſeſſion a muſket, from which, with a common muſket charge of powder, he fires two bullets at once with the ſame velocity that a ſingle bullet is diſcharged from a muſket on the common conſtruction with the ſame quantity of powder. And, what renders the experiment ſtill more ſtriking, the diameter of the bore of his muſket is exactly the ſame as that of a common muſket, except only in that part of it where it joins the chamber, in which part it is juſt ſo much contracted, that the bullet, which is next to the powder, may ſtick faſt in it. He adds, that though the bullets are of the common ſize, and are conſequently conſiderably leſs in diameter than the bore, means are uſed which effectually prevent the loſs of force by windage; and to this laſt circumſtance, he concludes, it is doubtleſs owing, in a great meaſure, that the charge appears to exert ſo great a force in propelling the bullets.

That the conical form of the lower part of the bore where it unites with the chamber has a conſiderable ſhare in producing this extraordinary effect, is, however, very certain, as he has found by experiments made with a view merely to aſcertain that fact.

The remaining pages of the Count's paper are occupied by a computation, tending to ſhew, that the force of the elaſtic fluid, generated in the combuſtion of gunpowder, may be ſatisfactorily accounted for upon the ſuppoſition that its force depends ſolely on the elaſticity of watery vapour or ſteam. For this purpoſe he recurs to the experiments of Mr. Betancour, publiſhed in Paris, under the auſpices of the Royal Academy of Sciences, in the year 1790, which ſhew that the elaſticity of ſteam is doubled by every addition of temperature equal to 30° of Fahrenheit. From the Count's reference, it appears that the experiments were carried as far as 280 degrees of that ſcale, in which caſe the preſſure was found to be equal to about four atmoſpheres. He affirms, that there does not appear to be any reaſon why the ſame law ſhould not hold in higher temperatures, and has therefore extended his computations through thirteen more terms of the geometrical ſeries, the laſt of which affords an elaſtic force equal to more than ſixty-five thouſand atmoſpheres. As an excuſe for not giving this computation in detail, I muſt ſimply remark, that ſomething more than a negative reaſon ſeems neceſſary to juſtify the extenſion of this law of increaſe from ſo limited a ſcale of experiments. For which reaſon I ſhall add no more of this ſpeculative part, than merely that the water of cryſtallization in the nitre, and the moiſture which the charcoal may be conceived to retain, appear to be fully ſufficient to account for the exploſive force by means of ſteam only, if the deductions from Mr. Betancour's experiments be admitted.

VIII.

Obſervations on Strontian. By Citizen PELLETIER. *Read to the National Inſtitute 30th April*, 1796.[*]

STRONTIAN is at preſent conſidered by many foreign chemiſts as a peculiar earth. Its diſcovery ſeems to me to be due to Dr. Hope, profeſſor of chemiſtry at Glaſgow; he having firſt deſcribed its characters and chemical properties in a diſſertation which he publiſhed 4th November, 1793, and which has ſince been printed in the Tranſactions of the Royal Society of Edinburgh. This memoir is nevertheleſs poſterior to Dr. Crawford's diſſertation on the internal uſe of muriate of barytes, in which he announces that he thinks it probable that the ſtrontian mineral contains an earth different from barytes [†].

---

[*] Tranſlated from the Annales de Chimie, xxi. p. 113, by J. F—r.

[†] The ſalt obtained from the combination of ſtrontian earth with muriatic acid is much more ſoluble in hot water than in cold, and conſequently eaſily cryſtallizes by cooling: muriate of barytes, on the contrary, is nearly as ſoluble in cold water as in hot, and cryſtallizes by evaporation.

An ounce of diſtilled water at the temperature of 70 degrees, diſſolves 9 drachms and 50 grains of muriate of ſtrontian: the ſame quantity of water, at the ſame temperature, only diſſolves 3 drachms and 35 grains of muriate of barytes.

The former produces at leaſt 15 degrees of cold by its ſolution; the latter not more than 5 degrees at the moſt.

M. Klaproth has alſo examined ſtrontian, but, as it appears, ſubſequently to Dr. Hope; or at leaſt, as he does not ſpeak in his work of Dr. Hope's experiments, there is reaſon to believe that they were not known to him.

Strontian is alſo mentioned in M. Schmeiſſer's mineralogical work, as different from the other known earths.

It is in the ſtate of carbonate that it is found in Argyleſhire in the weſtern part of the north of Scotland, accompanying a vein of lead-ore.

Klaproth, Blumenbach, and Sulzer of Ronnebourg, called it *ſtrontianite*; Hope called it *ſtrontite*. I conceive that the name of *ſtrontian*, taken from the place where it is found, may be properly aſſigned to it, as this word in itſelf has no ſignification, and cannot conſequently render it liable to be confounded with other ſubſtances.

The carbonate of ſtrontian has been for a long time conſidered as a variety of native carbonate of barytes; I myſelf looked upon it as ſuch in 1791, from ſome aſſays to which I had ſubjected a ſmall ſpecimen, which Mr. Greville of London had the goodneſs to procure me. Having aſſayed it comparatively with the carbonate of barytes from Anglezark, which was then called witherite, I thought I did not at that time perceive any remarkable difference between the two ſubſtances. Both of them when fuſed by the blow-pipe afforded white opaque vitreous globules, which, when expoſed for ſome time to the air, became reduced to powder. Expoſed to diſtillation in a moderate heat, carbonate of ſtrontian does not, any more than carbonate of barytes, emit any carbonic acid gas, though they are both diſſolved by the nitric and muriatic acids with efferveſcence and a diſengagement of this gas. The ſalts which reſulted from theſe combinations were by no means deliqueſcent, and I therefore took thoſe of the ſtrontian for nitrate and muriate of barytes, and ſtill rather becauſe their ſolutions were decompoſed by the alkaline, calcareous, and other ſulphates, as is the caſe with the barytic ſalts.

It is ſome months ſince we have been acquainted in France with the labours of M. Klaproth on the carbonate of ſtrontian; but thoſe of Dr. Hope, although prior, were unknown to us, and it is only within a few days that I have been apprized of them by M. Schmeiſſer.

In a letter which M. Hermbſtœdt wrote me ſix months ago, he announced that M. Klaproth had eſtabliſhed the properties of ſtrontian as a new earth, the diſcovery of which had been made ſeveral years ſince by M. Sulzer, and had been publiſhed by M. Blumenbach in his treatiſe on natural hiſtory. The diſtinctive characters which M. Klaproth had found in the carbonate of ſtrontian were:

Firſt, That it was ſpecifically lighter than native carbonate of barytes (witherite);

Secondly,

Muriate of barytes affords by evaporation flattened octagonal cryſtals, two of whoſe oppoſite ſides are always much longer than the others: muriate of ſtrontian, by rapidly cooling, cryſtallizes in elongated filaments, and, when ſlowly cooled, in hexagonal columns, of which three ſides are alternately wider, and the others narrower, and terminated in obtuſe triangular pyramids. Thus, though the carbonate of ſtrontian much reſembles that of barytes, theſe two ſubſtances have very different qualities. It is probable that the ſtrontian mineral has for its baſis a new ſpecies of earth which has not hitherto been examined, and which it is of importance not to ſubſtitute for barytes for medical uſes. *Extract from Dr. Crawford's Diſſertation.* P.

The above is a tranſlation of Citizen Pelletier's note. Not having Dr. Crawford's tract on the *terra ponderoſa ſalita*, or muriate of barytes, at preſent by me, I have not been able to examine the accuracy of the quotation, but have no reaſon to doubt it, except that I think he uſes a different nomenclature. T.

Secondly, That it produced with the nitric, muriatic, and other acids, ſalts which were more ſoluble than thoſe of barytes;

Thirdly, that the ſalt which it formed with the muriatic acid, being diſſolved in alcohol, gave it the property of burning with a red flame;

Fourthly and laſtly, That it might be deprived of its carbonic acid by calcination; and that it became by this means ſoluble in water, and the moſt ſo in boiling water; ſo that by cooling a portion was ſeparated in a cryſtalline form.

The ſpecimen of carbonate of ſtrontian, which I had in my collection, was not conſiderable enough to admit of a great number of experiments; it was however ſufficiently ſo to enable me to repeat a part of thoſe which are related by Meſſrs. Hope and Klaproth; and what moſtly determined me to appropriate it to this purpoſe was, that Cit. Coquebert had publiſhed in No. 5 of the Journal des Mines, that from ſome aſſays which we had made of the carbonate of ſtrontian, I had doubted whether it contained a peculiar earth. The following are the reaſons on which my doubts were founded:

1. It is ſeveral years ſince I had ſucceeded in diſengaging carbonic acid gas from carbonate of barytes by calcination; and, having then diſſolved the calcined barytes in hot water, had obtained a cryſtallization: I could not therefore conſider this character as excluſively belonging to ſtrontian.

2. I knew alſo that calcareous muriate diſſolved in alcohol gave it the property of burning with a red flame. This conſideration induced me therefore to ſuſpect a mixture of calcareous carbonate in that of ſtrontian, and the ſpecimen which I had was in fact combined with this ſubſtance. I ſhall now deſcribe the experiments which I have made to endeavour to detect this ingredient; and, as they were made comparatively with a ſimilar ſet on the native carbonate of barytes (witherite), I have thought it proper to preſent them together, that it may be ſeen in what reſpects the carbonate of ſtrontian reſembles, and in what it differs from it.

Compariſon of Strontian and Barytes.

1. CARBONATE of barytes is found with ſulphate of barytes in a lead-mine at Anglezark, (near Chorley) in Lancaſhire*. Carbonate of ſtrontian is found at Strontian in Argyleſhire, alſo accompanying a lead-ore with ſulphate of barytes.

2. The carbonate of barytes from Anglezark, taken internally, is poiſonous, ſo that in that country it is known by the name of rat-ſtone [pierre contre les rats]. A little dog to which I had given fifteen grains was ſeized with vomiting, and died eight hours afterwards; and having given to another of apparently equal ſtrength fifteen grains of carbonate of barytes obtained from the decompoſition of the ſulphate, it was alſo ſeized with vomiting, and died fifteen hours afterwards: the latter was opened by Cit. Chauſſier. Another dog to which I had given a like quantity of carbonate of barytes, prepared from ſulphate of barytes from the ci-devant province of Auvergne, had vomitings, but did not die, though he took it two days ſucceſſively: he vomited each time. I purpoſe to repeat theſe experiments with carbonates of barytes procured from different barytic ſulphates, and eſpecially thoſe which are not accompanied by any metallic ores. The carbonate of ſtrontian, on the contrary, may be taken internally without danger. I gave 20 grains of it to a little dog, but he was

---

* See the Mancheſter Memoirs, iii. 598. T.

not ſeized with any vomiting, and twenty hours afterwards I did not at all perceive that he had felt the ſmalleſt inconvenience. It will be proper to repeat this experiment with ſtronger doſes. Blumenbach was alſo convinced that the carbonate of ſtrontian, taken internally, did not at all derange the animal œconomy. Theſe obſervations, therefore, point out a difference between ſtrontian and barytes.

3. The colour of the carbonate of barytes from Anglezark is a grey white: it is ſometimes found cryſtalliſed, but more commonly in a ſtriated maſs. Its ſpecific gravity is from 4.2919 to 4.3710. The colour of carbonate of ſtrontian is a light green, though it is ſometimes found colourleſs and tranſparent: it is ſtriated, and ſometimes of a regular cryſtalline form. Its ſpecific gravity is from 3.6583 to 3.6750 *. This carbonate is conſequently lighter than that of barytes.

4. Native carbonate of barytes, expoſed to a fire which is not too violent, ſcarcely loſes any thing of its weight: in a ſtronger one it attacks the crucible, and paſſes into fuſion. Carbonate of ſtrontian alſo retains the carbonic acid pretty ſtrongly; but with caution, and a proper continuance of heat, the carbonic acid may be ſeparated in the proportion of five or ſix hundredths of the ſalt, without its attacking the crucible: ſome care muſt, however, be taken not to have the fire too ſtrong, as the earth would, in that caſe, attack the crucible, and run into a chryſolite-coloured glaſs. The carbonic acid therefore is leſs ſtrongly retained in carbonate of ſtrontian than in that of barytes.

5. Meſſrs. Hope and Klaproth had obſerved, that calcined ſtrontian was ſoluble in water, and that, when boiling, it diſſolved a ſufficient quantity to afford cryſtals by cooling; ſo that theſe two chemiſts regarded this property in ſtrontian as a diſtinctive character. M. Klaproth eſpecially never ſucceeded in calcining the native carbonic of barytes ſufficiently to try its ſolubility in that ſtate. When he gave it but a ſmall degree of heat, it was not deprived of its carbonic acid; and when he applied a greater, it became vitrified. Dr. Hope announced, in a ſupplement to his memoir, that he had found means to calcine the native carbonate of barytes in a black-lead crucible, and that he found this earth, thus calcined, was, as well as ſtrontian, ſoluble in boiling water, and ſuſceptible of cryſtallization; which properties he has accordingly ceaſed to conſider, ſince that time, as characters peculiar to ſtrontian. I have alſo ſucceeded in ſeparating with facility the carbonic acid from both the native and artificial carbonates of barytes, as well as that of ſtrontian, without uſing black-lead crucibles; and I ſhall here deſcribe the method which has conſtantly proved ſucceſsful with me, and the comparative experiments which I have made on this ſubject.

Proceſs for ſeparating the Carbonic Acid from the Carbonates of Barytes and Strontian.

1. TO 100 grains of native carbonate of barytes in powder, I added 10 grains of powdered charcoal; and, the whole being well mixed, I made it into a ſtiff paſte with boiled ſtarch, and rolled it into a ball. Having put into a crucible a little freſh-burnt charcoal

---

* In the ſpecific gravities in the original, that of diſtilled water is ſuppoſed to be 10, the decimal point being put one place more forward than is here done. I have ventured, in conformity to the more uſual, and, as I think, more convenient, method of putting unity for this common meaſure, to point them as above. T.

in powder, and laid the ball on it, I covered it with powdered charcoal, and luted on a cover with common loam. Things being thus diſpoſed, the crucible was kept in a very ſtrong fire for a full hour, which time was ſufficient to diſengage the carbonic acid. The crucible being cold, I opened it, and found the little ball perfectly compact; but it now weighed only 70 grains. I then triturated it in a glaſs-mortar with about nine ounces of boiling water, filtrated the liquor, and, in order to diſpoſe it the more to cryſtallization, put it into a glaſs retort, to ſeparate a portion of the water by diſtillation. By cooling, there were formed in the retort cryſtals ſeveral lines in length.

2. One hundred grains of carbonate of barytes prepared from the ſulphate by the ordinary proceſſes were treated with 10 grains of powdered charcoal, as above deſcribed, and the crucible equally expoſed to heat for only an hour. Having then treated the reſidue with boiling water, and concentrated the liquor in a retort, I obtained, on its cooling, cryſtals ſimilar to thoſe before mentioned.

3. One hundred grains of carbonate of ſtrontian being treated in the ſame manner, the reſidue, after calcination, weighed only 72 grains. Its ſolution in hot water appeared to me more ſaturated than that of barytes; and, without having recourſe to concentration, I obtained cryſtals on its cooling, though I had uſed about the ſame quantity of water as in the preceding experiments. I do not, however, believe that ſtrontian is much more ſoluble than barytes; for in ſeveral other experiments I have had ſolutions of barytes ſo fully ſaturated as to cryſtallize by cooling confuſedly and in a maſs. Thus it is very evident that the action of fire ſeparates carbonic acid from barytes and ſtrontian, and that theſe earths then become ſoluble in water, and in larger quantity if boiling, ſo as to give cryſtals by cooling. This character does not therefore belong excluſively to ſtrontian, as M. Klaproth ſuppoſes.

I have alſo remarked that the aqueous ſolutions, both of barytes and ſtrontian, when pure or calcined, have an odour ſomewhat ſimilar to that of cauſtic pot-aſh or ſoda, or what is commonly called a lixivious odour.

Habitudes of the Carbonates of Barytes and Strontian.

With nitric acid. NATIVE carbonate of barytes was totally diſſolved by diluted nitric acid, and the ſolution attended with a diſengagement of carbonic acid gas, in the proportion of twenty-two parts in the hundred. The concentrated liquor afforded cryſtals, the moſt ordinary figure of which was octahedral.

One hundred grains of carbonate of ſtrontian were alſo diſſolved in nitric acid; but the diſengagement of carbonic acid gas was more conſiderable than in the preceding experiment, the proportion being thirty hundredths. The ſalt which reſults from this combination alſo cryſtallizes in octahedrons.

[*To be continued.*]

PHILOSOPHICAL NEWS AND ACCOUNTS OF BOOKS.

ON the firſt of Brumaire (21 Oct. 1797.) Citizen Garnerin made the experiment of the parachute at the Garden de Mouſſeaux [*]. This experiment has not been before attempted at Paris. Blanchard had the notion ſoon after the diſcovery of balloons; and at ſeveral different towns, particularly Liſle, he let fall from the veſſel of his balloon dogs and other animals. Some years ago he ventured to deſcend in perſon in an experiment he made at Bâle; but either from the bad conſtruction of his parachute, or by falling among trees, he had the misfortune to break one of his legs.

Citizen Garnerin was more fortunate, and has given the moſt ſatisfactory proofs of his ſkill, firmneſs, and intrepidity, which the impatient public had ſeemed to doubt at the Garden Biron. Notwithſtanding the haſte with which this philoſopher was obliged to make his preparations, and ſeveral accidents which happened to his apparatus, he aſcended from the Garden de Mouſſeaux at half paſt five in the evening. Between the balloon and the veſſel was placed the parachute, half opened, and forming a kind of tent over the aërial traveller. The wind, which through the courſe of the day had been violent, was now become calm, as if to favour his enterpriſe, and carried him to the northward, over the plain of Mouſſeaux. Full of that intereſt, that inſurmountable emotion which ſeizes us on beholding a man quit the earth and advance towards the clouds by an apparatus ſo majeſtic, the eyes of the ſpectators were fixed on the balloon, which roſe with rapidity. When it was at a conſiderable height, the parachute and veſſel were ſeen at once to ſeparate from the balloon; the latter of which burſt (celui-ci éclate), emptied itſelf, and floated down with the wind. The parachute unfolded itſelf, while the veſſel which ſerved as ballaſt drew it towards the earth. Its fall was at firſt ſlow and vertical; but ſoon afterwards it exhibited a kind of balancing or vibration, and a rotation gradually increaſing, which might be compared with that of a leaf falling from a tree. Cries expreſſive of the general terror and aſtoniſhment were heard on all ſides. The crowd ruſhed towards the place of his deſcent, where at length the aëronaut landed, and without injury. Emotions of joy and congratulation ſucceeded thoſe of alarm, and he was brought back in triumph to the Garden of Mouſſeaux.

Such was the experiment. The narrator then proceeds to give the following detail:—The parachute, which reſembles a vaſt umbrella, is of cloth, and its diameter, when unfolded, is 25 feet. Citizen Garnerin eſtimates the height from which he fell at 300 toiſes, but C. Say reckons it no more than one-third of that ſpace [†]. He had 75 pounds of ballaſt in his car at the moment of his fall, which he ſays he ſhould have thrown out, if he had not been apprehenſive of wounding the ſpectators below. C. Say thinks he would have done very wrong; becauſe the danger to which he was expoſed did not ariſe from the velocity of his fall, but the vibration of the car, which might have ſtruck him againſt the earth, or againſt walls, trees, or other prominences. Now, if he had thrown out his ballaſt, the remarker thinks he would have rendered the vibration much more rapid, extenſive, and dangerous, and perhaps even have cauſed his parachute to overſet, and the whole to fall in a maſs.

The author explains the theory of the parachute by obſerving that the reſiſtance of the air and gravitation are two forces which act at the ſame time on the whole ſyſtem of the machine; that gravitation acts as if the whole maſs were united at its centre of gravity, while the reſiſtance acts as if the whole of this maſs was united at another centre little diſtant from the centre of gravity of the cloth which forms the parachute, and very different from the former. The parachute may be conſidered as ſuſpended in the air by this centre of reſiſtance.

---

[*] This account was drawn up by J. B. Say, Editor of La Decade philoſophique, literaire, et politique, No. 4.

[†] In the way of rough illuſtration, this may be ſtated at once and a half the height of St. Paul's Cathedral in London. N.

If the centre of gravity be not vertically beneath the centre of reſiſtance when the parachute is properly placed, it is evident (ſays he) that it will incline to one ſide, deſcend obliquely, oſcillate, and the ſmalleſt irregularity in its figure will cauſe it to turn round its vertical axis. It is alſo important that theſe two centres ſhould be at a diſtance from each other, ſince the oſcillations will more readily take place the nearer they are. If they were coincident, there would be no cauſe why the whole apparatus ſhould not overſet. C. Say therefore propoſes that the car or part of the ballaſt ſhould be ſuſpended at a conſiderable diſtance below the parachute.

ACCOUNTS OF BOOKS.

Recherches expérimentales ſur le Principe de la Communication latérale du Mouvement dans les Fluides, appliqué à l'Explication de différens Phénomènes hydrauliques. Par le Citoyen J. B. Venturi, Profeſſeur de Phyſique expérimentale à Modène, Membre de la Societé Italienne, &c. &c. A Paris, chez Houel & Ducros, Rue du Bacq, No. 940—Théophile Barrois, Rue Haute-feuille, No. 22. Ann. VI. 1797.——Or, Experimental Reſearches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. Venturi, Profeſſor of Experimental Philoſophy at Modena, &c. &c.

I HAVE been favoured with a copy of this curious and intereſting work by the learned Profeſſor, which I have read with much pleaſure. The commiſſion nominated for that purpoſe by the National Inſtitute of France have given a very correct analyſis, which I ſhall chiefly follow in my account.

Citizen Venturi has introduced an horizontal current of water into a veſſel filled with the ſame fluid at reſt. This ſtream entering the veſſel with a certain velocity, paſſes through a portion of the fluid, and is then received in an inclined channel, the bottom of which gradually riſes, until it paſſes over the border or rim of the veſſel itſelf. The effect is found to be, not only that the ſtream itſelf paſſes out of the veſſel through the channel, but carries along with it the fluid contained in the veſſel; ſo that after a ſhort time no more of the fluid remains than was originally below the aperture at which the ſtream enters. This fact is adopted as a principle or primitive phenomenon by the author, under the denomination of the lateral communication of motion in fluids, and to this he refers many important hydraulic facts. He does not undertake to give an explanation of this principle, but ſhews, p. 37, that the mutual attraction of the particles of water is far from being a ſufficient cauſe to account for it.

The firſt phenomenon which the author propoſes to explain by this eſtabliſhed principle is the emiſſion of a fluid through different adjutages applied to the reſervoir which contains it. It is known that the vein of fluid which iſſues from an orifice or perforation through a thin plate, becomes contracted, ſo as to exhibit a ſection equal to about 0,64 of the orifice itſelf, ſuppoſed to be circular; and that the place of the greateſt contraction is uſually at the diftance of one ſemi-diameter of the orifice itſelf. If a ſmall adjutage be adapted to the orifice, having its internal cavity of the ſame conoidal form as the fluid itſelf affects in that interval, the expenditure is the ſame as by the ſimple orifice. But if at the extremity of this adjutage a cylindric tube be affixed, of a greater diameter than that of the contracted vein, or a divergent conical tube, the expence of fluid increaſes, and may exceed the double of that which paſſes through the aperture in the thin plate, though the adjutage poſſeſs an horizontal or even aſcending direction.

By the interpoſition of a ſmall adjutage, adapted to the form of the contracted vein, Citizen Venturi aſcertained, in the firſt place, that there is an increaſe of velocity in the tubes he employed, though the velocity of emiſſion itſelf be leſs than that of the ſtream which iſſues from an hole in a thin plate. He afterwards proves, by the fact, that the interior velocity and expenditure of fluid, which is increaſed through tubes, even in the horizontal or aſcending direction, is owing to the preſſure of the atmoſphere. If the ſmalleſt hole be made in the ſide of the tube near the place of contraction of the vein, the increaſed expenditure does not take place; and when a vertical tube is inſerted in ſuch a hole, the lower end of which tube is immerſed in water or mercury, it is found that aſpiration takes place, and the water or mercury riſes; and this aſpiration in conical tubes is leſs in proportion, as the place of inſertion of the upright tube is more remote from the ſection where the greateſt contraction would have taken place. And, laſtly, the difference between the expenditure of fluid, through an orifice made in a thin plate, and that which is obſerved through an additional tube, does not take place in vacuo.

The influence of the weight of the atmoſphere on the horizontal or aſcending flux being thus eſtabliſhed, the author conſiders it as a ſecondary cauſe, referable to, and explicable by, his principle of the lateral communication of motion in fluids. In conical divergent tubes, for example, the effect of this lateral communication is, that the central cylindrical jet, having for its baſis the ſection of the contracted vein, carries with it the lateral fluid which would have remained ſtagnant in the enlarged part of the cone. Hence a vacuum tends to be produced in this enlarged part which ſurrounds the central cylindric ſtream; the preſſure of the atmoſphere becomes active to ſupply the void, and is exerted on the ſurface of the reſervoir, ſo as to increaſe the velocity of the fluid at the interior extremity of the tube.

The author proves, that the velocity or total expenditure of fluid through an aperture of given dimenſions, may be increaſed by a proper adjutage in the proportion of 24 to 10: he applies this reſult to the conſtruction of the funnels of chimneys. He determines the loſs of emitted fluid, which may be ſuſtained by ſinuoſity in pipes. He ſhews by experiment, that a pipe which is enlarged in any part, affords a much leſs quantity of fluid than if it were throughout of a diameter equal to that of its ſmalleſt ſection. This, as he remarks, is a circumſtance to which ſufficient attention has not been paid in the conſtruction of hydraulic machines. It is not enough to avoid elbows and contractions; for it ſometimes happens, that by an intermediate enlargement the whole of the advantage ariſing from other judicious diſpoſitions of the parts of the machine is loſt.

There are two cauſes of the increaſe of expenditure through deſcending pipes. The firſt is owing to the lateral communication of motion which takes place in deſcending pipes, in the ſame manner as in thoſe which poſſeſs an horizontal ſituation; the ſecond ariſes from the acceleration by gravity which takes place in the fluid while it falls through the deſcending

ing tube. This ſecond kind of augmentation was known to the ancients, though they poſſeſſed no good theory nor deciſive experiments reſpecting it. The author endeavours to eſtabliſh a theory on the principle of virtual aſcenſion combined with the preſſure of the atmoſphere. His deductions are confirmed by experiment, in which he has ſucceeded ſo far as to ſeparate the two cauſes of augmentation, and aſſigned to each their reſpective degree of influence.

Citizen Venturi then proceeds to different objects of enquiry, to which his principle ſeemed applicable. He gives the theory of the water-blowing * machine, and he determines by calculation the quantity of air which one of theſe machines can afford in a given time. He obſerves, that the natural falls of water in the mountains always produce a local wind; and he even thinks, that the falling ſtreams in the internal parts of mountains are in ſome inſtances the cauſe of the winds which iſſue from caves. He proves, by the facts, that it is poſſible in certain inſtances to carry off, without any machinery, the waters from a ſpot of ground, though it may be ſituated on a lower level than that of the channel which is to receive the water.

The whirlpools, or circular eddies of water ſo frequent in rivers, are, according to the theory of our author, the effect of motion communicated from the parts of the current which are moſt rapid, to thoſe lateral parts which are leaſt ſo. In the application of this principle, he points out the circumſtances adapted to produce ſuch eddies at the ſurface or at the bottom of rivers. He concludes, that every movement of this kind deſtroys a part of the force of the current, and that in a channel through which water conſtantly flows, the height of this fluid will be greater than it would have been if the dimenſions of the channel had been uniformly reduced to the meaſure of its ſmalleſt ſection.

There is another kind of whirling motion ſomewhat different in its nature from theſe laſt. It is produced in the water of a reſervoir, when it is ſuffered to flow through an horizontal orifice. The author deduces the theory of theſe vortices from the doctrine of central forces. The form of the hollow funnel, which in this caſe opens through the fluid of the reſervoir, is a curve of the 64th ſpecies of the lines of the third order, enumerated by Newton. Theory and experiment both unite here in proving, that it is not only poſſible, but that there really exiſts in nature a vortex, the concavity of which is convex towards the axis, and of which the revolutions of its different parts follow the ratio of the ſquare of the diſtance from the centre. Daniel Bernoulli was in the wrong, in his Hydrodynamics, to reproach Newton for having ſuppoſed a vortex to be moved according to this law.

In the laſt place, the author conſiders that lateral communication of motion which takes place in the air as well as in the water. This is the cauſe of ſuch local and partial

---

* *Soufflet d'eau.* It is alſo called *trompe*, but we have no appropriate name for this engine in Engliſh. The reader may conſult Lewis's Philoſophical Commerce of Arts, and Chaptal's Elements of Chemiſtry, for deſcriptions. It conſiſts of an upright pipe, through which a ſhower of water is made to fall. This ſhower carries down a maſs of air with it, which is received beneath a kind of tub, and conducted to the furnace by means of a pipe. The moſt remarkable natural phenomenon of this kind is the ſquall at ſea. When a cloud ſuddenly falls in the form of rain, it drives down a portion of the atmoſphere, which glides rapidly along the ſurface of the ſea, and is capable of ſuddenly overſetting veſſels, or carrying away their maſts. It may eaſily be imagined, that ſeamen are very attentive to look out for this phenomenon, and to guard againſt it in time by lowering their ſails, &c. N.

winds as ſometimes blow contrary to the direction of the general wind. It is by virtue of the ſame principle, that the reſonant vibration, excited laterally in the extremity of an organ-pipe, is communicated to the whole column of air contained in the pipe itſelf.

From the ſame principle, the author deduces the augmentation of force which ſound receives in conical divergent tubes, compared with thoſe of a cylindrical form. On this occaſion, he points out the remarkable differences which appear to take place between the reſonant vibrations of air contained in a tube, and the ſonorous pulſations propagated through the open atmoſphere.

In an Appendix, Citizen Venturi relates different experiments which he has made to determine the convergence and velocity of the fluid filaments which preſs forward to iſſue out of a reſervoir by an orifice through a thin plate. He proves, by a very clear experiment, that the contraction of the vein is made at a greater diſtance from the orifice under ſtrong than under weak preſſures. He explains, why in a right-lined orifice, the ſides of the contracted vein correſpond with the angles of the orifice and the angles with the ſides. He examines the expenditure through a tube, the extremity of which is thruſt into the reſervoir itſelf, according to the method of Borda in the Memoirs of the Academy of Sciences for the year 1766.

The Commiſſaries of the Inſtitute appointed to examine this work of Citizen Venturi, without undertaking to decide reſpecting all the applications of his principle, give him full credit for the acuteneſs and ſagacity he has diſplayed in this curious experimental courſe. The author himſelf is indeed ſufficiently candid to admit that every thing of this nature, which is not confirmed by direct experiment, ought to form a ſubject of diſcuſſion and enquiry. “I have not,” ſays he, page 9, “inſiſted upon theoretical conſiderations, excepting ſo “far as they combine with the facts, and ſo far as it was neceſſary to unite thoſe facts in a “ſingle point of view. The reader may, if he pleaſes, even omit the ſmall portion of “theory, and conſider my propoſitions ſimply as the reſult of experiment.”

There is no doubt but that the whole work will tend to confirm the reputation of its author as a ſkilful experimentaliſt, and enlightened obſerver.

Conſiderations on the Doctrine of Phlogiſton and the Decompoſition of Water. By Joſeph Prieſtley, LL. D. F. R. S. &c. Philadelphia printed, 1796.

Obſervations on the Doctrine of Phlogiſton and the Compoſition of Water. Part II. By Joſeph Prieſtley, LL. D. F. R. S. Philadelphia printed, 1797.

Two Lectures on Combuſtion. Supplementary to a Courſe of Lectures on Chemiſtry, read at Naſſau Hall, containing an Examination of Dr. Prieſtley's Conſiderations on the Doctrine of Phlogiſton and the Decompoſition of Water. By John Maclean, Profeſſor of Mathematics and Natural Philoſophy in the College of New Jerſey. Philadelphia printed, 1797.

As I hope to give a fuller account in future of Dr. Prieſtley's obſervations in favour of the old chemical ſyſtem, I ſhall for the preſent content myſelf with announcing the titles of the three laſt works.

The Medical Repoſitory, Vol. I. Nos. I. and II. octavo. The two numbers contain 287 pages, cloſely printed. New-York, printed for T. and J. Swords.

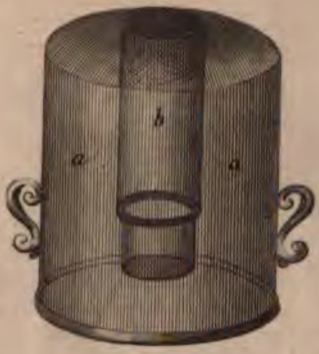
The authors and editors of this work are Doctors Samuel L. Mitchill, Member of the Legiſlative Aſſembly of the State of New-York, F.R.S. Edin. Profeſſor of Chemiſtry in Columbia College, and Doctors Edward Miller, and E. H. Smith, of New-York. The firſt Number appeared in July 1797, and the ſecond in the month of November laſt. It is intended to be publiſhed in quarterly numbers of at leaſt 100 octavo pages each, at the price of half a dollar per number. It conſiſts of medical eſſays or communications, a review of new publications, not only ſuch as are ſtrictly medical, but alſo thoſe which relate to agriculture and other branches of natural hiſtory, philoſophy, &c. or may be in any manner related to the objects contemplated in the plan of a medical repoſitory; and laſtly, medical facts, hints, enquiries, and news.

It is a well known fact, that the public will not aſſiſt in any periodical work until they have received proof, for a certain length of time, of the accuracy, ſpirit, and ability, with which the conductors are able from their own ſources, whether original or derivative, to ſupport it. A work of the kind before us is peculiarly calculated for the diffuſion of knowledge in a country like America, where, from various circumſtances, the written ſources of intellectual acquiſition are much more limited than in Europe. It is equally calculated to accelerate improvement in the whole of that conſiderable part of the globe, wherein the language of England is current. The preſent numbers ſhew no want either of ability or induſtry on the part of the authors, and will, it is to be hoped, meet the ſucceſs they merit.

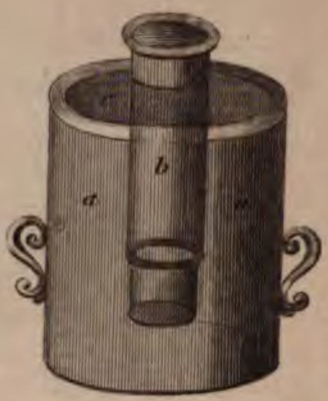
MY young correſpondent from Liverpool has ſhewn conſiderable ability in his meditations and conjectures on the many important chemical facts he mentions. When facts can be ſo diſpoſed as by mutual illuſtration to prove an entire theory, the individual who makes the arrangement, may, perhaps, do more for the advancement of ſcience, than the performer of many ſolitary experiments: but when this cannot be done, the chief uſe of imperfect concluſions or conjectures muſt be to point out new and deciſive experiments. W. S. may ſupport the hope of making ſuch experiments, by reflecting that the greateſt diſcoveries have been made by men who did not poſſeſs the means of acquiring much apparatus. Apothecaries' phials, Florence flaſks, baſons, cups, ſaucers, a blow-pipe and charcoal, common tobacco-pipes and garden-pots, as ſubſtitutes for crucibles and a furnace, and the ordinary bellows, together with a few chemical materials, would conſtitute no mean apparatus for philoſophical experiment. When he ſhall turn his thoughts towards the experimental reſearches, to which his obſervations evidently lead, I am confident he will be of opinion, that the publication of his conjectures, in their preſent ſtate, would not be deſirable.

Mr. Walker's freezing Processes

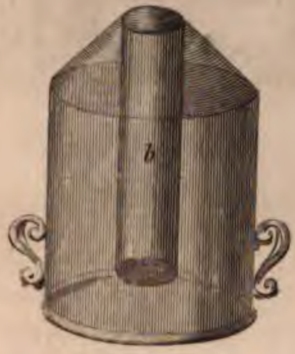
Fig. 1.



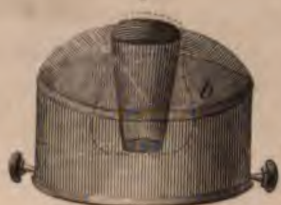
2



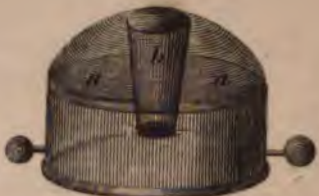
3



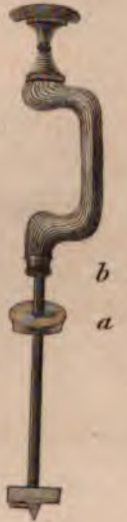
4



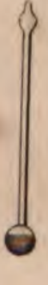
5



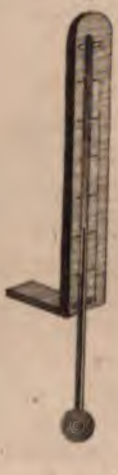
6



7



8



Barlow sculp.

Mr. Pearson's portable Electrical Machine.

Fig. 1.

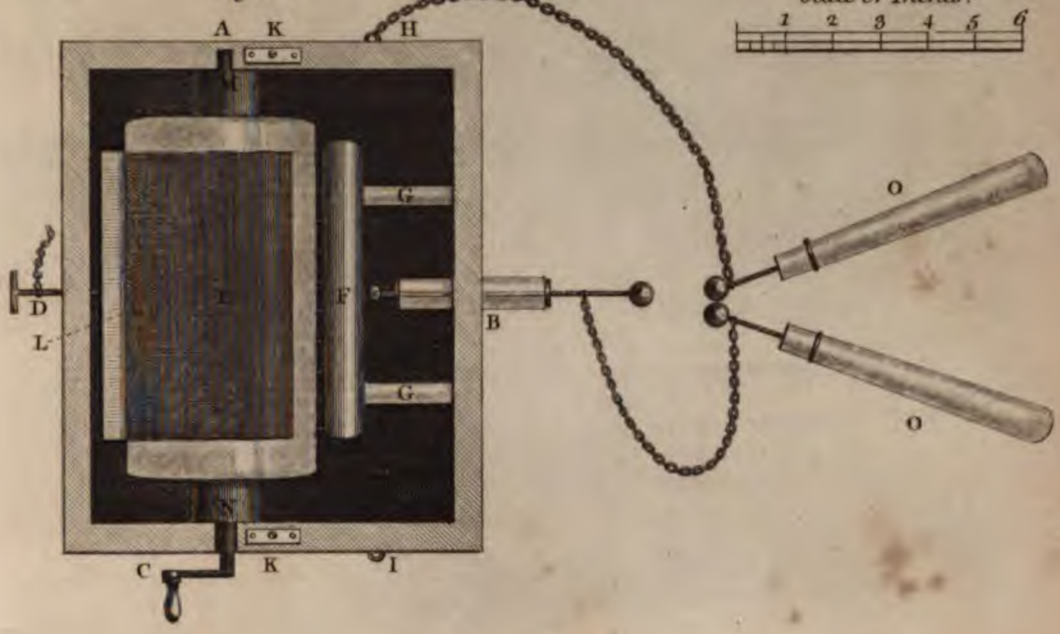


Fig. 2.

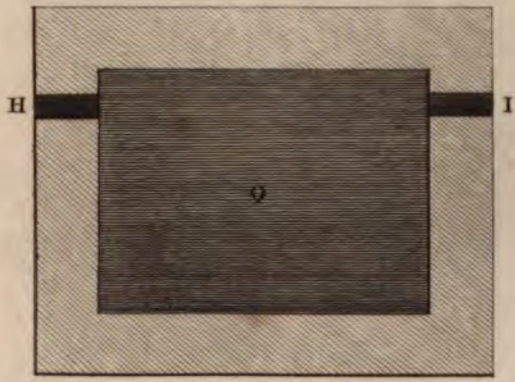
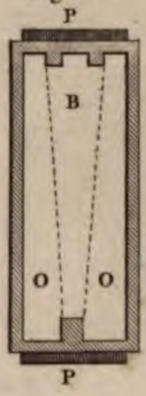


Fig. 3.



Barlow sculp.

THE NEW YORK
PUBLIC LIBRARY

ASTOR, LENOX AND
TILDEN FOUNDATIONS
R L

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MARCH 1798.

ARTICLE I.

Obſervations on Strontian. By Citizen PELLETIER. *Read to the National Inſtitute 30th April, 1796.*

[Concluded from p. 222.]

Habitudes of the Carbonates of Barytes and Strontian.

With muriatic acid. I DISSOLVED 100 grains of native carbonate of barytes in muriatic acid, and obtained 22 grains of carbonic acid gas. The ſolution being evaporated afforded cryſtals in the form of ſhort flattened priſms, or hexagonal plates, the weight of which was 138 grains. I alſo diſſolved in the ſame acid 100 grains of carbonate of barytes, obtained by the decompoſition of barytic ſulphate: the diſengagement of carbonic acid gas alſo amounted to 22 grains, and the muriate cryſtallized like the preceding, ſo that there was not any difference in their appearance *.

---

* The ſucceſs which Dr. Crawford met with in the internal uſe of muriate of barytes, in the treatment of ſcrophulous diſeaſes, begins to be known in France; and ſeveral phyſicians already preſcribe this new medicine. As, however, it is very active, and ſometimes even dangerous, it cannot be too ſtrongly recommended that it be given at firſt only in a very ſmall doſe, and that the effects which it may produce during its adminiſtration be accurately obſerved. The Society of Health is at preſent engaged in a general work, in which it is intended to determine what advantages medicine may derive from the uſe of muriate of barytes. They have alſo appointed commiſſaries to obſerve its effects in the treatment of horſes; and Cit. Huſard and Biron, who are intruſted with theſe latter experiments, have already given both the muriate and carbonate of barytes to horſes ſeized with the farcy [farcin]. Both theſe medicines given to different horſes in the quantity of two drachms a day ſeemed for a while to have effected a perfect cure: they were even conſidered as cured at the end of a fortnight;

It is not the ſame with the carbonate of ſtrontian: this was totally diſſolved by muriatic acid, but the diſengagement of carbonic acid gas amounted to thirty hundredths, and the ſalt obtained from this combination was in long needles or very ſlender rhomboidal cryſtals, terminated by a pyramid with two faces: the priſms are ſometimes hexahedral. This ſalt is alſo more ſoluble than muriate of barytes. I obtained 476 grains of the muriate from 100 grains of the carbonate. Dr. Crawford was one of the firſt who obſerved the great difference which there was in the form of the cryſtals of the muriates of barytes and ſtrontian, as well as their different degrees of ſolubility in water, and thence ſuſpected that theſe two ſubſtances could not be of the ſame nature.

With ſulphuric acid. Native carbonate of barytes reduced to powder is decompoſed by ſulphuric acid, with diſengagement of carbonic acid gas. The reſult of this combination is ſulphate of barytes, inſoluble in water.

Carbonate of ſtrontian is alſo decompoſed by ſulphuric acid, with diſengagement of carbonic acid gas; and the compound which is obtained is alſo but little ſoluble in water. Dr. Hope has obſerved that four ounces of diſtilled water only diſſolved half a grain of it, and that if muriate of barytes be added to this ſolution, a precipitation of ſulphate of barytes takes place: ſulphate of ſtrontian is therefore more ſoluble than that of barytes.

With acetous acid. The acetous acid diſengages the carbonic acid from carbonate of barytes.

The ſame acid alſo decompoſes carbonate of ſtrontian. The ſalts which reſult from theſe combinations, viz. the acetites of barytes and ſtrontian, are obtained in a cryſtalline form, and are not deliqueſcent.

Red Flame of Alcohol, holding in ſolution Nitrate or Muriate of Strontian or Lime.

AMONGST the characters by which Meſſrs. Hope, Klaproth, and others have diſtinguiſhed ſtrontian from barytes, there is one which they agree in conſidering as peculiarly diſtinctive. Chemiſts have obſerved that alcohol, in which either nitrate or muriate of barytes is diſſolved, burns with a flame of a white yellow, whilſt alcohol holding in ſolution nitrate or muriate of ſtrontian burns with a flame of a carmine red. Dr. Hope relates in his memoir that Dr. Aſh had ſo long ſince as 1787 obſerved the particular colour which muriate of ſtrontian gives to the flame of alcohol. The experiments repeated before the pupils of the polytechnic ſchool preſented the ſame reſults; but as the nitrate and muriate of lime alſo communicate to alcohol the property of burning with a red flame, I thought it right to aſcertain whether the nitrate and muriate of ſtrontian did not contain lime. For this purpoſe I tried the following experiments:

---

when the one to which the muriate of barytes had been given died without any apparent circumſtance to indicate his death: the ſecond, that which had taken the artificial carbonate of barytes, alſo died ſuddenly ſome days afterwards. The two horſes having been opened, all the viſcera were found in a ſound ſtate, and exhibited no indication, either of the effect of the barytic preparations, or of the repercuſſion of the diſeaſe. The experiments are continued on other horſes.

Since this article has been compiled, I have been informed, that a third horſe which was under a courſe of carbonate of barytes, alſo died ſuddenly. It ſeems, therefore, more and more evident, that the carbonates of barytes, whether native or artificial, may prove mortal when taken internally. P.

Experiments to aſcertain whether Strontian contains Lime.

1. TO a ſolution of nitrate of barytes I added fluoric acid, perfectly pure and free from ſulphuric acid: the mixture took place without precipitation.

2. To a ſolution of nitrate of ſtrontian I added the ſame fluoric acid: the mixture alſo took place without precipitation.

3. To ſolutions of the nitrates of barytes and ſtrontian put into ſeparate veſſels I added two or three drops of nitrate of lime, and afterwards ſome fluoric acid: this acid then very ſoon produced a white precipitate, which was fluate of lime.

It follows, from theſe comparative experiments, that the nitrate of ſtrontian did not contain lime; for, if it had contained ever ſo little, the fluoric acid would have occaſioned a precipitation. This acid has ſo ſtrong an affinity with lime that it takes it from the ſulphuric acid, when ſulphate of lime is holden in ſolution in water; ſo that when a few drops of fluoric acid are added to a ſelenitic water, a precipitation takes place as readily as when the oxalic acid is uſed. The fluoric acid preſents therefore an excellent means of detecting the preſence of lime. Very pure fluate of ammoniac may alſo be uſed; for, if this be mixed with the nitrate, muriate, or ſulphate of lime, a decompoſition takes place by double affinity, and the precipitate is fluate of lime.

Nitrate of Strontian is not precipitated, as Nitrate of Barytes is, by the Pruſſiates of Pot-aſh or Lime.

WE have ſeen, that the characters of ſtrontian in which it appears moſt ſimilar to barytes, exhibit, nevertheleſs, differences ſufficiently well marked, when they are ſubjected to a rigorous examination. The following experiment, which is owing to the obſervations of Dr. Hope, preſents a more ſtriking diſtinction between the two earths. It was known that nitrate of barytes is totally precipitated by the pruſſiates of pot-aſh and lime. Dr. Hope having added pruſſiate of pot-aſh to a ſolution of nitrate of ſtrontian, perceived only a ſlight precipitation, occaſioned by the iron which accompanies the carbonate of ſtrontian. I had a mind to aſcertain this myſelf, and therefore prepared three ſolutions, viz. 1. A ſolution of native carbonate of barytes in nitric acid: 2. A ſolution in the ſame acid of carbonate of barytes, prepared by the decompoſition of barytic ſulphate: 3. A ſolution of carbonate of ſtrontian in the ſame acid. Theſe three ſolutions being placed in ſeparate veſſels, I added a ſufficient quantity of pruſſiate of pot-aſh, to decompoſe them entirely. There was in effect an abundant precipitate in the nitrates of barytes, and the ſupernatant liquors being tried with carbonate of pot-aſh gave no ſign of further precipitation. The nitrate of ſtrontian, on the contrary, gave only a weak blue precipitation, in conſequence of the iron which it contained; and an exceſs of pruſſiate of pot-aſh precipitated nothing more: but the ſupernatant liquor, tried with carbonate of pot-aſh, afforded a very abundant white precipitate, which was carbonate of ſtrontian.

Thus it appears manifeſt, that pruſſiate of pot-aſh does not at all decompoſe the nitrate of ſtrontian, whereas it totally decompoſes the nitrate of barytes. This method points out, therefore, a diſtinctive character between the two earths. I ſhall not here examine the nature of the precipitate which is obtained from the nitrate of barytes when decompoſed by pruſſiate of pot-aſh, nor whether the decompoſition takes place in conſequence of the formation of a triſule, or of a mixture of ſulphuric acid, which the pruſſiate of pot-aſh may ſometimes contain. The comparative experiments which I have ſtated having been made with the ſame pruſſiate of pot-aſh, the nitrate of ſtrontian ought to have been decompoſed, as well as that of barytes, if theſe earths had been of the ſame nature.

Conſtituent Parts of the Native Carbonates of Strontian and Barytes.

TO detail at large the different experiments which I have made to determine the conſtituent parts of the native carbonates of ſtrontian and barytes, would be too prolix: I ſhall therefore only ſay, that it was by ſolution in muriatic acid that I determined the quantity of carbonic acid gas, and by calcination that of the earth. The mean reſult which I obtained was,

In 100 grains of native carbonate of barytes, or witherite,

Barytes, - - -	62
Carbonic acid, - -	22
Water, - - -	16
	100

In 100 grains of carbonate of ſtrontian,

Strontian, - - -	62
Carbonic acid, - -	30
Water, - - -	8
	100

Concluſion.

I HAVE not been able to carry my reſearches further reſpecting ſtrontian, not having any more to ſubmit to new experiments: as ſoon as I can procure ſome, I ſhall continue them, as I perceive that it will be advantageous to eſtabliſh the difference between it and barytes by more numerous and more ſtriking characters. The reſults of my examination, and that of Meſſrs. Hope and Klaproth, are:

1ſt, That the carbonate of ſtrontian is neither poiſonous nor emetic, as both the native and artificial carbonates of barytes are;

2dly, That the carbonate of ſtrontian is ſpecifically lighter than that of barytes;

3dly, That it more eaſily parts with it's carbonic acid, and contains alſo a larger proportion of it than carbonate of barytes does;

4thly, That calcined ſtrontian is ſoluble in cold and hot water, but more abundantly in the latter, ſo as to afford cryſtals by cooling;—a property, indeed, which it poſſeſſes in common with barytes;

5thly, That the muriate and other ſalts of ſtrontian are more ſoluble than the homologous barytic ſalts; and the nitrate and muriate communicate to alcohol the property of burning with a red flame, whereas the ſame barytic ſalts tinge the flame of their alcoholic ſolution with a yellowiſh blue;

6thly, That ſtrontian does not contain lime;

7thly and laſtly, That the nitrate of ſtrontian is not decompoſed by pruſſiate of pot-aſh, which decompoſes the nitrate of barytes.

All theſe characters already eſtabliſh a ſufficiently diſtinguiſhable difference between ſtrontian and barytes, and a ſtill greater between it and all other known earths, ſo that it may be conſidered as a peculiar earth.

CONTINUATION.

IN the memoir which I read to the Inſtitute the 30th of April laſt, on the earth known by the name of ſtrontian, I remarked, that this earth was, when calcined, very ſoluble in boiling water, and that a portion became ſeparated in a cryſtalline form by cooling. I alſo remarked, that barytes acquired by calcination the property of becoming ſoluble in boiling water, and alſo affording cryſtals by cooling. It is in this ſtate of cryſtallization that ſtrontian and barytes muſt be found pure; and if they are of the ſame nature, as ſome chemiſts conceive, they ought to give ſimilar reſults when ſubmitted to comparative experiments. It has been thought that the properties which this earth exhibits in its combinations with the acids might be attributed to the preſence of carbonate of lime in the carbonate of ſtrontian; but calcined ſtrontian diſſolved in water cannot be ſuſpected to contain lime, and it is therefore in this ſtate that I have thought proper to procure ſome, in order to examine it comparatively with barytes, which I have alſo taken the pains to prepare in like manner in cryſtals, and perfectly deprived of carbonic acid. I ſhall here relate to the Inſtitute the experiments which I have made on this ſubject.

1. I firſt put into a ſmall capſule ſixteen grains of pure cryſtallized barytes, with thirty-ſix grains [demi-gros] of pure nitric acid of 12½°. The combination took place with diſengagement of caloric, and the cryſtals of barytes without being diſſolved ſoon preſented opaque cryſtals, which were nitrate of barytes. I added 144 grains [deux gros] of diſtilled water, and the whole was diſſolved.

2. Sixteen grains of cryſtals of ſtrontian were treated with thirty-ſix grains of the ſame acid. The combination took place with diſengagement of caloric, but the cryſtals were diſſolved, and did not become opaque as in the preceding experiment. I added 144 grains of water, in order to have a ſolution ſimilar in ſtrength to the former.

3. A portion of the ſolution, No. 1, or that of nitrate of barytes prepared with pure barytes, being put into a glaſs, I added to it a pretty conſiderable quantity of pruſſiate of pot-aſh. A precipitation took place, and the liquor was not afterwards precipitated by the addition of carbonate of potaſh.

The other portion of the nitrate being evaporated, afforded cryſtals of barytic nitrate in octahedrons.

4. A portion of the ſolution, No. 2, or of the nitrate of pure ſtrontian, tried in like manner with pruſſiate of pot-aſh, afforded a ſlight precipitation; but, although I added an exceſs of the pruſſiate, the ſupernatant liquor ſtill yielded a pretty abundant precipitate with carbonate of pot-aſh.

The other portion of the ſolution of nitrate of ſtrontian being evaporated, afforded octahedral cryſtals, which were more ſoluble than thoſe of the barytic nitrate.

5. Fifty-four grains of barytes, pure and in cryſtals, being treated with 144 grains [deux gros] of weak nitric acid, the ſolution took place with heat, but without effervefcence. I added 144 grains of diſtilled water, and placed the ſolution to evaporate in a ſand-bath ſlightly heated; and when a portion of the water was evaporated, there were formed cryſtals in hexagonal plates ſimilar to thoſe of muriate of barytes. Their weight was thirty-two grains.

Twelve grains of this muriate, triturated in a glaſs mortar with half an ounce of alcohol, were

THE NEW YORK
PUBLIC LIBRARY

II.

Abſtract of Two Memoirs on a New Method of obtaining Barytes pure, and on the Properties of this Earth compared with thoſe of Strontian. By Citizens FOURCROY and VAUQUELIN. Read to the National Inſtitute, 30th April and 21ſt September 1796*.

THE difficulty of obtaining barytes pure, and the almoſt total impoſſibility of ſeparating this earth from the carbonic acid by calcination of its carbonate, are already ſufficiently known. In a courſe of Experiments undertaken jointly by Citizens Fourcroy and Vauquelin, to determine the characters and diſtinctive properties of the ſalts and compounds formed by barytes, one of the points which they were moſt deſirous of attaining, was of courſe to obtain this earth in a real ſtate of purity. Citizen Vauquelin having at length diſcovered a means of effecting this purpoſe by the decompoſition of nitrate of barytes by the action of fire, it became more eaſy to thoſe chemiſts to examine ſuch properties of this earth as had hitherto been unknown to them. The two memoirs of which we are here about to give an abſtract, ſhow the moſt remarkable properties which characterize both this earth and ſtrontian.

1. Nitrate of barytes in octahedral cryſtals, being expoſed to the action of fire in a porcelain retort, melts, ſwells, and gives out much oxygenous and azotic gas, with ſcarcely any nitrous vapour; and the retort being ſuffered to cool when no more elaſtic fluid is diſengaged, there is found on breaking it a grey, ſolid, but ſomewhat porous, maſs of a harſh taſte, and more cauſtic than quick-lime, which is pure barytes.

2. Expoſed to the blow-pipe on a piece of charcoal, this earth fuſes, bubbles up, and runs into globules which quickly penetrate into the charcoal.

3. In the air it effloreſces, cracks, burſts, ſwells up, heats and whitens, and, becoming thus rapidly flaked, it abſorbs 0.22 of its weight of water and carbonic acid.

4. It abſorbs water with extreme avidity, melts with hiſſing, heats conſiderably, ſolidifies the water, and cryſtallizes and hardens with it in ſuch a manner as to become a very tenacious cement, capable of adhering very ſtrongly to glaſs. A little more water changes it into a very bulky white powder. If it be entirely covered with water, it diſſolves in it with a violent hiſſing, and then cryſtallizes in tranſparent needles, which adhere together, and form compages like the particles of beaten plaſter.

5. Cold water diſſolves a twenty-fifth part of its weight; boiling water more than half: the latter by cooling depoſits very beautiful tranſparent priſms, which effloreſce and become pulverulent when expoſed to the air.

6. A ſolution of barytes has its ſurface more readily incruſted with a pellicle by expoſure to the air, and depoſits a more abundant precipitate by carbonic acid than lime-water.

7. The oxalic, citric, phoſphoric, and phoſphorous acids precipitate this ſolution, and the precipitates are re-diſſolved by the addition of an exceſs of the acids by which they are formed.

8. This ſolution decompoſes thoſe of ſoap, and of the nitrates of mercury, lead, and

---

* Tranſlated from Annales de Chimie, XXI. p. 276, by J. F——r.

ſilver, precipitating the firſt black, the ſecond white, and the laſt of a fawn-colour. An exceſs of the barytic ſolution renders the oxides of lead and ſilver ſoluble.

9. Barytes thus prepared is ſoluble in alcohol. It is dreadfully poiſonous, and kills animals. Its moſt remarkable and moſt characteriſtic properties are its extreme cryſtallizability, which diſtinguiſhes it from all the other earths hitherto known in chemiſtry, and its great ſolubility in water.

The diſcovery by Meſſrs Hope and Klaproth of theſe two latter properties in ſtrontian induced us however to think for a while, with Citizens Pelletier and Coquebert, that barytes was ſo ſimilar to it as that they could not but be conſidered as very nearly allied, and perhaps even one and the ſame earth; but having received ſome fragments of ſtrontianite, of which we had not before been enabled to inveſtigate the properties ourſelves, we were eager to examine this new earth comparatively with barytes; and the reſult of this examination, as related in the ſecond memoir, which we are now abſtracting, ſhewed us that, notwithſtanding ſome very ſtrong analogies, theſe two earthy ſubſtances are really different, as Citizen Pelletier has concluded from his own reſearches relative to this ſubject. The following are the principal facts ſtated in this ſecond memoir.

Native carbonate of ſtrontian has a ſhade of light green, which that of barytes has not; it requires a little more muriatic acid for its ſolution, and contains more carbonic acid. Muriate of ſtrontian cryſtallizes by cooling, whilſt that of barytes cryſtallizes by evaporation: the former in hexagonal priſms, the latter in inclined plates. The ſolution of muriate of ſtrontian thickens into a jelly by evaporation, whilſt that of muriate of barytes dries in cryſtalline plates: the former, which is very ſoluble in alcohol, makes it burn with a flame of a beautiful purple; the latter, which is almoſt inſoluble in this liquid, gives it a yellow flame.

The greateſt and moſt remarkable difference which exiſts between ſtrontian and barytes is, that ſtrontian has leſs affinity with acids than barytes, and even than the fixed alkalies; ſo that a ſolution of barytes precipitates muriate of ſtrontian in white flocks.

Nitrate of ſtrontian differs from that of barytes; 1ſtly, in giving more nitrous vapour when decompoſed by fire, in conſequence of its retaining the nitric acid leſs ſtrongly than barytes; and, 2dly, in being three times as ſoluble in water.

Strontian obtained from its nitrate by the action of fire is leſs harſh than barytes; it does not fuſe in like manner by the blow-pipe, but glitters with a phoſphoric flame; it is almoſt ten times leſs ſoluble in water; and it ſeems to be leſs powerful in its attractions than lime, which it does not ſeparate from acids. When precipitated by the oxalic acid, it is not re-diſſolved by an exceſs of this acid, as barytes is; but the ſulphate of ſtrontian is on the contrary reſoluble by an exceſs of ſulphuric acid, which that of barytes is not.

The aqueous ſolution of ſtrontian is not precipitated by the gallic acid, whereas that of barytes affords with this acid a greeniſh precipitate.

The authors of thoſe memoirs conclude from the Experiments (the reſults of which are here ſtated) that ſtrontian exhibits more properties which are different from thoſe of barytes than of ſuch as are analogous to them; and that, ſtriking as are the latter, it muſt nevertheleſs be concluded, from the whole of the phænomena, that theſe earths are really two different ſubſtances; a concluſion which is more particularly evident in reſpect of their different degrees of affinity, and of ſtrontian not being a poiſon, whereas barytes is a very active and violent one.

Several of the properties which they have deſcribed had been before obſerved by Meſſrs. Klaproth, Hope, and Pelletier; but ſeveral alſo are of their own diſcovery. We have not here ſpoken of ſuch of the reſults afforded by the two earths in their combinations, as were perfectly ſimilar to each other; it having been our principal object to point out thoſe ſpecific characters by which they are to be diſtinguiſhed.

It is clear that we may reaſonably entertain a hope, that more extenſive experiments ſhall one day point out uſes both of barytes and ſtrontian, as well as of their combinations, of great importance in reſpect of the arts, when they ſhall have been found more abundantly in nature, and when the progreſs of ſcience ſhall have perfected the means of obtaining them in a ſtate of purity. The extreme ſolidity which barytes aſſumes when ſlaked, may lead us to preſume that it may be of uſe for making very hard and durable cements, or perhaps even mordants for the application of different matters on ſtone, glaſs, pottery and other ſubſtances of a like nature.

III.

Extract of a Letter from Count MUSSIN PUSCHKIN, *Vice-Preſident of the Department of Mines at Peterſburg. On the Salts, Precipitates, and Amalgam of Platina; on Cobalt; on Antimonial Soap; and on the Decompoſition of Soap by the Acid Extracts of Colouring Matters**.

I. *On the Salts and Precipitates of Platina.*

THE brick-coloured ſalt, which is obtained by pouring muriate of ammoniac into a ſolution of platina, is totally ſoluble in water, and depoſits, when diſſolved in boiling water, a black matter, which M. Fourcroy believes to be iron; but, in my opinion, it ſeems rather to be plumbago, though I have not yet made the requiſite experiments to aſcertain this fact. A ſpecimen of this ſubſtance accompanies this letter.

In order that the whole of the ſalt may diſſolve, it is neceſſary to boil it for a long time with water; and when it is perceived that no more of the ſalt is taken up, the water muſt be decanted, and freſh water poured into the veſſel. An ounce of the ſalt required upon the whole between eight and nine pounds of water poured on and decanted at five different times. By this means the whole of the ſalt was not only diſſolved, but its colour immediately became a beautiful orange; and by theſe cryſtallizations, by evaporation on the water-bath and new ſolutions, after which the fluid was left to a very ſlow evaporation, the orange-colour of the cryſtals became converted into a moſt brilliant topaz-colour, and by new ebullitions was found no longer to depoſit any black matter. I have not yet ſucceeded in obtaining larger cryſtals than thoſe which Werner calls *very ſmall*; but, by the aſſiſtance of a good Engliſh microſcope, all thoſe which the topaz-coloured ſalt afforded were obſerved to be perfectly tranſparent, and with ſcarcely any exception affected the form either of an octahedral pyramid, or that of a polyhedron compoſed of ſix perfect hexagons united together by eight iſoſceles triangles. There were ſome leſs regular cryſtals, which partook of the nature of both theſe figures.

---

* Annales de Chimie, XXIV. 205.

The alkalis precipitated (though with difficulty) from the aqueous ſolutions a ſmall quantity of yellow powder, reſembling Naples-yellow. I ſuppoſe, however, that this precipitate was compoſed of cryſtals, though none could be diſcerned with the magnifier: and it did not appear to be ſoluble in water; for the nitrous acid, by remaining upon it for forty-eight hours, did not appear to have affected it in the leaſt. The light yellow precipitate which is obtained by vegetable alkali, after ſal-ammoniac has thrown down the whole of the brick-coloured precipitate, was ſeparated by the filter, and ſubmitted without edulcoration to the action of nitrous acid in the proportion of nearly half an ounce of the acid to one dram of the precipitate, in conical veſſels, ſuch, for example, as a wine-glaſs, in which the precipitate occupied the ſmalleſt poſſible ſpace, and the acid covered it a finger's depth, and preſented a conſiderable ſurface to the air. In three or four days the acid takes up more or leſs, according to the temperature, and aſſumes the conſiſtence of a jelly, at firſt yellow, but afterwards of a chryſolite green more or leſs conſiſtent, but always in a certain degree moiſt. Before the blow-pipe this jelly becomes converted into a black ſubſtance, probably the imperfect oxide of platina. This experiment appeared ſurpriſing to me at firſt; but I recollected that Margraff, by detonating ſaltpetre upon platina, and then waſhing and filtering, had likewiſe obtained a jelly which may have been of the ſame nature as mine. I muſt obſerve that I ſatisfied myſelf in the moſt convincing manner, that the acid and alkali I made uſe of were abſolutely pure, and that the platina alone, that is to ſay in the metallic ſtate, did not convert the nitrous acid into a jelly.

Urine, whether freſh or putrid, precipitates platina in the form of a ſuper-compound ſalt, at the ſame time that a grey yellow precipitate is formed, which is neither ſaline nor ſoluble in water. Part of this precipitate is formed a few moments after the urine is poured into the ſolution of platina: the ſalt is formed afterwards. In order to obtain this ſalt, it is neceſſary that the ſolution of platina ſhould be very concentrated, and carry with it an abundant portion of the firſt precipitate. I have not yet attempted to diſſolve this ſalt in boiling water, but I think it would exhibit a very beautiful topaz-yellow; for ſome of the cryſtals, when obſerved in the microſcope, were ſemi-tranſparent, and had not the red colour of the ſalt precipitated by the muriate of ammoniac. With reſpect to the grey-yellow precipitate, I ſuppoſe it to be phoſphate of iron contaminated with ſome foreign ſubſtance, and perhaps mixed with a ſmall quantity of the precipitate of platina.

When urine is poured into a ſolution of the red ſalt in water, a beautiful lemon-yellow precipitate is formed, and the grey matter afterwards falls down ſlowly. This may perhaps afford a new method of ſeparating iron from platina, if this grey precipitate ſhould in fact prove to be the phoſphate of iron.

II. *On the Amalgam of Platina.*

REFLECTING on the facility with which the ſuper-compound ſalts of platina are reduced in the fire, I thought that an amalgam of platina might perhaps be obtained by triturating theſe ſalts with mercury. To aſcertain this, I took a dram of the orange-coloured ſalt of platina, and triturated it with an equal weight of mercury in a mortar of chalcedony.

In a few minutes the ſalt loſt its colour, becoming at firſt brown, and afterwards greeniſh-brown; the matter was reduced to a very fine powder. Another dram of mercury was then added and the trituration continued, when the powder became grey; a third dram of mercury began to form an amalgam, and when the quantity amounted to ſix drams the amalgam was perfect. The whole operation ſcarcely employed twenty minutes. I added mercury to the quantity of nine times the weight of the ſalt, notwithſtanding which the amalgam was very tenacious; a fact which is indeed ſurpriſing, when it is conſidered that the ſalt contains no more than about 40 parts of platina in the hundred. In my Experiment therefore 24 grains of platina were ſufficient to give conſiſtence to 540 grains of mercury. This amalgam was eaſily ſpread out under the peſtle; it perfectly well received the impreſſions of the moſt delicate ſeals; its grain was very cloſe and brilliant, and in no reſpect inferior to that of the beſt amalgam of tin.

Though the ſight of a perfect amalgam of platina made in a few minutes, the production of which had coſt Lewis ſeveral weeks, and Sickingen ſeveral days, gave me pleaſure, I had not leſs ſatisfaction in obſerving the ſingular phenomenon I ſhall proceed to deſcribe:

Being deſirous of clearing my amalgam of its ſaline parts by waſhing, I triturated it in water in a glaſs mortar, and had ſcarcely given a few ſtrokes of the peſtle before I obſerved the ſurface of the amalgam to be covered with a black powder, mixed with ſome yellow particles. In leſs than ten minutes the whole of the amalgam diſappeared, inſtead of which I had this black powder. The yellow matter, which conſiſted of undecompoſed ſalt of platina, having likewiſe diſappeared by trituration; the black powder was ſeen, upon decanting the water, in extremely brilliant maſſes (*parcelles*), which probably were platina. The mercury had therefore paſſed with the greateſt facility to the ſtate of black oxide, nearly approaching the metallic ſtate. A portion of running mercury was found beneath; in the quantity of about two drams. Having taken a portion of the amalgam in the palm of my hand, and rubbed it with my finger, the ſame decompoſition took place in a few inſtants, and left a black powder interſperſed with brilliant particles. On the ſuppoſition that this oxide would eaſily paſs to the ſtate of cinnabar in the humid way, I poured the ſulphate of ammoniac (Beguin's volatile liver of ſulphur) upon it, and in leſs than 24 hours the powder became of a dull red, intermixed with metallic particles, which were evidently platina. I aſcertained afterwards by ſome experiments, that various metallic ſubſtances, and all the animal matters which I tried, decompoſe this amalgam by ſimple contact. For theſe reaſons it is abſolutely neceſſary to uſe a glaſs or ſiliceous peſtle and mortar in the compoſition of the amalgam.

In what manner are we to explain the ſpeedy reduction of the platina without oxidation of the mercury during the formation of the amalgam, as well as the much more ſpeedy oxidation of the mercury when brought into contact with water, or metallic or animal matters? This appears to me to be a very difficult queſtion, of which I expect the ſolution from more enlightened chemiſts. For the ſmall quantity of oxygene in the ſalt of platina is ſurely inſufficient to deprive ſo large a portion of mercury of its metallic ſtate; beſides which, in the dry trituration, ſuch as that which is made in the hollow of the hand, the acid is not diſſolved. Is it likely that there may be a decompoſition of atmoſpheric air, and a diſengagement of caloric from the vital air * ? This muſt be determined by new experiments, which I have good reaſon to expect from the illuſtrious philoſopher to whom I have the honour to addreſs myſelf. I ſublimed the oxided powder of mercury in a ſmall glaſs retort. It afforded metallic mercury, and the muriate of mercury. The ſubſtance which remained at the bottom of the retort was friable, light, and of a colour inclining to a deep grey yellow. The ſmall quantity of the powder of amalgam which I ſubjected to experiment was not ſufficient to admit of an examination of this reſidue. I ſhall not fail to repeat the experiment more at large, and acquaint you with the reſults. In the mean time I requeſt that the contents of this letter may ſerve as a ſupplement to the hiſtory of platina, and beg that you will give it a place in your Annals under that title.

III. *Concerning Cobalt.*

WITH regard to the experiments I have made on cobalt, I muſt ſuſpend my information until I ſhall have been able to procure the regulus of cobalt in a quantity ſomewhat conſiderable, for the purpoſe of repeating my experiments in the large way. Thoſe which I have already made have afforded ſeveral intereſting phenomena, particularly with regard to the effects of cold as a chemical agent. At preſent, I ſhall only remark, that the oxides of cobalt, which have hitherto been ſuppoſed, after the methods of the beſt chemiſts, to be abſolutely pure, are nevertheleſs contaminated with much foreign matters.

IV. *On the Antimonial Soap.*

PROFESSOR RUDOLPH has made an antimonial ſoap, after my method of preparing the mercurial ſoap. He finds it very active.

V. *On the Decompoſition of Soaps by the Acid Extracts of Colouring Matters.*

I HAVE obſerved, in decompoſing ſoaps, that the acid extracts of colouring matter, ſuch, for example, as that of Brazil wood by the diluted acetous acid, perfectly decompoſe ſoaps, whether made with oil or wax. The oleo-colorant or cero-colorant compounds thus obtained are abſolutely inſoluble in water, and afford extremely brilliant colours, which may perhaps be advantageouſly uſed in painting, particularly encauſtic †. It would be likewiſe intereſting to aſcertain preciſely whether the colouring matter has not more analogy with earths than has hitherto been thought, and whether it would not be advantageous, particularly in red dies, to employ the ſoaps of tin and of cochineal.

---

* The author thinks, and with reaſon, that the oxygene united to the platina is far from being ſufficient to oxide ſo large a quantity of mercury ; but he has not perhaps ſufficiently attended to the facility with which the leaſt oxidable metals paſs to the ſtate of oxide, when they are rendered fluid, and their parts diſengaged by their affinity with mercury. This has long been obſerved with regard to the amalgam of gold, which ſoon becomes covered with a purple oxide by mere expoſure to the air. Citizen Guyton, in a memoir on certain properties of platina, read to the National Inſtitute the firſt Meſſidor, in the year 4, has exhibited a new proof of this principle in the oxidation of an amalgam of platina, obtained by the direct combination of the two metals, by means of an elevated temperature, and by the ſimple effect of the affinity which was announced by the rank which platina occupies in the ſcale of adheſions to mercury. *Note of the Editors of the Annales.*

† Citizen Chauſſier has already communicated, in his leſſons to the Polytechnic School, ſeveral experiments of this kind, which have perfectly ſucceeded. The ſoap of copper afforded a very brilliant green, which it is preſume

IV.

Obſervations on the Fundamental Property of the Lever; with a Proof of the Principle aſſumed by Archimedes in his Demonſtration. By the Rev. S. Vince, A.M. F.R.S.*

THE want of a demonſtration of the property of the lever upon clear and ſelf-evident principles has juſtly been conſidered as a great deſideratum(defect) in the ſcience of mechanics, as the moſt important parts of that branch of natural philoſophy are founded upon it. Archimedes was, I believe, the firſt who attempted it. He ſuppoſes, that if two equal bodies be placed upon a lever, their effect to turn it about any point is the ſame as if they were placed in the middle point between them. This propoſition is by no means ſelf-evident, and therefore the inveſtigation which is founded upon it has been rejected as imperfect. Huygens obſerves, that ſome mathematicians, not ſatiſfied with the principle here taken for granted, have, by altering the form of the demonſtration, endeavoured to render its defects leſs ſenſible, but without ſucceſs. He then attempts a demonſtration of his own, in which he takes for granted, that if the ſame weight be removed to a greater diſtance from the fulcrum, the effect to turn about the lever will be greater: this is a principle by no means to be admitted, when we are ſuppoſed to be totally ignorant of the effects of weights upon a lever at different diſtances from the fulcrum. Moreover, if it were ſelf-evident, his demonſtration only holds when the lengths of the arms are commenſurable. Sir I. Newton has given a demonſtration, in which it is ſuppoſed, that if a given weight act in any direction, and any radii be drawn from the fulcrum to the line of direction, the effect to turn the lever will be the ſame, on whichever of the radii it acts. But ſome of the moſt eminent mathematicians ſince his time have objected to this principle, as being far from ſelf-evident; and, in conſequence thereof, have attempted to demonſtrate the propoſition upon more clear and ſatisfactory principles. The demonſtration by M'Laurin, as far as it goes, is certainly very ſatisfactory; but as he collects the truth of the propoſition only from induction, and has not extended it to the caſe where the arms are incommenſurable, his demonſtration is imperfect. The demonſtration given by Dr. Hamilton, in his Eſſays, depends upon this propoſition, that when a body is at reſt, and acted upon by three forces, they will be as the three ſides of a triangle parallel to the directions of the forces. Now this is true when the three forces act at any point of a body; whereas, conſidering the lever as the body, the three forces act at different points, and

---

preſumed was known to the ancient maſters. On this ſubject, conſult the 3d cahier of the Journal of the Polytechnic School, p 426 and 427.

* Phil. Tranſ. M.DCC.XCIV. 33.

therefore the principle, as applied by the author, is certainly not applicable. If in this demonſtration we ſuppoſe a plane body, in which the three forces act, inſtead of ſimply a lever, then the three forces being actually directed to the ſame point of the body, the body would be at reſt. But in reaſoning from this to the caſe of the lever, the ſame difficulties would ariſe as in the proof of Sir I. Newton. But admitting that all other objections could be removed, the demonſtration fails when any two of the forces are parallel. Another demonſtration is founded upon this principle, that if two non-elaſtic bodies meet with equal quantities of motion, they will, after impact, continue at reſt; and hence it is concluded, that if a lever which is in equilibrio be put in motion, the motions of the two bodies muſt be equal; and therefore the preſſures of theſe bodies upon the lever at reſt to put it in motion, muſt be as their motions. Now in the firſt place, this is comparing the effects of preſſure and motion, the relation of the meaſures of which, or whether they admit of any relation, we are totally unacquainted with. Moreover, they act under very different circumſtances; for in the former caſe the bodies acted immediately on each other, and in the latter they act by means of a lever, the properties of which we are ſuppoſed to be ignorant of. When forces act on a body conſidered as a point, or directly againſt the ſame point of any body, we only eſtimate the effect of theſe forces to move the body out of its place, and no rotatory motion is either generated, or any cauſes to produce it conſidered in the inveſtigation. When we therefore apply the ſame propoſition to inveſtigate the effect of forces to generate a rotatory motion, we manifeſtly apply it to a caſe which is not contained in it, nor to which there is a ſingle principle in the propoſition applicable. The demonſtration given by Mr. Landen, in his Memoirs, is founded upon ſelf-evident principles; nor do I ſee any objections to his reaſoning upon them. But as his inveſtigation conſiſts of ſeveral caſes, and is beſides very long and tedious, ſomething more ſimple is ſtill much to be wiſhed for, proper to be introduced in an elementary treatiſe of mechanics, ſo as not to perplex the young ſtudent, either by the length of the demonſtration or want of evidence in its principles. What I here propoſe to offer will, I hope, render the whole buſineſs not only very ſimple, but alſo perfectly ſatisfactory.

The demonſtration given by Archimedes would be very ſatisfactory and elegant, provided the principle on which it is founded could be clearly proved, viz. that *two equal powers at the extremities, or their ſum at the middle, of a lever, would have equal effects to move about any point.* Now, that the effects will be the ſame, ſo far as reſpects any progreſſive motion being communicated to the lever when at liberty to move freely, is ſufficiently clear; but there is no evidence whatever that the effects will be the ſame to give the lever a rotatory motion about any point, becauſe a very different motion is there produced, and we are ſuppoſed to know nothing about the efficacy of a force at different diſtances from the fulcrum to produce ſuch a motion. Beſides, the two motions are not only different, but the *ſame* forces are known to produce *different* effects in the two caſes; ſo that in the former caſe the two equal powers, at the extremities of the arms, produce equal effects in generating a *progreſſive* motion; but in the latter caſe, they do *not* produce *equal* effects in generating a *rotatory* motion. We cannot, therefore, reaſon from one to the other. The principle, however, may be thus proved:

Let A C, Pl. XXII. Fig. 1. be two equal bodies on a ſtraight lever, A P moveable about P. Biſect A C in B : produce P A to Q and take B Q = B P, and ſuppoſe the end Q to be ſuſtained by a prop. Then as A and C are ſimilarly ſituated in reſpect to each end of the lever, that is, A P = C Q, and A Q = C P, the prop and fulcrum muſt bear equal parts of the whole weight; and therefore the prop at Q will be preſſed with a weight equal to A. Now take away the weights A and C, and put a weight at B equal to their ſum; and then the weight at B being equally diſtant from Q and P, the prop and fulcrum muſt ſuſtain equal parts of the whole weight, and therefore the prop will now alſo ſuſtain a weight equal to A. Hence, if the prop Q be taken away, the moving force to turn the lever about P in both caſes muſt evidently be the ſame; therefore the effects of A and C upon the lever to turn it about any point, are the ſame as when they are both placed in the middle point between them. And the ſame is manifeſtly true if A and C be placed without the fulcrum and prop. If therefore A C be a cylindrical lever of uniform denſity, its effect to turn itſelf about any point will be the ſame as if the whole were collected into the middle point B; which follows from what has been already proved, by conceiving the whole cylinder to be divided into an infinite number of laminæ perpendicular to its axis of equal thickneſſes.

The principle therefore aſſumed by Archimedes is thus eſtabliſhed upon the moſt ſelf-evident principles, that is, that equal bodies at equal diſtances muſt produce equal effects; which is manifeſt from this conſideration, that when all the circumſtances in the cauſe are equal, the effects muſt be equal. Thus the whole demonſtration of Archimedes is rendered perfectly complete, and at the ſame time it is very ſhort and ſimple. The other part of the demonſtration we ſhall here inſert for the uſe of thoſe who may not be acquainted with it.

Let X Y, Fig. 2. be a cylinder, which biſect in A, on which point it would manifeſtly reſt. Take any point Z, and biſect Z X in B, and Z Y in C; then, from what has been proved, the effects of the two parts Z X, Z Y, to turn the lever about A, are the ſame as if the weight of each part were collected into B and C reſpectively, which weights are manifeſtly as Z X, Z Y, and which therefore conceive to be placed at B and C. Now $AB = AX - XB = \frac{1}{2}XY - \frac{1}{2}XZ = \frac{1}{2}YZ$; and $AC = AY - YC = \frac{1}{2}XY - \frac{1}{2}ZY = \frac{1}{2}XZ$; conſequently $AB : AC :: \frac{1}{2}YZ : \frac{1}{2}XZ :: YZ : XZ ::$ the weight at C : the weight at B.

The property of the ſtraight lever being thus eſtabliſhed, every thing relative to the bent lever immediately follows.

V.

Obſervations on the Acid of Tin, and the Analyſis of its Ores. Read at the Sitting of the Claſs of Mathematical and Philoſophical Sciences of the National Inſtitute of France, the firſt of Meſſidor, in the Year 5. By Citizen GUYTON*.

IT has long been obſerved that the nitric acid calcines tin inſtead of diſſolving it, and the phlogiſtic hypotheſis was incapable of explaining this phenomenon. It remained among the number of facts for which the moſt fruitful imagination could not afford a probable ſolution. This difficulty has vaniſhed ſince it has been diſcovered that the metals are capable of being converted into acids by a ſufficient quantity of oxygene. Tin is one of the metals which muſt have moſt effectually contributed to fix the attention of chemiſts upon this combination. In this manner it is ſeen that the precipitation of gold by tin in the form of a purple oxide is ſimply, as I have announced in the Elements of Chemiſtry of the Academy of Dijon, the reſult of the action of the ſtannic acid upon gold. This oxigenation of tin is much more evidently ſeen in the operation which I have deſcribed in the firſt volume of the Chemical Dictionary of the Encyclopedia, p. 632 ; an operation which ſeems well calculated to exhibit at the ſame inſtant every fundamental principle of the pneumatic chemiſtry. For when the diluted nitric acid and tin are treated ſimply in the retort, the reſult is oxide of tin and nitrate of ammoniac, the latter of which is formed by the azote or baſis of the acid, and the hydrogene of the water, without the appearance of nitrous gas at any period of the operation.

---

* Inſerted in the XXIVth volume of the Annales de Chimie.

No further difficulty remained therefore with reſpect to the oxidation of tin by the nitric acid, as it is evidently effected by the decompoſition of the acid and the affinity of the tin for oxygene. But no account has yet been given of the ſtate in which this metal exiſts in its ores, particularly in the cryſtals of tin (zinn-graupen, zinnſtein, of the Germans), nor of the cauſes which oppoſe their ſolution in acids.

This queſtion preſented itſelf to my conſideration on reading the publication of Mr. Klaproth on the ores of tin in the ſecond volume of Analyſes with which he has enriched chemiſtry and mineralogy.

He begins by comparing the products of the ſame ore treated with alkaline reducing fluxes, and with charcoal alone. The difference convinced him, that in the firſt proceſs the alkali retains a conſiderable portion of the metal in the ſtate of oxide. He afterwards endeavoured to complete the analyſis in the humid way. For this purpoſe he followed Bergman's proceſs, which conſiſts in digeſting the powdered ore in concentrated ſulphuric acid; adding muriatic acid after the cooling, and precipitating the ſolution by ſoda. By this treatment 131 parts of precipitate ought to be afforded for every 100 parts of metal contained in the ore. Mr. Klaproth, in fact, obtained a ſolution of about 0,19 of that rather ſcarce tin ore which the Engliſh call wood-tin; but he could not ſucceed in charging the muriatic acid with any perceptible quantity of tin, by ſubjecting the other more common varieties to the ſame treatment.

Reflecting on the nature of the obſtacle which might probably oppoſe the action of the muriatic acid on a ſubſtance which he eſtimated to contain 0,99 of oxide of tin, he imagined that it could be nothing but an exceſs of oxygene.

To remove this exceſs, he firſt attempted to treat the ore in the retort with a mixture of ſulphur. Moſt of the ſulphur was ſublimed, and the reſidue, ſlightly agglutinated, had ſtill the ſame grey whitiſh appearance as after the pulverization, excepting that ſome brilliant gold-coloured ſpecks were ſeen; namely, ſulphate of tin, or aurum-muſivum. The mineral, however, was not more ſoluble in the muriatic acid than before.

After many other trials equally unſucceſsful, the celebrated chemiſt of Berlin had recourſe to that ſubſtance which in his hands has become ſo powerful an inſtrument of analyſis.

lyſis. He treated his ores with ſix parts of potaſh in a crucible of ſilver, and the ſucceſs was beyond his expectations. By the firſt operation, 0,91 were rendered ſoluble in water, precipitated and afterwards taken up by the muriatic acid. The muriate of tin being then decompoſed by carbonate of ſoda, a pure oxide was obtained, which was eaſily re-diſſolved in the ſame acid, and precipitated by zinc. This precipitate, fuſed in a crucible with tallow, afforded a button of the ſame weight as had been obtained from the ſame kind of ore in the dry way.

By this means he was authoriſed to conclude as follows, with regard to the brown ore of Schlackenwald:

In 100 parts	Tin - - - -	75
	Iron - - - -	0,5
	Silex - - - -	0,75
	Oxygene - - -	23,75
		100

I have treated in the ſame manner a cryſtal of tin ore of this kind, alſo from Schlackenwald; not becauſe I had any doubt of the ſucceſs announced by a chemiſt who has ſo long been in poſſeſſion of well-earned confidence; but this ſucceſs ſeemed to be a confirmation of the conjecture on which Mr. Klaproth had founded his experiments, and I ſuppoſed that there was good reaſon to doubt whether the complete ſaturation or ſuperſaturation of tin by oxygene were the true cauſe of its inſolubility in the muriatic acid. For I could not at any period of the operation diſcern either the ſubſtance which might ſeize this exceſs of oxygene, or any trace of the phenomena which muſt have accompanied its diſengagement.

In order to obſerve the circumſtances with more facility, I operated in a ſmall crucible of platina over a reverberatory lamp of Argand (ſur une lampe d'Argand à réchaut*). Fifty-five centigrammes of the brown cryſtals of tin, reduced into fine powder, were well mixed with ſix times the quantity of potaſh purified by alcohol, and dried. The mixture was moiſtened with a few drops of water, then evaporated to dryneſs, and afterwards heated to the commencement of fuſion. After the firſt operation, hot water was poured on the maſs, and took up more than half the mineral. Muriatic acid firſt precipitated it, and afterwards diſſolved it with the greateſt facility. The precipitate or metallic oxide, reproduced by carbonate of potaſh, was in fact completely ſoluble by the ſame acid, as Mr. Klaproth had affirmed.

But after having thus witneſſed the facts, my firſt doubt concerning the direct ſolubility of this ore was rather ſtrengthened than removed. For it cannot be affirmed that the exceſs of oxygene was diſſipated during the fuſion with potaſh, becauſe the metal could not form a ſoluble combination with the alkali, but in conſequence of its oxidation to the laſt degree, or, to ſpeak more correctly, its acidification; ſo that the lixivium of the filtered reſidue is a true ſtannate, or perhaps a ſtannite †, of tin (potaſh).

---

* The author intends ſoon to give a deſcription of this lamp.

† Mr. Hatchett in his paper on the molybdate of lead, and the molybdic acid, inſerted in the Philoſophical Tranſactions of London for 1796, compares preciſely the action of the nitric acid on molybdena, to ſuperoxygenate it, to that which produces the ſame effect on tin. It may converſely be inferred that tin is acidified like molybdena. G.

In the mean time, as we are forced to admit that all the oxygene of the ore is found in the alkaline ſolution, the more effectual action of the acid upon the metal cannot be ſuppoſed to ariſe from the loſs of a portion of this principle, becauſe it continues ſtill in the ſame ſtate of ſaturation; neither is there any ſign of oxygenated muriatic acid gas; and it would likewiſe be difficult to conceive, why in this circumſtance there ſhould rather be a diſengagement of the gas than when the acid is digeſted on the ore, as is obſerved in the oxides of manganeſe and of lead.

To eſtabliſh this point of theory on a more deciſive experiment, I diſſolved ſix grammes of tin in the nitric acid, and evaporated ſeveral times to dryneſs the new acid which was ſucceſſively poured on. I ſuppoſe it cannot be doubted but that the tin in this ſtate had combined with as much oxygene as it is capable of fixing. Nevertheleſs, the maſs of white oxide, waſhed until the water which came off cauſed no further change in vegetable colours, was very ſoluble in muriatic acid.

What therefore is the cauſe of the inſolubility of the ore, which conſiſts alſo of tin and oxygene with ſcarce one hundredth part of foreign matter? This cauſe is to be found only in the ſtate of aggregation of the latter. This aſſertion cannot be thought ſtrange, except on account of the little regard which has hitherto been paid to its energy. If combinations be the reſult of affinity or elective attraction, is not this attraction itſelf a power which may be rendered ineffectual by the ſum of the forces which act in the contrary direction? Theſe truths ſurely cannot appear repulſive to Mr. Klaproth, who has rendered them ſo evident, by ſhewing that the ruby, the ſapphire and the adamantine ſpar, of which the elements are in their own nature ſo eaſily ſoluble, do not reſiſt the ordinary methods of analyſis but in conſequence of the aggregation of their integral parts.

VI.

On Fairy Rings.

THE appearance in the graſs, commonly called Fairy Rings, is well known. It conſiſts either of a ring of graſs of more luxuriant vegetation than the reſt, or a kind of circular path in which the vegetation is more defective than elſewhere. It appears to be pretty well aſcertained, that the latter ſtate precedes the former. Two cauſes are aſſigned for this phenomenon: the one, which cannot be controverted, is the running of a fungus; the other, which has been conſidered as an effuſion of theory, is grounded on a ſuppoſition that the exploſion of lightning may produce effects of the ſame kind on the ground as Dr. Prieſtley's battery was found to produce on the poliſhed ſurface of a plate of metal, that is to ſay, a ſeries of concentric rings. Some obſervations, which I find in my common-place book, appear to ſhew that this laſt effect may, in certain circumſtances, take place.

On Tueſday the 19th of June, 1781, a very powerful thunder-ſtorm paſſed over the weſtern extremity of London. I was then at Batterſea, and made no other remark on the phenomena than that the exploſions, which were very marked and diſtinct, were in many inſtances forked at the lower end, but never at the top; whence it follows, that the clouds were in the poſitive ſtate for the moſt part. On the following Sunday, namely the 24th, I happened

I happened to be in Kenſington Gardens, in every part of which extenſive piece of ground the lightning had left ſome marks of its agency, chiefly by diſcoloration of the graſs in zigzag ſtreaks, ſome of which were fifty or ſixty yards in length. Inſtances of this ſuperficial courſe of the lightning along the ground before it enters the earth are ſufficiently frequent. But the circumſtance which attracted my attention the moſt was ſeen in a ſmall grove of trees at the angular point of one of the walks. Plate XXII. figure 3. exhibits a ſketch of part of the gardens, in which the angular point is denoted by the letter A. Figure 4. repreſents the poſition of the trees. The numbers expreſs the diſtances between the trees in feet.

Cloſe to the ſtem of the tree A was a hole in the ground four inches long and two wide, proceeding to the ſouthward; and at two feet further to the ſouth was another ſimilar hole. Between theſe two holes the ground was torn away. This appearance is repreſented in figure 3, in which the letter A repreſents the trunk of the tree. The graſs was very much ſcorched to the diſtance of about three feet in every direction from the trunk, and in this burned ſpace there were ſeveral other ſmaller holes.

Cloſe to the trunk of B, on the ſouth ſide, there was a hole in the ground.

Near the tree C there was alſo a hole in the ground, and it was ſurrounded with a faint ring of burnt graſs at a little diſtance; but as the graſs was grown again, it ſeemed probable, that the ring was occaſioned by ſome earlier ſtroke than that on Tueſday.

The tree D was ſurrounded by a ring of 6 feet radius, and 18 inches broad. Within the ring the graſs was freſh; but on the ſurface of the ring the graſs and the ground were much burned. To the eaſtward of the tree upon the ring itſelf were two holes, in which the ground had the appearance of aſhes. The tree E had half a faint ring to the weſtward.

The tree F was ſurrounded by a faint ring of two yards radius. Within the ring the graſs was unhurt. To the weſtward, at about three feet diſtance from the inner ring, was part of another ſimilar ring, of much the ſame appearance; the verdure being unhurt in the interval between the rings.

I imagined the leaves of the trees were a little curled, but could obſerve no blaſted boughs; a circumſtance which, together with the other facts, appears to indicate that theſe appearances were produced by the recent ſtorm of the 19th.

VII.

Experimental Reſearches to aſcertain the Nature of the Proceſs by which the Eye adapts itſelf to produce diſtinct Viſion.

[Concluded from page 479.]

IN the eel there is a tranſparent horny convex covering at ſome diſtance before the eye, to defend it from external accidents. This covering to an eye fitted to ſee in air, would entirely take off the effects ariſing from change of figure in the cornea; but in water, where no ſuch change could be attended with advantage, ſuch a covering is employed as an external defence.

In the eyes of fiſhes the ciliary proceſſes are entirely wanting. The cryſtalline lens is ſpherical and imbedded in the vitreous humour, which is incloſed in cells of a ſtronger texture than in other animals.

The iris does not admit of motion; this is taken notice of by Haller; and the reaſon probably is, that the light in water is never too ſtrong for the eye to bear.

There is a muſcle ſituated between the retina and the ſclerotic coat, which is, as Mr. Home thinks, common to all fiſh. This muſcle is particularly deſcribed by Mr. Haller; and its uſe is ſtated to be that of bringing the retina nearer the cryſtalline lens for the purpoſe of ſeeing objects at a greater diſtance. Mr. Hunter called it the choroide muſcle, and has preſerved ſeveral preparations of it.

This muſcle has a tendinous centre round the optic nerve, at which part it is attached to the ſclerotic coat; the muſcular fibres are ſhort, and go off from the central tendon in all directions; the ſhape of the muſcle is nearly that of a horſe-ſhoe; anteriorly it is attached to the choroide coats, and by means of that to the ſclerotic. Its action tends evidently to bring the retina forwards; and in general the optic nerve in fiſhes makes a bend where it enters the eye, to admit of this motion without the nerve being ſtretched.

In thoſe fiſhes that have the ſclerotic coat completely covered with bone, the whole adjuſtment to great diſtances muſt be produced by the action of the choroide muſcle; but in the others, which are by far the greater number, this effect will be much aſſiſted by the action of the ſtrait muſcles pulling the eye-ball againſt the ſocket, and compreſſing the poſterior part, which, as it is the only membraneous part in many fiſhes, would appear to be formed ſo for that purpoſe.

In fiſhes, the eye in its natural eaſy ſtate appears to be adjuſted to near objects, requiring ſome change to adapt it to ſee diſtant ones; in this reſpect differing entirely from the bird, the quadruped, and the human.

The preceding obſervations, on the ſtructure of the eye, indicate two methods of adjuſting the eye; one for ſeeing in air, the other for ſeeing in water; and that the cryſtalline lens, as the moſt conſpicuous part, appears to have engroſſed the whole attention of former enquirers, who in general do not ſeem to have paid ſufficient attention to the elongation of the axis and change of curvature in the cornea. That the axis of viſion is really lengthened, and the lens moved forward, appears highly probable from the whole of the facts; and ſince the elongation of the eye, the change of poſition of the cryſtalline, or the alteration of curvature in the cornea, have ſeverally appeared to be the cauſe of the requiſite adjuſtment, the combination of all three muſt undoubtedly be ſufficient to effect the purpoſe.

In the Philoſophical Tranſactions for 1797, page 1, Mr. Home explains certain morbid affections of the ſtrait muſcles and cornea of the eye, and conſiders their treatment. He ſtates the uſes of the muſcles to be; firſt, that of moving the eye-balls in different directions; ſecondly, that of cauſing the two eyes to correſpond; and thirdly, that of producing adjuſtment by their lateral preſſure. Imperfection in any one of theſe different actions muſt be conſidered as a diſeaſe. Three different caſes occur in practice; namely, indiſtinct viſion, double viſion, and ſquinting.

Indiſtinct viſion is explained to conſiſt of an inability in the muſcles to ſupport that degree of tenſion which is requiſite to adjuſt the eye to near objects. In this morbid affection of the ſtraight muſcles they may be capable of performing all the intermediate contractions as uſual, but not the extreme degrees without conſiderable pain. As theſe ſymptoms have not been accounted for before in this way, the author adduces ſome curious inſtances of ſimilar affections of other muſcles, and concludes that bad effects muſt neceſſarily ariſe from every thing which irritates or weakens the parts themſelves, or the general habit; and that ſuch means ought to be adopted as may ſoothe the parts in their ſenſations, and quiet and ſtrengthen their actions, ſince in that way only the muſcular fibres can poſſibly recover their tone.

On the ſubject of double viſion the author remarks, that many opinions have been advanced to account for the ſingle appearance of objects contemplated by both eyes. The opinion of Dr. Reid of Glaſgow, that viſion is ſingle when the impreſſions are made on correſpondent parts of the retina in the two eyes, and double when this is not the caſe, appears to him to be ſtrongly confirmed by the facts. This want of correſpondence may be produced by ſome change in the refracting media of one of the eyes, or elſe by a want of ſimilar actions in the muſcles of both eyes reſpectively. The former takes place after the cryſtalline lens of one or both eyes has been extracted, and the convex lens made uſe of to produce the requiſite focal adjuſtment is not duly placed. The latter is more particularly the object of medical treatment. Mr. Home adviſes repoſe, that is to ſay, that the diſordered eye ſhould be covered for a time. For, as he remarks, the firſt object of attention, with regard to ſtrained or over-fatigued muſcles, muſt be to put them into an eaſy ſtate, and confine them from motion; and this practice is no leſs applicable to the muſcles of the eye than thoſe of other parts.

As double viſion is produced by a moderate derangement of the optic axes, ſquinting is effected by a much greater derangement. In the caſe of ſquinting, the author proves that the object is not ſeen by both eyes; but that one eye, more or leſs perfect, is directed to the object, while the other, which in ſuch caſes is imperfect, is drawn aſide by habit, in order that its operation may not diſturb the perception received by the other eye. The greater ſtrength, ſhortneſs and ſtraitneſs of the adductor muſcle cauſes the deviation to be made towards the noſe. Theſe doctrines are illuſtrated by appoſite caſes. Squinting takes place in three different circumſtances; that is to ſay, where one eye has only an indiſtinct viſion; where both eyes are capable of ſeeing objects, but the one is leſs perfect than the other; and where the muſcles of one eye have, from practice, as in the caſe of frequently looking through teleſcopes, acquired a power of moving it independently of the other.

When ſquinting ariſes from abſolute imperfection in the eye, there is no cure. Where it ariſes from weakneſs only in the ſight of one eye, it may in ſome inſtances be got the better of by confining the perſon to the uſe of one eye by covering the other; in this way the muſcles, from conſtant uſe, become perfect in the habit of directing the organ, and acquire ſtrength and power of adjuſtment. The time required muſt depend upon the weakneſs of the ſight, and length of time during which the muſcles have been left to themſelves.

The Author ſhews, that he cornea is not, like the cuticle, devoid of life; though, like tendons and ligaments, it is neither ſupplied with red blood nor poſſeſſes ſenſibility, but is made up of membraneous ligaments, which are continuations of the tunica conjunctiva and the tendons of the four ſtrait muſcles. When wounded, it commonly unites, like other living parts, by the firſt intention, and in ſome caſes with inflammation exceeding the limits of adheſion; and the whole internal cavity of the eye proceeds to a ſtate of ſuppuration. Theſe ſtages of inflammation are only met with in parts poſſeſſed of life.

From the opinion of the cornea being void of life, the opacities which are found to take place on it have been ſuppoſed to ariſe from inanimate matter laid over its ſurface. And under that notion acrid and irritating applications have been uſed to remove it, ſuch as powdered glaſs, powdered ſugar, &c. Such applications being of ſervice, have confirmed the opinion.

Dr. Home conſiders the cornea as a ligamentous part, and, as ſuch, weak in its vital powers. This ariſes from ſuch parts having no veſſels carrying red blood. When they inflame, which is a ſtate of increaſed action, they therefore require a different mode of treatment from the other parts of the body, whoſe vital powers are ſtrong in conſequence of being largely ſupplied with red blood.

The truly healthy inflammation requires an increaſed action in the parts affected; and if this, either from weakneſs or indolence, is not kept up, the inflammation does not go rapidly through its ſtages, but remains in a ſtate between reſolution and ſuppuration. In ligamentous ſtructures the actions muſt therefore be rouſed and ſupported, when under inflammation, to promote reſolution, and to prevent the parts from falling into an indolent diſeaſed ſtate. This is however attended with difficulty, and they too often become conſiderably thickened by a decompoſition of coagulating lymph during the adheſive ſtate of inflammation, which in the cornea renders it opake. The thickening of the parts remains after the inflammation is gone, and can only be removed by abſorption, which is beſt effected by the application of very ſtimulating medicines.

He extends theſe principles to all ligamentous ſtructures. Theſe require a peculiar treatment, which may be illuſtrated both in inflammations of the joints, and of the cornea of the eye; the applications made uſe of with the greateſt advantage in both caſes being of a very ſtimulating kind.

The inſtance of the cure of an opacity of the cornea in the eyes of Tobit by the gall of a fiſh, as related in the apocryphal book which bears his name; with other circumſtances of like practice among the Arabians; and alſo certain facts and obſervations which occurred in his own practice, are related as proofs of the advantage of ſtimulating applications to the cornea. In ſome old caſes of opacity, he found gall the beſt application. He uſed it pure, and alſo diluted. The gall of quadrupeds, in theſe trials, gave more pain than the gall of fiſh. The painful ſenſation was very ſevere for an hour or two, and then went off, and the beneficial effects appeared to be in proportion to the local violence at the time of its application. The object of his obſervations having been to explain the principle upon which thoſe effects depend, this knowledge may, as he obſerves, regulate the practice ſuitable to be adopted in ſuch caſes. It will ſhew the impropriety of uſing ſuch medicines while the inflammatory action is increaſing, and will point out their adoption the moment the inflammation appears to be at a ſtand, inſtead of poſtponing theſe remedies till an indolent unhealthy ſtate takes place, which too often terminates in opacities not to be afterwards removed by any application.

VIII.

Experiments and Obſervations on the Inflexion, Reflexion, and Colours of Light.

By HENRY BROUGHAM, *Jun. Eſq.*[*]

IT has always appeared wonderful to me, ſince nature ſeems to delight in thoſe cloſe analogies which enable her to preſerve ſimplicity, and even uniformity in variety, that there ſhould be no diſpoſitions in the parts of light with reſpect to inflexion and reflexion analogous or ſimilar to their different refrangibility. In order to aſcertain the exiſtence of ſuch properties, I began a courſe of Experiments and Obſervations, a ſhort account of which forms the ſubſtance of this paper. For the ſake of perſpicuity I ſhall begin with the analytical branch of the ſubject, comprehending my obſervations under two parts: flexion, or the bending of the rays in their paſſage by bodies, and reflexion. And I ſhall conclude by applying the principles there eſtabliſhed to the explanation of phenomena, in the way of ſyntheſis.

As in every experimental enquiry much depends on the attention paid to the minuteſt circumſtances, in juſtice to myſelf I ought to mention that each experiment was ſet down as particularly as poſſible, immediately after it was made; that they were all repeated every favourable day for nearly a year, and before various perſons; and as any thing like a preconceived opinion with reſpect to matter of theory that is in diſpute, will, it is more than probable, influence us in the manner of drawing our concluſions, and even in the manner of recording the experiments that lead to theſe, I have endeavoured, as much as poſſible, to keep in view the ſaying of the Brahmin, "that he who obſtinately adheres to "any ſet of opinions, may bring himſelf at laſt to believe that the freſh *ſandal-wood* is a flame of fire[†]."

PART I. *Of Flexion.*

IN order to fix our ideas on a ſubject which has never been treated of with mathematical preciſion, we ſhall ſuppoſe, for the preſent, that all the parts of light are equally acted upon in their paſſage by bodies, and deduce ſeveral of the moſt important propoſitions which occur, without mentioning the demonſtrations.

Def. 1. If a ray paſſes within a certain diſtance of any body, it is bent inwards; this we ſhall call inflexion. 2. If it paſſes at a ſtill greater diſtance, it is turned away; this may be termed deflexion. 3. The angle of inflexion is that which the inflected ray makes with the line drawn parallel to the edge of the inflecting body; and the angle of incidence is that made by the ray before inflexion, at the point where it meets the parallel. And ſo of the angle of deflexion.

Propoſition I. The force by which bodies inflect and deflect the rays, acts in lines perpendicular to their ſurfaces.

Prop. II. The ſines of inflexion and deflexion are each of them to the ſine of incidence in a given ratio—(and what this ratio is, we ſhall afterwards ſhew).

Prop. III. The bending force is to the propelling force of light as the ſine of the difference between the angles of inflexion (or deflexion) and incidence to the coſine of the angle of inflexion or deflexion).

---

[*] Philoſophical Tranſactions, M.DCCXCVI. 1.

[†] Aſiatic Reſearches, vol. i. p. 224.

Prop. IV. The rays of light may be made to revolve round a centre in a ſpiral orbit.

Prop. V. If the inflecting ſurface be of conſiderable extent, and a plane, then the curve deſcribed may be found by help of the Prop. XLI. Book I. of Principia; provided only the proportion of the force to the diſtance be given. Thus, if the bending force be inverſely as the diſtance, the curve cannot be found; for, in order to obtain its equation, a curvilinear area muſt be ſquared, which, in this caſe, is a conic hyperbola; the relation, however, between its ordinates and abſciſſæ may be obtained in fluxions, thus: $y\dot{y} + b\dot{y} = a^2\dot{x}^2$.

If the force (which is moſt probable) be inverſely as the ſquare of the diſtance, the curve to be ſquared is the cubic hyperbola; ſpecies LXV. genus III. of Newton's Enumeration; and this being quadrable, the curve deſcribed by the light will be the *parabola campaniformis pura*; Species LXIX. of Newton.

If the force be inverſely as the cube of the diſtance, the curve is a circular arch, and that of deflexion is a conic hyperbola *. If the inflecting body be a globe or cylinder, and the force be inverſely as the ſquare of the diſtance from the ſurface; then, by Prop. LXXI. Book I. of Principia, the attraction to the centre is inverſely as the ſquare of the diſtance from that centre; and therefore, by Prop. XI. and XIII. of the ſame Book, the ray moves in an ellipſe by the inflecting, and in an hyperbola by the deflecting force; each having one focus in the centre of the body. The truth of theſe things mathematicians will eaſily determine.

Prop. VI. If a ray fall on a ſpecular ſurface, it will be bent before incidence into a curve having two points of contrary flexure, and then will be bent back the contrary way into an equal and ſimilar curve, as in Fig. 1. Pl. XXIII.

Corollary to theſe propoſitions. If a pencil of rays fall converging on an interpoſed body, the ſhadow will be leſs than the body by twice the ſine of inflexion.

And if a pencil fall diverging on the body, the ſhadow will be greater than the body by twice the ſine of inflexion; but leſs than it ſhould be, if the rays had paſſed without bending, by twice the ſine of the difference between the angles of inflection and incidence. The ſine or angle of incidence is greater than the ſine or angle of inflexion, when the incident rays make an acute angle with the body; but, when they make an obtuſe or right angle, then the ſine or angle of inflexion is leſs than that of incidence. The ſine of incidence is greater than that of deflexion, if the angle made by the incident ray with the body is obtuſe; but leſs, if that angle be acute or right. If a globe or circle be held in a beam of light, the rays may be made to converge to a focus.

Hitherto it has been ſuppoſed that the parts of which light conſiſts have all the ſame diſpoſition to be acted upon by bodies which inflect and deflect them; but we ſhall now ſee that this is by no means the caſe.

Obſervation I. Into my darkened chamber I let a beam of the ſun's light through a hole in a metal plate (fixed in the window-ſhut) of $\frac{1}{40}$ of an inch diameter; and all other light being abſorbed by black cloth hung before the window and in the room, at the hole I placed a priſm of glaſs, whoſe refracting angle was 45 degrees, and which was covered all over with black paper, except a ſmall part on each ſide, which was free from impurities, and through which the light was refracted, ſo as to form a diſtinct and tolerably homogeneous ſpectrum on a chart at ſix feet from the window. In the rays, at two feet from the priſm, I placed a black unpoliſhed pin (whoſe diameter was everywhere one tenth of an inch) parallel to the chart and in a vertical poſition. Its ſhadow was formed in the ſpectrum on the chart, and had a conſiderable penumbra, eſpecially in the brighteſt red, for it was by no means of the ſame thickneſs in all its parts; that in violet was broadeſt and moſt diſtinct; that in the red narroweſt and moſt confuſed; and that in the intermediate colours was of an intermediate thickneſs and degree of diſtinctneſs. It was not bounded by ſtraight but by curvilinear ſides, convex towards the axis, to which they approached as to an aſymptote, and that neareſt in the leaſt refrangible rays, as is repreſented in Fig. 2, where A B is the axis, I K L M N A, and H G F E D A the two outlines. Nor could this be owing to any irregularity in the pin, for the ſame thing happened in all ſorts of bodies that were uſed; and alſo if the priſm was moved on its axis, ſo that the colours might aſcend and deſcend on theſe bodies; ſtill, wherever the red fell, it made the leaſt, and the violet the greateſt ſhadow.

---

* Principia, Lib. i. Prop. 8.

Obſ. II. In the place of the pin I fixed a ſcreen, having in it a large hole, on which was a braſs plate pierced with a ſmall hole $\frac{1}{42}$d of an inch in diameter. Then cauſing an aſſiſtant to move the priſm ſlowly on its axis, I obſerved the round image made by the different rays paſſing through the hole to the chart; that made by the red was greateſt, by the violet leaſt, and by the intermediate rays, of an intermediate ſize. Alſo, when at the back of the hole, I held a ſharp blade of a knife, ſo as to produce the fringes mentioned by Grimaldo and Newton; thoſe fringes in the red were broadeſt and moſt moved inwards towards the ſhadow, and moſt dilated when the knife was moved over the hole, and the hole itſelf on the chart was more dilated during the motion when illuminated by the red, than when illuminated by any other of the rays, and leaſt of all when illuminated by the violet. Now in Obſervation I. the angle of incidence of the red rays was equal to that of the violet and all the reſt, and yet the angle of inflexion was greateſt and leaſt in the violet; and indeed the difference between the two was greater than appears at firſt from the experiment; for that part of the ſhadow which was formed by the violet, fell at a greater diſtance from the point of incidence than did that part which was formed by the red, from the divergency of the different rays upwards by the refraction, as appears in Fig. 3, where D E is the window, F G the beam propagated through the hole F, refracted by the priſm K I H and pointing on the chart O P q s; the ſpectrum v r, being ſeparated in L r, the red rays incident on the pin C D at C; and M v, the violet incident at D; the ſhadow of D C being formed in v r, ſo that v being farther from D, than r is from C, therefore (by the propoſitions formerly laid down) the ſhadow in v ſhould be conſiderably leſs than in r, if the rays were equally inflected. Laſtly, in Obſ. II. the angle of the red's incidence was nearly equal to that of the violet's, by the motion of the priſm, and the conſequent motion of the colours; only that if there was any difference it was on the ſide of the violet: and yet the violet was leaſt inflected, and the red moſt inflected; and ſo of the ſecond inflexion by the knife-blade: wherefore I conclude that the rays of the ſun's light differ in degree of inflexibility, and that thoſe which are leaſt refrangible are moſt inflexible.

Obſ. III. My room being darkened as before, and a conical beam propagated through the ſmall hole in the window ſhut; at this hole I placed a hollow priſm made of broken plates of mirror, and of ſuch an angle that, when filled with diſtilled water, it caſt a ſpectrum on an horizontal table, and was there received on a chart ſeven feet from the window. I then placed on the ſame table, and in the rays between the chart and the priſm, at three inches from the chart, two ſharp knife-blades, with even edges, and fixed to a board with wax, ſo as to make an angle with one another ; moving them nearer and nearer, till I ſaw the fringes appear in the red light on the chart, and then in the orange and other colours ſucceſſively. I then withdrew one, and the fringes became faint and narrow, and not all within the ſhadow of the remaining knife, but at its edge, and even in the light of the ſpectrum. Laſtly, when I ſlowly approached the other, they moved into the ſhadow, and became broader and farther ſeparated one from another ; there being the like fringes in both ſhadows. This I repeated in all the rays, and plainly ſaw, that at the approach of the knife the fringes became broader and farther removed from one another, and from the light, in the red than in the violet, or any of the other rays.

Obſ. IV. In repeating the foregoing Experiment, I obſerved a very curious phenomenon. When the angle of the knife-blades was ſo held in any of the rays, as to make the hyperbolic fringes deſcribed by Newton[*], and theſe being always of the colour in which they were held ; moving the angle a little, ſo as to make the fringes out of the light that went to the top of any one diviſion of the ſpectrum, and alſo out of that which went near the bottom of the next, the fringes were made of two colours ; one part was of the higheſt colour, and the other of the loweſt ; but both were on the ground of the higheſt. Thus, if held on the confine of the green and blue, the upper half of each fringe was blue, the under green, but both parts in the blue diviſion of the ſpectrum ; and trying the ſame in all the rays, it was evident that the red was moved farther into the orange, and the orange into the yellow, than the blue was into the indigo, or the indigo into the violet. Now in Obſ. III. the fringes were formed by the inflexion of one knife, and were moved into its ſhadow, and ſeparated and dilated by the deflexion of the other ; and this moſt in the red, and leaſt in the violet. Likewiſe in Obſ. IV. the fringes of one colour were deflected into the region of the next, and this moſt in the red, and leaſt in the violet ; although in both obſervations the violet, from the poſition of the chart, was fartheſt from the angle, and conſequently, had the rays been equally deflected, the violet ſhould have been fartheſt moved, and moſt dilated by the deflexion : but, ſeeing that, at equal angles of incidence in the third, and at leſs in the fouth Obſervation, the red was moſt, and the violet leſs, deflected, it is evident that the moſt inflexible rays are alſo moſt deflexible.

Having thus found that the parts of light differ in flexibility, I wiſhed next to learn two things ; in what proportion the angle of inflexion is to that of deflexion at equal incidences ; and ſecondly, what proportion the different flexibilities of the different rays bear to one another. But the nature of the coloured fringes muſt firſt be underſtood ; ſo that I defer this enquiry till after I have made uſe of the principles now laid down for the explanation of natural phenomena, and proceed in the mean time to

PART II.—*Of Reflexion.*

THAT bodies reflect light by a repulſive power extending to ſome diſtance from their ſurfaces, has never been denied ſince the time of Sir Iſaac Newton[†]. Now this power extends to a diſtance much greater than that of apparent contact, at which an attraction again begins ſtill at a diſtance, though leſs than that at which before there was a repulſion; as will appear by the following demonſtration, which occurs to me, and which is general with repect to the theory of Boſcovich*. In Fig. 4, let the body A have for P an attraction which at the diſtance of AP is proportional to PM; then let P move towards A, ſo as to come to the ſituation P', and let the attraction here be $P'M'$; as it is continual during the motion of P to P', MM' is a curve line. Now in the caſe of the attraction of bodies for light, and for one another, PM is leſs than $P'M'$; and conſequently MM' does not ever return into itſelf, and therefore it muſt go *ad infinitum*, having its arc between AB and AC, to which it approaches as aſymptotes, the abſciſſa always repreſenting the diſtance, and the ordinate the attraction at that diſtance. Let P' now continue its motion to P'', and M' will move M''; and if P'' meets A, or the bodies come into perfect contact, $P''M''$ will be infinite; ſo that the attraction being changed into coheſion will be infinite, and the bodies inſeparable, contrary to univerſal experience; ſo that P can never come nearer to A than a given diſtance. In the caſe of gravity PM is inverſely as the ſquare of AP, ſo that the curve NMM''' is the cubic hyperbola; but the demonſtration holds, whatever be the proportion of the force to the diſtance. It appears then, that flexion, refraction, and reflexion, are performed by a force acting at a definite diſtance; and it is reaſonable to think even *a priori*, that, as this ſame force in other circumſtances is exerted to a different degree on the different parts of light in refracting, inflecting, and deflecting them, it ſhould alſo be exerciſed with the like variations in reflecting them. Let us attend to the proof which enables us to change conjecture into conviction.

---

[*] Optics, Book iii. Obſ. 8.

[†] Ibid. Book iii. Part III. Prop. 8.

Obſ. I. The ſun ſhining into my darkened chamber through a ſmall hole $\frac{1}{40}$th of an inch in diameter, I placed a pin of $\frac{1}{30}$th of an inch diameter in the cone of light (one half inch from the hole) inclined to the rays at an angle of about 45°; and its ſhadow was received on a chart parallel to it, at the diſtance of two feet. The ſhadow was ſurrounded by the three fringes on each ſide, diſcovered by Grimaldo; beyond theſe there were two ſtreaks of white light diverging from the ſhadow, and mottled with bright colours, very irregularly ſcattered up and down; but on uſing another pin, whoſe ſurface was well poliſhed, and placing it nearer the hole than before, the colours in the ſtreaks became much brighter (and the ſtreaks themſelves narrower), being extended from one ſide to the other, ſo that, except in a very few points here and there, no white was now to be ſeen; and, on moving the pin, the colours moved alſo. But they diſappeared if the pin was deprived of its poliſh by being held in the flame of a candle, or if a roll of paper was uſed inſtead of the pin; alſo they were much brighter in direct than in reflected light, and in the light of the ſun at the focus of a lens, than in his direct unrefracted light. Placing a piece of paper round the hole in the window-ſhut, I obſerved the colours continued there; and inclining the chart to the point where they left off, I ſaw them continued on it and then proceed as before to the ſhadow. If the pin was held horizontally, or nearly ſo, they were ſeen of a great ſize on the floor the walls and roof of the room, forming a large circle; and if the chart was laid horizontally, and the pin held between the hole and it, in a vertical poſition, the circle was ſeen on the chart, and became an oval by inclining the pin a little to the horizon.

---

* Nova Theoria Philoſophiæ Naturalis.

Obſ. II. Having produced a clear ſet of colours, as in the laſt Obſervation, I viewed them as attentively as poſſible, and found that they were divided into ſets, ſometimes ſeparated by a gleam of white light, ſometimes by a line of ſhadow, and ſometimes contiguous, or even running a little into one another. They were ſpectra or images of the ſun, for they varied with the luminous body by whoſe rays they were formed, and with the ſize of the beam in which the pin was held; and when by placing it between my eye and the candle a little to one ſide I let the colours fall on my retina, I plainly ſaw that they reſembled the candle in ſhape and ſize (though a little diſtended), and alſo in motion, ſince, if the flame was blown upon, they had the like agitation, The colours, therefore, which fell on the chart were images of the ſun: they had parallel ſides pretty diſtinctly defined, but the ends were confuſed and ſemicircular, like thoſe of the priſmatic ſpectrum. Like it, too, they were oblong, and in ſome the length exceeded the breadth ſix, even eight, times. The breadth was, as I found by meaſurement, exactly equal to that of the ſun's image received on a chart as far from the pin as the image was, and the length was always to the breadth at all diſtances in the ſame ratio, but not in all poſitions of the pin; for, if it was moved on its axis, the images moved towards the ſhadow on one ſide, and from it on the other, becoming longer and longer (the breadth remaining the ſame) the nearer they came to the ſhadow on the one ſide, and ſhorter in the ſame proportion the farther they went from it on the other.

Obſ. III. Having picked out an image that appeared very bright and well defined, I let it through a hole with moveable ſides in the upper part of a ſort of deſk, which moved to any opening by hinges, and had a chart for its under ſide, on which the image fell, and I ſhut the hole ſo cloſe as to prevent any of the others from coming through. I then had a full opportunity of examining it in all reſpects, and I counted in it diſtinctly the ſeven priſmatic colours; the red was fartheſt from the ſhadow of the pin, and from the pin itſelf; then the orange; then the yellow, green, blue, and indigo; and the violet neareſt of all: in ſhort, it was exactly ſimilar to a priſmatic ſpectrum much diminiſhed in length and breadth, and turned horizontally on the wall oppoſite to the priſm, with the red fartheſt away. In figure 5, *s e* is the pin reflecting the rays C P and C O, which paſs through P O the hole in the deſk E D, to the chart or bottom of the deſk R T S D; and from there the ſpectrum I K divided into its colours, I being violet, and K red. On moving the hole in the deſk, and letting through other images, the colours were not in all arranged the ſame way: but I moved the pin on its axis, and obſerved thoſe where the order was inverted, to move, not only with reſpect to the pin, but alſo with reſpect to the contiguous images; and I was ſurpriſed to ſee them aſſume the order of colours firſt mentioned, namely the red outermoſt, and the violet innermoſt. In like manner, the images which before the motion were regular, on moving into the places left by the others had always the order of their colours inverted, ſo that the thing muſt be owing to ſome irregularities in the pin's ſurface; for thoſe which were made by a ſmall glaſs tube filled with quickſilver, and freed from ſcratches by a blow-pipe, preſerved during the motion the proper order of colours. Another irregularity in the arrangement was alſo obſervable even in the glaſs tube; for two contiguous images, by mixing one with another for two or three ſucceſſions, appeared each to have outermoſt a dull colour between red and violet, and innermoſt a green: but here unleſs the ſucceſſion continued through all the images, the outermoſt of all was red, and the innermoſt image had univerſally violet in the inſide.

Obſ. IV I placed at a hole in the window-ſhut a priſm to refract the rays, and received the ſpectrum at the diſtance of ſix feet from the window on a chart; then at the diſtance of two feet I placed a ſcreen with a hole in the middle of it, through which I let paſs ſucceſſively the different rays. At the diſtance of one inch from the hole, between it and the chart, I placed the reflecting cylindrical body; the images were found on the chart and walls of the room round to the ſides of the hole on the ſcreen, and were always wholly of the colour in which they were formed, except in the confines of the green, where a ſmall quantity of white light fell, and made them of all the ſeven colours; but this was almoſt wholly prevented by uſing a priſm with a greater refracting angle, and holding the pin and ſcreen farther from it. I then removed the ſcreen, and left the reflector in its place, ſo as it might reach through the rays; and thus there were formed images having in them, from top to bottom, the ſeven colours, one after another, the loweſt diviſion being red, the higheſt violet. They were inclined conſiderably towards their tops, and were much broader at the bottom or red parts than at the tops or violet parts. And laſtly, the reflector being moved ſo that the images might be diſturbed (as in the former experiment made in the white light), the red was moſt, the violet leaſt dilated. In caſe theſe effects might be owing to any peculiarities in the ſhape or poſition of the reflector, I placed at three feet from the priſm a lens of four inches breadth, to collect the rays to a focus, ſix feet beyond which I held a chart, and there received the ſpectrum inverted, the red being uppermoſt, and the violet undermoſt; holding the reflector at two feet from the focus and four from the chart, the images were formed juſt as before, only inverted, inclining towards the violet, of greater breadth towards the red, and more diſtended towards the ſame quarter when the reflector was moved.

Obſ. V. Things remaining as in the laſt part of the laſt Experiment; at the focus of the lens I placed a ſecond priſm, which refracted the rays into a white beam *, and this I received on a ſcreen with a hole in the middle, through which a ſmall part of it paſſed, and falling on the reflector placed behind was formed by it into images after the manner of the firſt Experiment, each having in regular order the ſeven priſmatic colours. One of the brighteſt and moſt diſtinct I let paſs through a hole in the ſecond ſcreen, and it fell on the chart. I then cauſed an aſſiſtant to intercept the red rays between the firſt priſm and the lens, and immediately the red part of the image vaniſhed; and when the violet was intercepted, the violet of the image vaniſhed; and if the green was intercepted, the green was wanting in the image. In ſhort, whatever colours were ſtopped, the ſame were miſſing in the image. In Fig. 6. the rays paſſing through the hole C of the window, A B are refracted by the priſm P M N, and ſeparated into D V, D G, and D R, violet, green and red; which, being collected into a focus F by the lens L, are there again refracted by a priſm P′ M′N′, and formed into a white beam a b m n, part of which is intercepted by the ſcreen S S′, and part paſſes through the hole h, as h H to H on the chart X Y Z W, and part is reflected by the body *a q* into a ſet of images which are received on a ſcreen T U, and one of them, r g v, let paſs to W X Y Z; but when an obſtacle E ſtops D R, r the red vaniſhes; and if D G be ſtopped, g the green vaniſhes; and if D V be ſtopped, v diſappears. Laſtly, if D R and D G be ſtopped, g and r vaniſh.

---

* Optics, Book ii. Part II. Prop. 2.

Obſ. VI. Having produced a ſet of bright images, I let one paſs through the deſk deſcribed in the third Experiment, and received it on a ſmall lens $\frac{1}{2}$ inch broad to collect the rays into a focus, which I received on the chart by moving it a little on its hinge; and by all the obſervations I could make, and all the teſts I could think of, it was white inclining to yellow, and of the ſame nature and conſtitution with the ſun's direct light: but if any ray was ſtopped before coming to the lens, the focus was a mixture of the remaining rays; and the chart being moved a little farther round, the image was formed on it, the colours being in an inverted order. At the focus I held a reflector, and there were formed images of all the ſeven colours, as in the ſun's direct light (Exp. 1.); if the light was ſufficiently ſtrong, and the deſk near the window-ſhut hole, one of theſe could even be collected by a ſecond lens into a white focus. This experiment is rendered more uniform by ſubſtituting for the lens a concave metallic mirror, and placing at the focus another mirror to reduce the rays into a beam which may be made of any compoſition we pleaſe by ſtopping one or more of the colours at the hole in the deſk. I obſerved in the courſe of theſe experiments a phenomenon worth mentioning; if a comb (as in Newton's experiment*) be very ſwiftly moved before one of the images or more, a ſenſation of white is produced: but this is ſtill more evident if the pin be ſwiftly moved round its axis; for then the images move alſo, and, running into one another, cauſe a ſenſation of perfect whiteneſs.

Obſ. VII. I let an image through the hole in the deſk, and viewed it through a glaſs priſm, holding its axis parallel to the ſides of the image, and its refracting angle upwards. I found that if the image was bright, and free from white light, the colours were not changed by the refraction; but if it was mixed and diluted with white, the priſm, decompounding the white, cauſed the image to appear violet at one ſide, and red at the other; yet ſtill this only confuſed the colours of the image without changing them. Farther, if the priſm was moved on its axis, the violet was lifted higher than the red or any of the other colours. Nor was the conſtitution of the colours at all changed by reflexion from a pin or mirror, except in ſo far as they were mixed by a concave one, as mentioned in the laſt experiment. If a pin was held behind the hole to reflect the colours, it formed other images of the colour in which it was held, and as far as I could judge threw the red to the greateſt diſtance and breadth and inclination. Nor were the colours of the image changed by reflexion from natural bodies, for theſe were all of the colours in which they were held, but brighteſt in that which they were diſpoſed to reflect moſt copiouſly. Likewiſe the rings of colour made by thin plates were broadeſt in the red, and narroweſt in the violet; and the like happened to the fringes that ſurround the ſhadows of bodies. Laſtly, the ſhadows of bodies were themſelves broadeſt in the violet, and narroweſt in the red.

Obſ. VIII. I filled with water a glaſs tube, whoſe diameter was $\frac{1}{4}$th of an inch, and conſequently the radius of curvature $\frac{1}{8}$th, and whoſe ſides were $\frac{1}{10}$th of an inch thick; then ſtanding at four feet from a candle I held the tube $\frac{1}{4}$th of an inch from my eye, ſo that the light of the candle might be refracted through it, and moved my eye-lids cloſe enough to prevent the extraneous ſcattered light from entering along with that which was regularly refracted. I ſaw ſeveral images of the candle, all highly coloured, and the colours were, in order from the candle outwards, red, orange, and ſo on to violet. I then filled the tube with clear diluted ſulphuric acid, and dropped a ſmall piece of chalk to the bottom, when immediately an efferveſcence took place by the eſcape of fixed air, which roſe in bubbles through the tube; and looking at the candle through one of theſe, I ſaw the images formed with the colours ſtill in the ſame order, but a little larger than before.

---

* Optics, Book i. Part II. Prop. 5.

We are now to ſee to what concluſions theſe experiments lead us. The firſt experiment ſhews, that all ſorts of lights, whether direct or reflected or refracted, produce colours by reflexion from a curve ſurface. From the ſecond we learn that theſe colours are diſtinct images or ſpectra of the luminous body, much dilated in length, but not at all in breadth, and that the angle of incidence being changed, the dilatation of the images is alſo changed; and from the third experiment it appears, that each full image is compoſed of ſeven colours, red, orange, yellow, green, blue, indigo, and violet; and that the proper order is red outermoſt, and violet innermoſt, the reſt being in their order. The fourth experiment ſhews, that theſe images are produced, not by any accidental or new modification impreſſed on the rays; but by the white light being decompoſed by reflexion; that the mean rays, or thoſe at the confine of the green and blue, are reflected at an angle equal to that of incidence, and the red at a leſs, the violet at a greater angle. Experiments 5th and 6th prove, beyond a doubt, the decompoſition and ſeparation of the rays by reflexion; for in both we ſee that the colours in the images are thoſe, and thoſe only, which were mixed in the ray by reflexion or refraction before and at incidence; whilſt the 6th is (in addition) a proof that all the rays of any one image, if mixed together, compound a beam exactly ſimilar to the beam that was at firſt decompounded. The ſeventh experiment ſhews, that the colours into which the rays are ſeparated by reflexion are homogeneous and unchangeable; that they differ in flexibility and refrangibility; that they bear the ſame part in forming images by reflexion, and fringes by flexion, and colours from thin plates, which the rays ſeparated by the priſm do. And in the 8th experiment we ſee, that when the rays are placed in the ſame ſituation with reſpect to refraction, whether out of a rarer into a denſer, or a denſer into a rarer medium, in which they before were with reſpect to reflexion, the poſition of the colours produced is diametrically oppoſite in the two caſes. Seeing, then, that in all ſorts of light, direct, refracted, reflected, ſimple and homogeneous or heterogeneous, and compounded, and in whatever way the ſeparation and mixture may have been made, ſome of the rays, at equal or the ſame incidences, are conſtantly reflected nearer the perpendicular than the mean rays, and others not ſo near; and ſeeing that by ſuch reflexion the compound ray of whatever kind is ſeparated into parts ſo ſimple that they can never more be changed; and conſidering the different places to which theſe parts are reflected; it is evident, that the ſun's light conſiſts of parts different in reflexibility, and that thoſe which are leaſt refrangible are moſt reflexible. By reflexibility I here mean a diſpoſition to be reflected near to the perpendicular in any degree.

Although I have given what I take to be ſufficient proof of this property of light, yet I am aware that ſomething more is requiſite. It will be aſked, why does neither a plane, a common convex, nor a common concave mirror ſeparate the rays by reflexion? This is what has always hindered us from even ſuſpecting ſuch a thing as different reflexibility. I ſhall, however, take an opportunity of removing this obſtacle in the ſecond part of the plan, when I come to explain the reaſon of the colours made by the reflecting body, and the manner of their formation. At preſent I ſhall only caution thoſe who may wiſh to repeat the above experiments, that the hole in the window-ſhut muſt be ſmall, the room quite dark, the pin well poliſhed, and the deſk, chart, &c. placed at a diſtance from the pin not greater than three feet, otherwiſe the images will be dilute and dim; nor, on the other hand, leſs than ſix inches, otherwiſe they will be too ſhort, and the colours not far enough ſeparated one from another.

My next object of enquiry was the different degrees of reflexibility belonging to each ray. It appears, not only from mathematical conſiderations ſufficiently obvious, but alſo from the experiments I have related, that though the different rays have, at the ſame or equal incidences, different angles of reflexion, yet each ray is conſtant to itſelf in degree of reflexibility, and that its ſine of reflexion bears always the ſame ratio to its ſine of incidence. The queſtion then is, what are the ſines of reflexion of the different rays, the ſine of incidence being the ſame to all?

Obſ. IX. In ſummer, at noon, when the ſun's light was exceedingly ſtrong, and there was not the veſtige of a cloud in the ſky, I produced an uncommonly fine ſet of images by fixing, at an inch from the ſmall hole $\frac{1}{50}$th of an inch diameter, a pin $\frac{1}{25}$th of an inch diameter. One of the brighteſt of theſe I let paſs through the deſk to the chart below at $2\frac{1}{2}$ feet from the pin, and the image was three inches from the ſhadow, in a ſtraight line. I delineated it carefully by drawing two parallel lines for the ſides, and marking the ſemicircular ends. Then with the point of a ſmall needle I marked the confines of the contiguous colours on one of the parallel ſides, and afterwards drew acroſs the image parallel lines. This operation I repeated with the ſame and different images at many diſtances from the pin, and on different days, with various ſorts of pins and ſizes of holes, &c. &c. and all theſe repetitions were made before I once examined the reſult of any one meaſurement, that I might be unprejudiced in trying the thing over again. I then compared the ſketches of divided images which I thus obtained, and found ſufficient reaſon to conclude, that the differences between the ſines of reflexion in the different rays were in the harmonical order. For the diviſions were nearly as $\frac{1}{9}$, $\frac{1}{18}$, $\frac{1}{12}$, $\frac{1}{12}$, $\frac{1}{15}$, $\frac{3}{80}$, $\frac{1}{16}$, which, when compounded with the ſcale, gave 1, $\frac{15}{16}$, $\frac{9}{10}$, $\frac{5}{6}$, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{11}{18}$, $\frac{1}{2}$; and theſe are exactly the change of the notes in an octave, obtained by taking the ſums of the octave, and a ſecond major, a third major, a fourth, a fifth, a ſixth major, a ſeventh major, and an eighth, inſtead of the difference between a double octave and a ſecond major, a third major, and ſo on. Thus the ſpectrum by reflexion is divided exactly as the ſpectrum by refraction, only that the former is inverted, and the different rays have reflexibilities, that are inverſely as their refrangibilities. Having ſettled this (I flatter myſelf) curious and important point, I proceeded next to enquire into the abſolute reflexibility of the extreme colours; for if this be known, the angle of incidence being given, the angle of reflexion of all the different rays may be found. For obtaining a ſolution of this problem I made the following experiment:

Obſ. X. The ſun ſhining ſtrongly through the ſmall hole in the window-ſhut, and the rays diverging into a cone whoſe baſe fell on an horizontal chart $2\frac{1}{2}$ feet from the hole, between the hole and chart I placed a ſcreen which had a plate and ſmall hole in it. The rays paſſing through this fell on a ſmall pin, ſo placed that the images formed might

be

be at right angles to the ſhadow; one of theſe I meaſured together with its diſtance from the ſhadow, the diſtance of the ſhadow from the hole, the breadth of the ſhadow, and the diameter of the pin: theſe meaſures were as follows. In fig. 7, C is the centre, and B *e n* the circumference of the pin, G M the chart, and G D a line in it, being the axis of all the images, at right angles to C D, the diſtance of C from D the centre of the ſhadow, and alſo to the ſhadow itſelf; G E is the parallel ſide of the image, G being red, E violet, and F the confine of the green and blue. C*e* is a radius parallel to E D, and C A another drawn through B, the point where O B is incident at the angle O B A, to which (by what was before ſhown) A B F is equal. By meaſurement G E is $\frac{1}{4}$th of an inch, C B $\frac{1}{80}$th, C D $4\frac{1}{2}$; now the ſhadow being leſſened by a penumbra, this added to half the ſhadow, and their ſum to the diſtance between the penumbra and the violet, gave E D $\frac{417}{400}$ths of an inch. From whence it is eaſy to calculate, that the angle of incidence being 77° 20′, the angle of the red's reflection A B G is 75° 50′ and that of the violets 78° 51′. Now the natural ſines of 77° 20′, 75° 50′, and 78° 51′, are as 9756, 9695, and 9811; or as 250, 248, and 251, which are very nearly as $77\frac{1}{2}$, 77, and 78; and making an allowance for the omiſſions made in the reductions, the errors in the operations and meaſurements, they may be accounted as accurately in the above proportion. Now their extremes 77 and 78 are the very proportions of the red's refrangibility to the violet's*. So that the reflexibility of the red is to that of the violets as the refrangibilities inverſely. But it is obvious that the ſine of incidence is not the ſame in the two caſes; for in the one it is equal to that of the mean rays reflection; while in the other none of the rays are refracted at an angle equal to that of incidence, otherwiſe they would not be refracted at all. This, however, being a conſequence of the eſſential diſtinction in the circumſtances, does not impair the beautiful analogy which we have ſeen is preſerved in the two operations, and which proves them to be different exertions of the ſame power. Now we may find, from the data obtained, the ſines of all the rays in the ſpectrum by adding to 77, the lengths of the ſpaces into which it is divided, and which are, without any ſenſible error, as the differences of thoſe ſines. The ſines of the red will be from 77 to $77\frac{1}{8}$; the orange from $77\frac{1}{8}$ to $77\frac{1}{5}$; the yellow from $77\frac{1}{5}$ to $77\frac{1}{3}$; the green from $77\frac{1}{3}$ to $77\frac{1}{2}$; the blue from $77\frac{1}{2}$ to $77\frac{2}{3}$; the indigo from $77\frac{2}{3}$ to $77\frac{7}{9}$; the violet from $77\frac{7}{9}$ to 78. So that, the ſine of incidence being given, that of the reflexion of all the different rays may be found; and the angle of incidence being 50° 48′, the angles of reflexion are as follows: Of the extreme red 50° 21′; of the orange 50° 27′; of the yellow 50° 32′; of the green 50° 39′; of the blue 50° 48′; of the indigo 50° 57′; of the violet 51° 3′; and of the extreme violet 51° 15′.

I ſhall conclude this part of the ſubject with a few remarks on the phyſical cauſe of reflexibility. As light is reflected by a power extending to ſome diſtance from the reflecting ſurface, the different reflexibility of its parts ariſes from a conſtitutional diſpoſition of theſe to be acted upon differently by the power. And as theſe parts are of different ſizes, thoſe which are largeſt will be acted upon moſt ſtrongly. I ſhall not heſitate to go a ſtep farther. In fig. 8, let EC be the reflecting ſurface, DH the perpendicular, and AB a ray incident at B, and produced to F, and reflected into GB; draw GH parallel to FB, and GF to HB; then HB : (HG :) : BF : : ſin. HGB : ſin. HBG or : : ſin. GBF : ſin. HBG. But GBF is the ſupplement of GBA, the ſum of the angles of reflexion and incidence; wherefore HB : BF :: the ſine of the ſum of the angles of reflexion and incidence, to the ſine of the angle of reflexion; ſo that if I be the angle of incidence, R that of reflexion, V the velocity of light, and F the reflecting force; $F = \frac{V \times \text{ſin.}\,(R + I)}{\text{ſin.}\,R.}$. By accommodating this formula to the different caſes, we obtain F in all the rays; and the ratio of F in one ſet to F in another being required, we have (by ſtriking out V, which is conſtant) $F : F' :: \frac{\text{ſin.}\,(R + I)}{\text{ſin.}\,R} : \frac{\text{ſin.}\,(R' + I')}{\text{ſin.}\,R'}$. Suppoſe we would know F and F′ in the red and violet reſpectively; $I = 50^\circ\,48' - R = 50^\circ\,21'$, and $R' = 51^\circ\,15'$; then $F : F' :: \frac{\text{ſin.}\,101^\circ\,9'}{\text{ſin.}\,50^\circ\,21'} : \frac{\text{ſin.}\,102^\circ\,3'}{\text{ſin.}\,51^\circ\,15'}$ Performing the diviſion in each by logarithms, and finding the natural ſines correſponding to the quotients; $F : F' :: 1275 : 1253$. But the force exerted on the red is to that exerted on the violet as the ſize of the red to the ſize of the violet (by hypotheſis); therefore the red particles are to the violet as 1275 to 1253. This may be extended to all the other colours by ſimilar calculations;, their ſizes lying between 1275 and 1253, which are the extreme red and extreme violet; thus the red will be from 1275 to 1272½; the orange from 1272½ to 1270; the yellow from 1270 to 1267; the green from 1267 to 1264; the blue from 1264 to 1260; the indigo from 1260 to 1258; and the violet from 1258 to 1253.

---

* Optics, Book i. Part I. Prop. 7.

All this follows mathematically, on the ſuppoſition that the parts of light are acted upon in proportion to their ſizes: and to ſay the truth, I ſee no other proportion in which we can reaſonably ſuppoſe them to be influenced; for ſuch an action is not only conformable to the univerſal laws of attraction and repulſion, but alſo to the following arguments. If the action be not in the ſimple ratio, it muſt either be in a lower or in a higher. Let it be in a lower, as that of the ſquare root, then the ſize of the red would be to the ſize of the violet as the ſquares of the forces; that is, as 1625625 to 1572009: a difference evidently too great; and *à fortiori* of the cube or any other root. On the other hand, if the action were in a higher ratio, as that of the ſquare, then the particles would be as the ſquare roots of the forces, or nearly as 35.70 to 35.39, a difference evidently too ſmall; for if the ſize of the red particles were only $\frac{3}{10}$ths the greater than that of the violet, and the velocity of both were equal, the momentum and conſequently the intenſity of the red could not ſo much exceed that of the violet as we find it does; and as ſeems to me to be proved by the experiment of Buffon (on accidental colours), who found, that after looking at a white object, when he ſhut his eyes, it firſt became violet, then blue or a mixture of blue and the other colours, and laſt of all red: ſo in the impreſſion of the white, compounded of the impreſſions of all the other rays mixed together, the violet was firſt obliterated or weakeſt, and the red laſt or ſtrongeſt. To this reaſoning on the intenſity of the particles as owing to their ſize, I ſee only two objections that can be made. The one is, that the intenſity is increaſed when the rays are thrown into a focus: but we muſt recollect, that the rays in this caſe are mixed, and their particles ſo blended as to be increaſed in ſize; for the number of ſeparate rays thrown into one place will not increaſe their intenſity ſenſibly. The other objection is that paſſage in Newton, where he ſays, " that the orange and yellow are the moſt luminous

nous of all the colours, affecting the ſenſes moſt ſtrongly [*]." Now, beſides that this is an aſſertion oppoſed by the poſitive experiment juſt now quoted, I think an anſwer may be thus made to it: The white light, from which the ſpectrum is never free, which inclines to yellow, and which is compoſed alſo of red, abounds in the yellow and orange of the ſpectrum; ſo that both of theſe colours derive their ſuperior luſtre rather than intenſity from this circumſtance; or, if they have any degree of the latter more than the red, it is in fact owing to their mixture with the red and the other rays which are all in the white.

[*To be continued.*]

IX.

An Account of the Manner in which Heat is propagated in Fluids, and its general Conſequences in the Economy of the Univerſe. By BENJAMIN *Count of* RUMFORD.

[Concluded from p. 348.]

IN order to aſcertain the action of water upon ice retained beneath it, Count Rumford took a cylindrical glaſs jar, 4,7 inches in diameter, and 13,8 inches high. Into this he put 43,87 cubic inches, or 1 lb. 11$\frac{1}{7}$ oz. troy of water. When the jar had been placed in a freezing mixture, and the water was congealed, the ice adhered firmly to the bottom and ſides of the jar, and was juſt three inches high. The jar was then placed in a mixture of pounded ice and pure water, and kept in that ſituation for four hours, in order that the cake of ice might be brought to the temperatnre of 32 degrees.

The jar ſtill ſtanding in a ſhallow diſh in the pounded ice and water, the ſurface of which cold mixture was juſt on a level with the ſurface of the ice in the jar, 73$\frac{1}{4}$ oz. troy of boiling-hot water were gently poured in, which filled it to the height of eight inches above the ſurface of the jar.

In theſe experiments it was evidently of the greateſt conſequence to prevent thoſe irregularities which would ariſe from the action of pouring. The expedient firſt adopted for this purpoſe was to cover the ice with a circular piece of ſtrong paper, which was gently removed after the pouring. But this not being thought ſufficiently effectual, a flat ſhallow diſh of light wood, half an inch deep, and ſomewhat leſs externally than the diameter of the jar, was provided. Its bottom was about a quarter of an inch thick, and was perforated with a great number of ſmall holes, which gave it the appearance of a ſieve. This perforated wooden diſh, having been previouſly made ice-cold, was placed on the ſurface of the ice in the jar, and the hot water was gently poured into the diſh through a long wooden tube. As the perforated diſh floated and remained conſtantly at the ſurface of the water, and as the water paſſing through many hundreds of ſmall holes was not projected downwards with force, the violent motions in the maſs of water in the jar were thus in a great meaſure prevented. The water was not ſuffered to iſſue from the wooden tube in a perpendicular ſtream; but, the bore being cloſed, it was made to iſſue horizontally through a number of ſmall holes in the ſides of the tube at its lower end.

---

[*] Optics, Book i. Part I. Prop. 7.

As ſoon as the operation of pouring the hot water into the jar was finiſhed, the perforated diſh was carefully removed, and the jar was covered with a circular wooden cover, from the centre of which a ſmall mercurial thermometer was ſuſpended. The irregularities of the experiments were greatly diminiſhed by the expedient of the wooden diſh. One more improvement however was added to the manipulation. It conſiſted in pouring $8\frac{1}{4}$ cubic inches of ice-cold water upon the ice in the jar, previous to the introduction of the wooden diſh. This water covered the ſurface to the height of 0,478 of an inch.

Much as I could have wiſhed to relate the whole of the intereſting experiments in this eſſay, the requiſite attention to brevity in this abridgment has made it neceſſary to omit every matter of ſubordinate detail. For the ſame reaſon I forbear to ſtate the reſults afforded by the two firſt methods of trial. Thoſe with the complete apparatus were as follows; the temperature of the room being 41 degrees.

No. of the Experiment.	Time the hot Water was on the Ice.	Temperature of the hot Water one inch below its ſurface.		Quantity of Ice melted.
		At the Beginning.	At the End.	
	Minutes.			Grains.
No. 25	10	192°	182°	580
26	30	190	165	914
27	180	190	95	3200

From the reſults of theſe three experiments, the Count proceeds to determine how much ice was melted *in the act of pouring the water into the jar*, and conſequently the rate at which it was melted in the ordinary courſe of the experiment; ſuppoſing equal quantities to be melted in equal times. I give the reaſoning in his own words.

As in the 27th experiment 3200 grains were melted in 180 minutes, and in the 25th experiment 580 grains were melted in 10 minutes, we may ſafely conclude that the ſame quantity muſt have been melted in the ſame time (10 minutes) in the 27th experiment. If therefore from 3200 grains, the quantity melted in 180 minutes in this laſt experiment, we deduct 580 grains for the quantity melted during the firſt ten minutes, there will remain 2620 grains for the quantity melted in the ſucceeding 170 minutes, when, the motions occaſioned in the water on its being poured into the jar having ſubſided, we may ſuppoſe the proceſs of melting the ice to have gone on regularly.

But if in the regular courſe of the experiment no more than 2620 grains were melted in 170 minutes, it is evident that not more than 154 grains could have been melted in the ordinary courſe of the proceſs in ten minutes; for 170 minutes : 2620 grains : : 10 minutes : 154 grains. If therefore from 580 grains, the quantity of ice actually melted in ten minutes in the 25th experiment, we deduct 154 grains, there remains 426 for the quantity melted in pouring the water into the jar.

Let us ſee now how far this agrees with the reſult of the 26th experiment. In this experiment 914 grains of ice were melted in 30 minutes. If from this quantity we deduct 426 grains, the quantity which, according to the foregoing computation, muſt have been melted *in pouring the hot water into the jar*, there will remain 478 grains for the quantity melted in the ordinary courſe of the proceſs in 30 minutes, which gives 159 grains for the quantity melted in ten minutes; which differs very little from the reſult of the foregoing computation, by which it appeared to be = 154 grains. This difference, however, ſmall as it is, is ſufficient to prove an important fact; namely, that the effects produced by the motion into which the hot water had been thrown on being poured into the jar, had not ceaſed entirely in 10 minutes, or when an end was put to the 5th experiment. We ſhall therefore come nearer the truth, if, in our endeavours to diſcover the quantity of ice melted in any given time in the ordinary courſe of the experiment, we found our computation on the reſults of the two experiments, No. 26 and No. 27.

In the latter of theſe experiments 3200 grains of ice were melted in 180 minutes, and in the former 914 grains were melted in 30 minutes. If, therefore, from 3200 grains, the quantity melted in 180 minutes, we take the quantity melted in the firſt 30 minutes = 914 grains, there will remain 2286 grains for the quantity melted in the ſucceeding 150 minutes, and this gives 152 grains for the quantity melted in 60 minutes. By the former computation it turned out to be 154 grains.

But if 152 grains of ice is the quantity melted in 10 minutes in the ordinary courſe of the proceſs, three times that quantity or 456 grains only would have been melted in this manner in the 30 minutes during which the 26th experiment laſted; and deducting this quantity from 914 grains, the quantity actually melted in that experiment, the remainder 458 grains ſhews how much muſt have been melted in the pouring the hot water on the ice, or in conſequence of the motion into which the water was thrown in the performance of that operation. By the preceding computation this quantity turned out to be 426 grains.

From the reſult of theſe computations the Count thinks we may ſafely conclude, that in the ordinary courſe of the experiments not more than 152 grains of ice were melted by the hot water in ten minutes.

He then proceeds to give an account of ſeveral experiments, in which the water employed to melt the ice was at a much lower temperature.

The previous congelation of water in the jar to the depth of four inches, the ſubſequent reduction to the temperature of 32° by the external contact of ice and water, and the weighing of the whole jar and ice contained therein, were performed as in the former experiments. The jar, which had been wiped with a dry cold napkin before weighing, was then replaced in the ice and water to the depth of its internal contents of ice. The quantity of 73¼ ounces troy of water at the temperature of 41° (which was alſo that of the room) was then poured in with the ſame precaution, of the wooden tube, diſh, &c. as before. The reſults of a ſet of experiments were theſe :

Number of the Experiment.	Temperature of the Water in the jar one inch below its ſurface.		Temperature of the Air.	Time the Water remained on the Ice.	Quantity of Ice melted.
	Beginning of Experiment.	End of Experiment.		Minutes.	Grains.
No. 28	41°	40°	41°	10	203
29	41	40	41	10	220
30	41	40	41	10	237
31	41	40	41	10	228
32	41	38	41	30	617
33	41	38	41	30	585

By the reſults of theſe experiments, and their correſpondence with each other, the extraordinary fact is eſtabliſhed, that boiling water does not fuſe more water while ſtanding on its ſurface in a given time, than water at the temperature of 41°, or only nine degrees above freezing.

Indeed, as the Count obſerves, there is reaſon to conclude that it does not thaw ſo much. For the quantity melted in ten minutes by the hot water was 152 grains: and in the experiments with the cold water the quantity was $189\frac{1}{2}$ grains. He was too much intereſted in theſe reſearches to leave this anomaly unexamined. It remained to be aſcertained, whether the latter experiments were not affected, as well as thoſe with hot water, by the agitation of the water in pouring in, and to what extent; and alſo how far the action of the external air, by cooling the outer part of the cylinder of heated water, and cauſing it to deſcend, might impede the riſing currents of cooled and expanded water from the ſurface of the ice. This laſt object was numerically determined by clothing the jar with cotton wool in ſome experiments, by ſurrounding it totally with ice and water in others, and comparing theſe reſults with ſuch as were afforded when all the part of the jar above the ice had been left expoſed to the atmoſphere. For theſe numbers, the judicious precautions of experiment, and the comparative remarks upon them, I muſt again refer the philoſopher to the Treatiſe itſelf, which every one who wiſhes to inveſtigate the ſubject ſtill farther, to repeat the experiments, or to draw more extenſive theoretical inferences from them, muſt neceſſarily conſult. The concluding Table, drawn from the others which precede, is as follows:

		Ice melted in 30 min. Grains.
In the experiments in which the part of the jar occupied by the water was expoſed uncovered to the air; then at 61°.	With boiling hot water - - -	$558\frac{2}{3}$
	With water at the temperature 61° -	646
	With water at the temperature 41° -	574
In the experiments in which the part of the jar occupied by the water was ſurrounded by pounded ice and water, and conſequently was at 32°.	With boiling hot water - - -	$399\frac{2}{5}$
	With water at the temperature 61° -	661
	With water at the temperature 41° -	542

The Count conſiders theſe experiments as the moſt unqueſtionable proof that water is a perfect *non-conductor of heat*, and that heat is propagated in it only in conſequence of the motions which the heat occaſions in the inſulated and ſolitary particles of that fluid. He remarks, that this diſcovery affords an inſight into the nature of the mechanical proceſs which takes place in chemical ſolutions; and he thinks that it will enable us to account in a ſatisfactory manner for all the various phenomena of chemical affinity, vegetation, and perhaps all the other motions among the inanimate bodies on the ſurface of the globe.

But without dwelling upon theſe circumſtances, leſs immediately deducible from the nature of the communication of heat through water, the author haſtens to apply his diſcoveries to the great operations which are regulated and performed on the ſurface of the globe by virtue of its moſt imperfect conducting power, and the law of its expanſion and contraction, from the changes of temperature. This ſubject occupies the third and concluding chapter of his eſſay. I ſhall give the ſubſtance as nearly in the words of the author as the purpoſes of abridgment will allow.

Though ſummer and winter, ſpring and autumn, and all the variety of the ſeaſons, are produced by the ſimple and admirable contrivance of the inclination of the axis of the earth to the plane of the ecliptic; yet this mechanical diſpoſition would not have been alone ſufficient to produce that gradual change of temperature which the neceſſities of animal and vegetable life appear to require. It ſeems neceſſary not only that the changes ſhould be gradual, but that the extremes of heat and cold ſhould be mitigated by ſome equalizing power.

The cold air from the northern and ſouthern regions of the earth ruſhes towards the heated parts in the regular winds which are known to prevail on its ſurface; and the return of theſe maſſes of air, which, when they become heated, riſe and flow back in the upper departments of the atmoſphere towards their former ſtation, muſt greatly tend to preſerve a more equal temperature of climates than would elſe have prevailed. But the agency of water in producing the ſame effect has been overlooked, though the remarkable law of its condenſation by cold renders it wonderfully ſuited to that purpoſe.

M. de Luc has ſhewn, that by cooling water from the boiling point 80° to the freezing point 0, if the whole contraction be divided into 80 equal portions, the condenſation or loſs of bulk for each 10° will be reſpectively, beginning at 80°, as follows: 18.0;—16.2;—13.8;—11.5;—9.3;—7.1;—3.9;—and 0.2. Whence it is ſeen, that the increaſe of ſpecific gravity, by cooling when near the mean temperature in England, is at leaſt ninety times leſs than when the water is near boiling hot.

The vaſt extent of the ocean, and its great depth, but ſtill more its numerous currents, and the power of water to abſorb a vaſt quantity of heat, render it peculiarly well adapted to ſerve as an equalizer of temperature.

On the retreat of the ſun after the ſolſtice, it is cloſely followed by the cold winds from the regions of eternal froſt, which are continually endeavouring to preſs on towards the equator. As the power of the ſun to warm the ſurface of the earth and the air diminiſhes very faſt in high latitudes on the days growing ſhorter, it ſoon becomes too weak to keep back the denſe atmoſphere which preſſes in from the polar regions, and the cold increaſes very faſt.

There is however a circumſtance by which theſe rapid advances of winter are in ſome meaſure moderated. The earth, but more eſpecially the water, having imbibed a vaſt quantity of heat during the long ſummer days, while they received the influence of the ſun's vivifying beams; this heat being given off to the cold air which ruſhes in from the polar region, ſerves to warm it and ſoften it; and conſequently to diminiſh the impetuoſity of its motion, and take off the keenneſs of its blaſt. But as the cold air ſtill continues to flow in as the ſun retires, the accumulated heat of ſummer is ſoon exhauſted, and all ſolid and fluid bodies are reduced to the temperature of freezing water. In this ſtage the cold in the atmoſphere increaſes very faſt, and would probably increaſe ſtill faſter, were it not for the vaſt quantity of heat which is communicated to the air by the watery vapours, which are firſt condenſed, and then congealed in the atmoſphere, and which afterwards fall upon the earth in the form of ſnow; and by that ſtill larger quantity which is given off by the water in the rivers and lakes and in the ground, upon its being frozen.

But in very cold countries the ground is frozen and covered with ſnow, and all the lakes and rivers are frozen over in the very beginning of winter. The cold then firſt begins to be extreme, and there appears to be no ſource of heat left which is ſufficient to moderate it in any ſenſible degree.

It has been ſhewn—the Count thinks he may venture to ſay proved—in the moſt ſatisfactory manner, that liquids part with their heat only in conſequence of their internal motions, and that the more rapid theſe motions are, the more rapid is the communication of the heat; that theſe motions are produced by the change in the ſpecific gravity of the liquid occaſioned by the change of temperature; and of courſe that they are more rapid as the ſpecific gravity of the liquid is more changed by any given change of temperature.

But it has been ſhown that the change in the ſpecific gravity of water is extremely ſmall, which takes place in any given change of temperature below the mean temperature of the atmoſphere; and particularly when the temperature of the water is very near the freezing point; and hence it follows that water muſt give off its heat very ſlowly when it is near freezing.

But this is not all. When water is cooled to within eight or nine degrees of the freezing point, it not only ceaſes to be farther condenſed, but is actually expanded by farther diminutions of its heat; and this expanſion goes on, as the heat is diminiſhed, as long as the water can be kept fluid; and when it is changed to ice it expands even ſtill more, and the ice floats on the ſurface of the uncongealed part of the fluid.

It is well known that there is no communication of heat between two bodies, as long as they are both at the ſame temperature; and it is likewiſe known that the tendency of heat to paſs from a hot body into one which is colder, with which it is in contact, is greater, as the difference is greater in the temperatures of the two bodies.

Suppoſe now that a maſs of very cold air repoſes on the quiet ſurface of a large lake of freſh water, at the temperature of 55° of Fahrenheit's thermometer. The particles of water at the ſurface, on giving off a part of their heat to the cold air with which they are in contact, and in conſequence of this loſs of heat becoming ſpecifically heavier than thoſe hotter particles on which they repoſe, muſt of courſe deſcend. This deſcent of the particles which have been cooled, neceſſarily forces other hotter particles to the ſurface; and theſe being cooled in their turns, bend their courſe downwards; and the whole maſs of water is put into motion, and continues in motion as long as the proceſs of cooling goes on.

Before he proceeds to trace this operation through all its various ſtages, the Count endeavours to remove an objection which may perhaps be made to his explanation of this phenomenon. As he ſuppoſes the maſs of air which reſts on the ſurface of the water to be *very cold*, and as he has taken it for granted that there is no communication whatever of heat between the particles of water in contact with this very cold air, and the neighbouring warmer particles of water, it may be aſked, how it happens that theſe particles at the ſurface are not ſo much cooled as to be immediately changed to ice? To this he anſwers, that there are two cauſes which conſpire to prevent the *immediate* formation of ice at the ſurface of the water. Firſt, the ſpecific gravity of the particle of water at the ſurface being increaſed at the ſame moment when it parts with heat, it begins to deſcend as ſoon as it begins to be cooled; and before the air has had time to rob it of all its heat, it eſcapes and gets out of its reach: and, Secondly, air being a bad conductor of heat, it cannot receive and tranſmit, or *tranſport* it with ſufficient celerity to cool the ſurface of water ſo ſuddenly as to embarraſs the motions of the particles of that liquid in the operation of giving it off.

But to return to our lake. As ſoon as the water in cooling has arrived at the temperature of about 40°, as at that temperature it ceaſes to be farther condenſed, its internal motion ceaſes, and thoſe of its particles which happen to be at its ſurface remain there; and after being cooled down to the freezing point they give off their latent heat, and ice begins to be formed.

As ſoon as the ſurface of the water is covered with ice, the communication of heat from the water to the atmoſphere is rendered extremely ſlow and difficult; for ice, being a bad conductor of heat, forms a very warm covering to the water; and moreover it prevents the water from being agitated by the wind. Farther, as the temperature of the ice at its lower ſurface is always very nearly the ſame as that of the particles of liquid water with which it is in contact (the warmer particles of this fluid, in conſequence of their greater ſpecific gravity, taking their places below), the communication of heat between the water and the ice is neceſſarily very ſlow on that account.

As ſoon as the upper ſurface of the ice is covered with ſnow (which commonly happens ſoon after the ice is formed), this is an additional and very powerful obſtacle to prevent the eſcape of the heat out of the water; and though the moſt intenſe cold may reign in the atmoſphere, the increaſe of the thickneſs of the ice will be very ſlow.

During this time the maſs of water which remains unfrozen will loſe *no part of its heat*; on the contrary, it will continually be receiving heat from the ground. This heat, which is accumulated in the earth during the ſummer, will not only ſerve in ſome meaſure to replace that which is communicated to the atmoſphere through the ice, and prevent its being furniſhed at the expence of the latent heat of the water in contact with its ſurface; but, when the temperature of the air is not much below that of freezing, this ſupply of heat from below will be quite ſufficient to replace that which the air carries off, and the thickneſs of the ice will not increaſe.

Whenever the temperature of the air is not actually colder than freezing water, the heat which riſes from the bottom of the lake will be all employed in melting the ice at its under ſurface, and diminiſhing its thickneſs.

It will indeed frequently happen, when the ice is very thick, and eſpecially when its upper ſurface is covered with deep ſnow, that the melting of the ice at its under ſurface will be going on, when the temperature of the atmoſphere is conſiderably below the freezing point.

As the particles of the water, which, receiving heat from the ground at the bottom of the lake, acquire a higher temperature than that of 40 degrees (and being expanded, and becoming ſpecifically lighter by this additional heat, riſe to the upper ſurface of the fluid water, and give off their ſenſible heat to the under ſurface of the ice), never return to the bottom, this communication of the heat which exhales from the earth produces very little motion in the maſs of the water; and this circumſtance is no doubt very favourable to the preſervation of the heat of the water.

When a ſtrong wind prevails, and the ſurface of the water is much agitated, ice is not formed, even though the whole maſs of water ſhould, by a long continuance of cold weather, have been previouſly cooled down to that point to which it is neceſſary that it ſhould be brought, in order that its internal motions may ceaſe, and that it may be diſpoſed to congeal. For though the particles at and near the ſurface may no longer have any tendency to deſcend, on being farther cooled, yet, as they have ſo conſiderable a quantity of ſenſible heat (eight or ten degrees) to diſpoſe of, after their condenſation by cold ceaſes; and as the agitation into which the water is thrown by the wind does not permit any particle to remain long enough in contact with the cold air to give off all its heat at once; there is a continual ſucceſſion of freſh particles at the ſurface, all of which give off heat to the air, but none of them have time to be cooled ſufficiently to form ice. The water will therefore loſe a vaſt quantity of heat; and as ſoon as the wind ceaſes, if the cold ſhould continue, ice will be formed very rapidly.

But it is not merely the agitation of the water which renders the communication of the heat very rapid; the agitation of the wind alſo tends to produce the ſame effect.

On the return of ſpring, the ſnow melting before the ſun, as he advances and his rays become more powerful, all the heat which the earth exhales is employed in diſſolving the ice at its under ſurface, while the ſun on the other ſide acts ſtill more powerfully to produce the ſame effect.

Though ice is tranſparent, yet it is not perfectly ſo; and as the light which is ſtopped, in its paſſage through it, cannot fail to generate heat *when* and *where* it is ſtopped or abſorbed, it is by no means ſurpriſing that ſnow ſhould be found to melt when expoſed in the ſun's rays, even when the temperature of the air in the ſhade is conſiderably below the point of freezing. Snow expoſed to the ſun melts long before the even ſurface of ice begins to be ſenſibly ſoftened by its beams; and it is not till ſome time after all the hills are bare that the ice on the lakes and rivers breaks up.

The rays which penetrate a bank of ſnow, being often reflected and refracted, deſcend deep into it, and the heat is depoſited in a place where it is not expoſed to be carried off by the cold air of the atmoſphere; but the rays which fall upon the horizontal and ſmooth ſurface of the ice are moſtly reflected upwards into the atmoſphere: and if any part of them are ſtopped at the ſurface of the ice, the heat generated by them *there* is inſtantaneouſly carried off by the cold air, and a particle of water is no ſooner made fluid than it is again frozen.

Hence we ſee, that the ſnow which in cold countries covers the ice that is formed on the ſurface of freſh water, not only prevents the heat of the water from being carried off by the air during the winter, but alſo aſſiſts very powerfully in thawing the ice early in the ſpring.

Let us now ſee what the conſequences would have been, had the condenſation of water with cold followed the law which obtains in regard to all other fluids.

As the internal motion of the water could not have failed to continue as long as its ſpecific gravity continued to be increaſed by parting with heat, ice would not have begun to be formed till the whole maſs of water had arrived at the temperature of 32° of Fahrenheit's thermometer.

To ſee what an enormous quantity of heat would be loſt when the water is deep, in conſequence of its whole maſs being cooled in this manner, we have only to compute how much ice this heat would melt, or how much water it would heat, from the point of freezing to that of boiling.

It has been ſhewn by experiments, that any given quantity of ice requires as much heat to melt it as an equal quantity of fluid water loſes in cooling 140°; conſequently the quantity of ice which might be melted by the heat given off by any given quantity of water in cooling any given number of degrees, is to the given quantity of water as the number of degrees which it is cooled to 140°.

Hence it follows, that when the temperature of the water is 8° above the freezing point, it gives off in cooling, down to that temperature, as much heat as would melt $\frac{8}{140}$ths or $\frac{2}{35}$ths of its weight of ice: the water therefore which is cooled from the temperature of 40° to that of 32°, if it be 35 feet deep, will give off as much heat, in being ſo cooled, as would melt a covering of ice two feet thick.

But this even is not all; for, as the particles of water, on being cooled at the ſurface, would in conſequence of the increaſe of their ſpecific gravity, on parting with a portion of their heat, immediately deſcend to the bottom, the greateſt part of the heat accumulated during the ſummer in the earth on which the water repoſes would be carried off and loſt before the water began to freeze; and when ice was once formed, its thickneſs would increaſe with great rapidity, and would continue increaſing during the whole winter. And it ſeems very probable, that, in climates which are now temperate, the water in the large lakes would be frozen to ſuch a depth in the courſe of a ſevere winter, that the heat of the enſuing ſummer would not be ſufficient to thaw them; and ſhould this once happen, the following winter could hardly fail to change the whole maſs of its waters to one ſolid body of ice, which never more could recover its liquid form, but muſt remain immoveable till the end of time.

In the month of February, after a froſt which had laſted a month, the temperature of the air being 38°, M. De Sauſſure found the temperature of the water at the Lake of Geneva, at the ſurface, at 41°, and at the depth of 1000 feet, at 40°. Had the froſt continued but a little longer, ice would have been formed; but had the conſtitution of water been ſuch, that the whole maſs of that fluid in the lake muſt have been cooled down to the temperature of 32° before ice could have been formed, this event could not have happened till the water had given off as much heat as would be ſufficient to melt a covering of ice above 57 feet thick.

This quantity of heat would be ſufficient to heat to the point of boiling a quantity of ice-cold water as large as the lake, and 49 feet deep.

When we trace ſtill further the aſtoniſhing effects which are produced in the world by the operations of that ſimple law, which has been found to obtain in the condenſation of water on its being deprived of heat, we ſhall find more and more reaſon to admire the wiſdom of the contrivance.

That high latitudes might be habitable, it was neceſſary that vegetables ſhould be protected from the effects of the chilling froſts of a long and ſevere winter; but if it be true that watery liquids do not part with their heat but in conſequence of their internal motion, and if theſe motions are occaſioned merely by the change produced in the ſpecific gravity of thoſe particles of the liquid which receive heat, or which part with it, who does not ſee how very powerfully the ſudden diminution and final ceſſation of the condenſation of water in cooling, as ſoon as its temperature approaches to the freezing point, operates to prevent the ſap in vegetables from being frozen?

But if, for the purpoſes of life and vegetation, it be neceſſary that the ground, the rivers, the lakes, and the trees, be defended againſt the cold winds from the poles, it may be aſked, how this inundation of cold air is to be warmed? The Count anſwers, By the waters of the ocean, which there is the greateſt reaſon to think were not only deſigned principally for that uſe, but particularly prepared for it.

Sea water contains a large proportion of ſalt in ſolution; and the condenſation of a ſaline ſolution on its being cooled, follows a law which is extremely different from that obſerved in regard to pure water, and which (as may eaſily be ſhown) renders it peculiarly well adapted for communicating heat to the cold winds which blow over its ſurface.

As ſea water continues to be condenſed as it goes on to cool, even after it has paſſed the point at which freſh water freezes, the particles at the ſurface, inſtead of remaining there, after the maſs of the water had been cooled to about 40°, and preventing the other warmer particles below from coming in their turns, and giving off their heat to the cold air (as we have ſeen always when freſh or pure water is ſo cooled)—theſe cooled particles of ſalt water deſcend as ſoon as they have parted with their heat, and in moving downward force other warmer particles to move upwards; and in conſequence of this continual ſucceſſion of warm particles which come to the ſurface of the ſea, a vaſt deal of heat is communicated to the air—incomparably more than could poſſibly be communicated to it by an equal quantity of freſh water at the ſame temperature, as will appear by the following computation:

Without taking into the account that very great advantage which ſea water poſſeſſes over freſh water, conſidered as an equalizer of the temperature of the atmoſphere, which ariſes from the comparative *lowneſs of the point of its congelation*; ſuppoſing even ſea water to freeze at as high a temperature as freſh water, namely at 32°; and ſuppoſing (what is ſtrictly true) that as ſoon as either ſea water or freſh water is frozen at its ſurface, and this ice covered with ſnow, the communication of heat from the water to the atmoſphere ceaſes almoſt entirely; the Count proceeds to determine how much more heat would, even on this ſuppoſition, be communicated to the air by ſalt water than by freſh water, after both have arrived at the temperature of 40°.

When freſh water, in cooling, has arrived at this temperature, it ceaſes to be farther condenſed with cold, and its internal motions (which, as hath already more than once been obſerved, are cauſed ſolely by the changes produced in the ſpecific gravity of its particles) ceaſe of courſe, and ice immediately begins to be formed on its ſurface: but as the condenſation of ſalt water goes on as its heat goes on to be diminiſhed, its internal motions will continue; and it is evidently impoſſible for ice to be formed at its ſurface, till the whole maſs of the water has become ice cold, or till its temperature is brought down to the ſuppoſed point 32°. It would therefore give off a quantity of heat equal to 8 degrees at leaſt of Fahrenheit's thermometer more than the freſh water would part with before ice could be formed on its ſurface.

To be able to form an idea of this enormous quantity of heat, we have only to recollect what has already been ſaid, and we ſhall find reaſon to conclude that it would be ſufficient to melt a covering of ice equal in thickneſs to $\frac{2}{35}$ths of the depth of the ſea. It would therefore be ſufficient in that part of the North ſea (lat. 67) where Lord Mulgrave ſounded at the depth of 4680 feet, to melt a cake of ice 265 feet thick.

But the heat evolved in the formation of each ſuperficial foot of ice, would be ſufficient to raiſe the temperature of a ſtratum of incumbent air 2220 times as thick as the ice (conſequently in the caſe in queſtion 265 × 2220 feet or 869 miles thick) 28 degrees, or from the temperature of freezing water to that of 50° of Fahrenheit's thermometer, or to the mean annual temperature of the northern parts of Germany.

The heat given off to the air by each ſuperficial foot of water in cooling *one degree*, is ſufficient to heat an incumbent ſtratum of air 44 times as thick as the depth of the water 10 degrees. Hence we ſee how very powerfully the water of the ocean, which is never frozen over except in very high latitudes, muſt contribute to warm the cold air which flows in from the polar regions.

But the ocean is not more uſeful in moderating the extreme cold of the polar regions than it is in tempering the exceſſive heats of the torrid zone; and what is very remarkable, the fitneſs of the ſea water to ſerve this laſt important purpoſe, is owing to the very ſame cauſe which renders it ſo peculiarly well adapted for communicating heat to the cold atmoſphere in high latitudes, namely, *to the ſalt which it holds in ſolution.*

As the condenſation of ſalt water with cold continues to go on, even long after it has been cooled to the temperature at which freſh water freezes, thoſe particles at the ſurface which are cooled by an immediate contact with the cold winds muſt deſcend, and take their places at the bottom of the ſea, where they muſt remain, till by acquiring an additional quantity of heat their ſpecific gravity is again diminiſhed. But this heat *they never can regain in the polar regions*; for innumerable experiments have proved beyond all poſſibility of doubt, that there is no *principle of heat* in the interior parts of the globe, which by exhaling through the bottom of the ocean could communicate heat to the water which reſts upon it.

It has been found that the temperature of the earth, at great depths under the ſurface, is different in different latitudes, and there is no doubt but this is alſo the caſe with reſpect to the temperature at the bottom of the ſea, in as far as it is not influenced by the currents which flow over it; and this proves to a demonſtration, that the heat which we find to exiſt without any ſenſible change during ſummer and winter at great depths, is owing to the action of the ſun, and not to central fires as ſome have too haſtily concluded.

But if the water of the ocean, which, on being deprived of a great part of its heat by cold winds, deſcends to the bottom of the ſea, cannot be warmed *where it deſcends*, as its ſpecific gravity is greater than that of water at the ſame depth in warmer latitudes, it will immediately begin to ſpread on the bottom of the ſea, and to flow towards the equator; and this muſt neceſſarily produce a current at the ſurface in an oppoſite direction. There are the moſt indubitable proofs of the exiſtence of both theſe currents.

The proof of the exiſtence of one of them would, indeed, have been quite ſufficient to have proved the exiſtence of both; for one of them could not poſſibly exiſt without the other: but they are ſeveral direct proofs of the exiſtence of each of them.

What has been called the gulph ſtream in the Atlantic Ocean, is no more than one of theſe currents, namely that at the ſurface, which moves from the equator towards the north pole, modified by the trade winds, and by the form of the continent of North America; and the progreſs of the lower current may be conſidered as proved directly by the cold which has been found to exiſt in the ſea at great depths in warm latitudes; a degree of temperature much below the mean annual temperature of the earth in the latitudes where it has been found, and which of courſe muſt have been brought from colder latitudes.

The mean annual temperature in the latitude of 67° has been determined by Mr. Kirwan, in his excellent treatiſe on the temperature of different latitudes, to be 39°; but Lord Mulgrave found on the 20th of June, when the temperature of the air was 48½°, that the temperature of the ſea he depth of 4680 feet was 6 degrees below freezing, or 26° of Fahrenheit's thermometer.

On the 31ſt of Auguſt in the latitude of 69°, where the annual temperature is about 38°, the temperature of the ſea at the depth of 4038 feet was 32°; the temperature of the atmoſphere (and probably that of the water at the ſurface of the ſea) being at the ſame time at 59½.

But a ſtill more ſtriking and incontrovertible proof of the exiſtence of currents of cold water at the bottom of the ſea, ſetting from the poles towards the equator, is the very remarkable difference that has been found to ſubſiſt between the temperature of the ſea at the ſurface, and at great depth at the tropic, though the temperature of the atmoſphere there is ſo conſtant, that the greateſt changes produced in it by the ſeaſons ſeldom amount to more than five or ſix degrees; yet the difference between the heat of the water at the ſurface of the ſea, and that at the depth of 3600 feet, has been found to amount to no leſs than 31 degrees; the temperature above, or at the ſurface, being 84, and at the given depth below, no more than 53 [*].

It appears to the Count to be extremely difficult, if not quite impoſſible, to account for this degree of cold at the bottom of the ſea in the torrid zone, on any other ſuppoſition than that of cold currents from the poles; and the utility of theſe currents in tempering the exceſſive heats of thoſe climates is too evident to require any illuſtration.

Theſe currents are produced, as we have already ſeen, in conſequence of the difference in the ſpecific gravity of the ſea water at different temperatures: their velocities muſt therefore be in proportion to the change produced in the ſpecific gravity of water by any given change of temperature; and hence we ſee how much greater they muſt be in ſalt water than they could poſſibly have been had the ocean been compoſed of freſh water.

It is not a little remarkable, that the water of all great lakes is freſh, and nearly ſo in all inland ſeas (like the Baltic) in cold climates, which communicate with the ocean by narrow channels. We ſhall find reaſon to conclude, that this did not happen without deſign, when we conſider what conſequences would probably enſue, ſhould the waters of a large lake in an inland ſituation in a cold country (ſuch as the lake Superior for inſtance in North America) become as ſalt as the ſea.

Though the cold winds which blow over the lake in the beginning of winter would be more warmed, and the temperature of the air on the ſide of the lake oppoſite to the quarter from whence theſe winds arrive, would be rendered ſomewhat milder than it now is; yet, as the water of the lake would give off an immenſe quantity of heat before a covering of ice could be formed on its ſurface for its protection, it would on the return of ſpring be found to be extremely cold; and as it would require a long time to regain, from the influence of the returning ſun, the enormous quantity of heat loſt during the winter, it would remain very cold during the ſpring, and probably during the greateſt part of the ſummer; and this could not fail to chill the atmoſphere, and check vegetation in the ſurrounding country to a very conſiderable diſtance. And though a large lake of ſalt water in a cold country would tend to render the winter ſomewhat milder on one ſide of it, namely on the ſide oppoſite to the quarter from whence the cold winds came; yet this advantage would not only be confined to a ſmall tract of country, but would not any where be very important, and would by no means counterbalance the extenſive and fatal conſequences which would be produced in ſummer by ſo large a collection of very cold water.

---

[*] Philoſophical Tranſactions M.DCC.LII.

When the winter is once fairly ſet in—when the earth is well covered with ſnow, and the rivers and lakes with ice, and more eſpecially when the ice as well as the land is covered with that warm winter garment, a few degrees more of cold in the air cannot produce any laſting bad conſequences. It may oblige the inhabitants to uſe additional precautions to guard themſelves, their domeſtic animals, and their proviſions, from the uncommon ſeverity of the weather; but it can have very little influence in the temperature of the enſuing ſummer; and it is even probable, if it influences it at all, that it tends rather to make it warmer than colder. Lakes of ſalt water could therefore be of no real uſe in winter in cold countries, and in ſummer they could not fail to be very hurtful; while freſh lakes, as they are frozen over almoſt as ſoon as the winter ſets in, and long before the whole maſs of their water is cooled down to the temperature of freezing, muſt preſerve the greater part of their heat through the winter; and if they are of no uſe during the cold ſeaſon, they probably do little or no harm in ſummer*.

X.

Uſeful Notices reſpecting various Objects.—Welding of Caſt-Steel—Flexure of Compound Metallic Bars by Change of Temperature.

1. *Welding of Caſt-Steel. By Sir* THOMAS FRANKLAND, *Bart.*†

THE uniting of ſteel to iron by welding is a well-known practice; in ſome caſes for the purpoſe of ſaving ſteel; in others, to render work leſs liable to break, by giving the ſteel a back or ſupport of a tougher material.

Ever ſince the invention of caſt-ſteel (or bar-ſteel refined by fuſion) it has generally been ſuppoſed impoſſible to weld it either to common ſteel or iron; and naturally—for the deſcription in Watſon's Chemical Eſſays (vol. iv. p. 148) is juſt, that in a welding heat it "runs away under the hammer like ſand." How far the Sheffield artiſts, who ſtamp much low-priced work with the title of caſt-ſteel, practiſe the welding it, I am ignorant; but though I have enquired of many ſmiths and cutlers in different parts of the kingdom, I have not yet found the workman who profeſſed himſelf able to accompliſh it. If therefore I ſhould deſcribe a ſimple proceſs for the purpoſe, I may be of uſe to the very many who are incredulous on the ſubject. If any one has made the diſcovery on principle, he has reaſoned thus: Caſt-ſteel in a welding heat is too ſoft to bear being hammered; but is there no lower degree of heat in which it may be ſoft enough to unite with iron, yet without hazard of running under the hammer? A few experiments decided the queſtion; for the fact is, that caſt-ſteel in a white heat, and iron in a welding heat, unite completely.

---

* Throughout this Eſſay, but more particularly in the laſt chapter, the author has very ſtrongly inſiſted on the final cauſes of the laws he has endeavoured to develope. I have no wiſh at preſent to diſcuſs the queſtion, how far the propagation of truth may be advanced by ſpeculations on thoſe cauſes; but have added this note, leſt I ſhould be thought to have mutilated his Eſſay by leaving out ſo ſtriking a part. My aim has been to communicate his phyſical diſcoveries only. N.

† Phil. Tranſ. M.DCC.XCV. 296.

It muſt not be denied that conſiderable nicety is required in giving a proper heat to the ſteel; for, on applying it to the iron, it receives an increaſe of heat, and will ſometimes run on that increaſe, though it would have borne the hammer in that ſtate in which it was taken from the fire.

I need ſcarcely obſerve, that when this proceſs is intended, the ſteel and iron muſt be heated ſeparately, and the union of the parts propoſed to be joined, effected at a ſingle heat. In caſe of a conſiderable length of work being required, a ſuitable thickneſs muſt be united, and afterwards drawn out, as is practiſed in forging reap-hooks, &c.

The ſteels on which my experiments have been made, are Walker's of Rotherham, and Huntſman's, between which I diſcover no difference; and though there may be ſome trifling variation in the flux uſed for melting, they are probably the ſame in eſſentials.

2. *Flexure of Compound Metallic Bars by Change of Temperature.*

A BAR of ſteel upwards of ſix inches long, 0.56 inch broad, and 0.05 inch thick, was hard-ſoldered * to a bar of braſs of the ſame dimenſions, but twice the thickneſs. This compound bar was fixed at one end to a ſimple metallic bar, as in fig. 2. Pl.V. (facing p. 96) and at the other end there was a ſliding piece to meaſure the deviations by flexure. It muſt be obſerved, that a thin piece of metal was placed between the ſimple and compound bars, at the pinned part A; ſo that the bars were not permitted to come into contact in any temperature, but each poſition was determined by the ſliding piece, by means of a micrometer-ſcrew and magnifier applied to it whenever its place was required to be known. The clear part of the compound bar from the pin A, to the end D, was exactly ſix inches.

By heating the apparatus from 66° to 212°, namely 146°, the catch was moved through 129 parts of the micrometer, each of which was $\frac{1}{1400}$th of an inch. The quantity 129 was therefore equal to 0.0921 inch.

Both ſides of the compound bar were then carefully filed away till the metals were each (as near as could be aſcertained) only half their former thickneſs. The flexure, by a like difference of temperature, as in the former trial, now proved 278 parts, or 0.199 inch.

The bar was then filed to about $\frac{3}{8}$ths of its original thickneſs, ſtill preſerving the relative thickneſſes of the metals. Its flexure by the ſame heat as before was 313 parts, or 0.224 inch; in which, however, from the ſpring of the thin bar, it was doubted whether the catch had been moved to the ſame diſtance as the bar itſelf might have moved alone.

On conſidering theſe deviations, the numbers prove to be inverſely as the thickneſſes of the bars, with no greater error than might ariſe from the meaſures by which the real thickneſſes were aſcertained. Theſe experiments may therefore be ſaid to agree with the theory †.

---

* For the information of thoſe who may not be acquainted with the proceſſes of ſoldering, I muſt obſerve, that this operation conſiſts in uniting the pieces by the fuſion of a kind of braſs, which contains an extraordinary proportion of zinc, and flows into the joint before the heat is ſufficient to melt the pieces. This kind of work will bear hammering and bending, becauſe the ſolder is not brittle. Soft-ſoldering is effected by plumbers' ſolder or tin, which forming a kind of bell metal with the copper is brittle in the joint, and therefore unfit for work intended to ſuſtain either blows or flexure.

† Philoſophical Journal, I. 64.

Fig. 1.

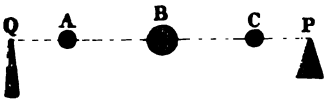


Fig. 2.

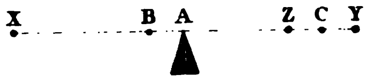


Fig. 3.

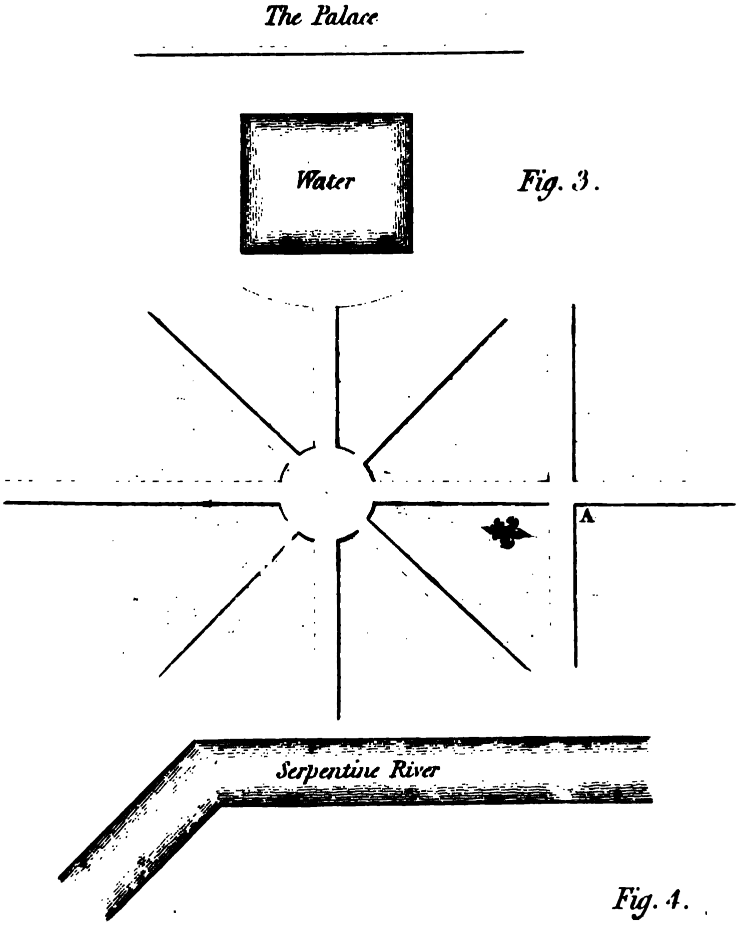


Fig. 4.

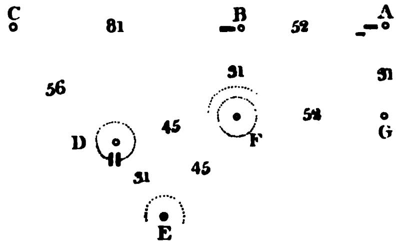
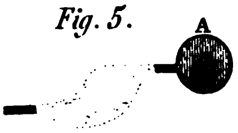


Fig. 5.



Barlow sculp.

THE NEW YORK
PUBLIC LIBRARY

R

Fig. 1.

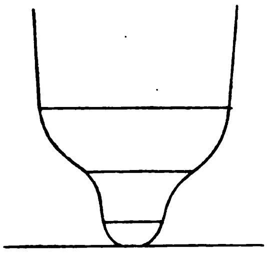


Fig. 2.

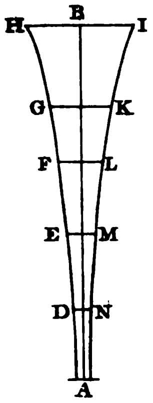


Fig. 3.

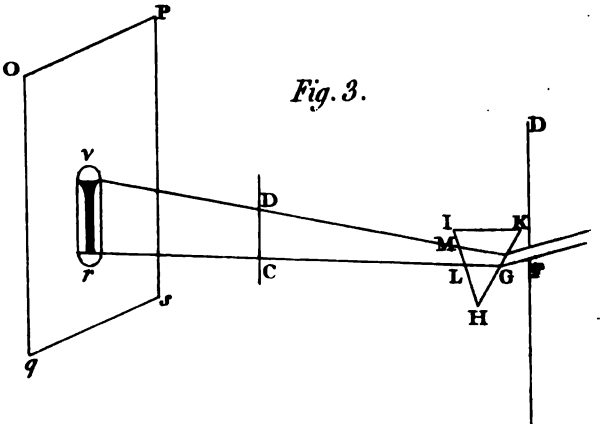


Fig. 4.

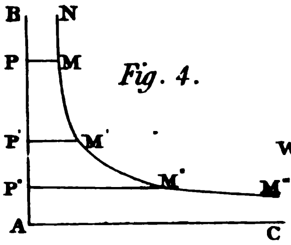


Fig. 5.

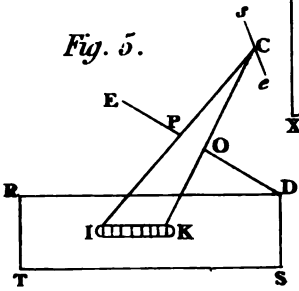


Fig. 6.

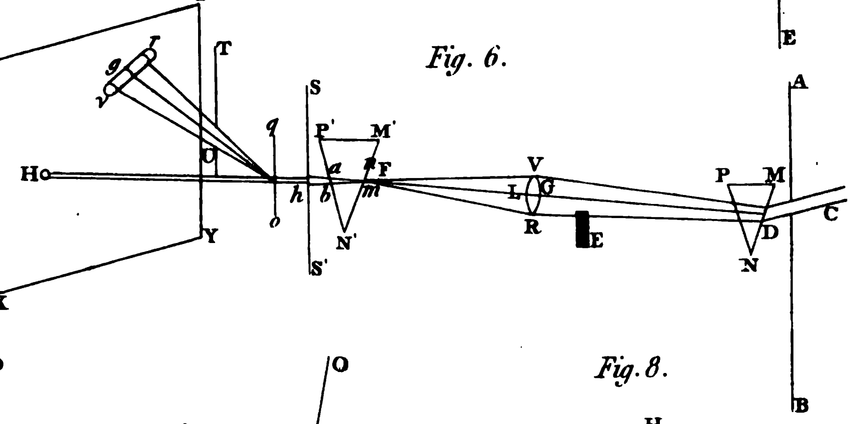


Fig. 7.

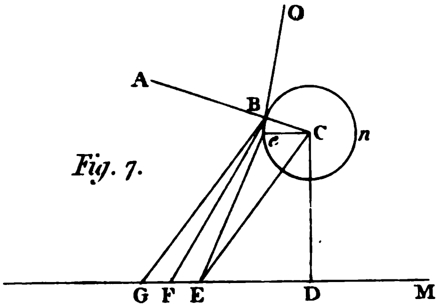


Fig. 8.

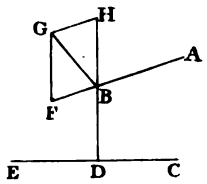


Fig. 9.

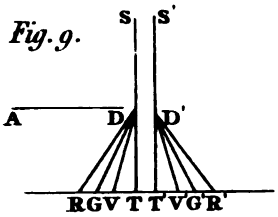


Fig. 10.

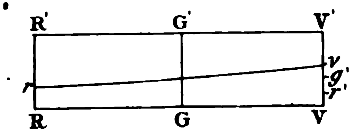


Fig. 11.

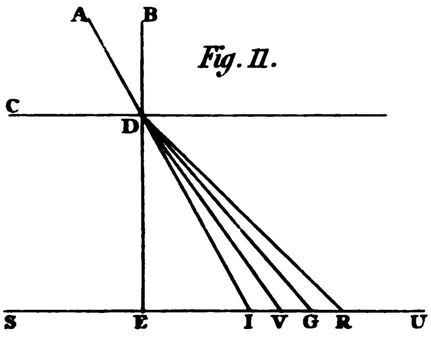


Fig. 12.

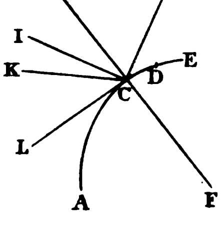
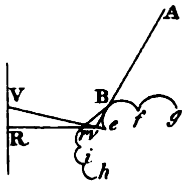


Fig. 13.



Barlow sculp.

THE NEW YORK
PUBLIC LIBRARY

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH 1798.—*SUPPLEMENT.*

ARTICLE I.

Obſervations on Water-Spouts ſeen from Nice. By M. MICHAUD, Correſpondent with the Royal Academy of Sciences at Turin [*].

WATER-ſpouts, as Mr. Senebier remarks, are phenomena which ariſe too ſeldom, and are too difficult of obſervation, to admit of our forming an accurate notion of their circumſtances, or pointing out the means by which their cauſes may be inveſtigated. Yet, as nature is never more diſpoſed to explain her ſecret operations than in ſuch as are performed on a great ſcale, there is no doubt but that theſe phenomena, if well explained, would lead to uſeful reſults [†].

I do not flatter myſelf with the hope of giving a complete explanation of the cauſe of water-ſpouts, ſuch as M. Senebier appears to deſire; but as the true explanation can only be had by careful and frequent obſervation, I hope my work will not be totally uſeleſs if it ſhould afford an additional ſtep towards the deſired end.

My obſervations did not in ſtrictneſs commence till the 6th of January 1789, but it appears neceſſary, for the ſatisfaction and information of my readers, to mention ſome facts of an earlier date. I muſt therefore remark, that after a mild ſeaſon for the greateſt part of the month of December 1788, at Nice, where the winter is not in general ſevere, with clear weather in the day-time, our atmoſphere underwent a total change on the day of the new moon, which was the 27th of that month. On that day a very violent ſtorm of wind aroſe, attended with a degree of cold as acute as ever was known in the memory of man. The ſky became covered with clouds, and ſnow fell to the depth of more than eight inches.

---

[*] Turin Mem. VI.

[†] Senebier ſur les Moyens de perfectionner la Météorologie. Journal de Rozier, Mai 1787.

~~As the ſame effects are produced by the ſame cauſes, I have little doubt but that water-~~ ſpouts were formed near us on the night between the 27th and 28th. But, as I did not ſee them, it becomes unneceſſary for me to enlarge upon any which I did not myſelf clearly obſerve.

The ſevere cold had frozen the ſnow, and rendered it ſo compact that, notwithſtanding the influence of ſeveral days of clear weather ſoon afterwards, in which the heat of the ſun was very perceptible, there was not the leaſt drop of water fell from the eaves of the houſe in which I dwell, which is expoſed to the ſun in winter for eight hours, and, being ſituated near the ſea, is perfectly ſheltered on the north ſide by the eminence of the rock of the caſtle. This fact appeared very ſurpriſing to me, after a reſidence of about forty years in this town. Several old perſons remarked, that this ſnow would wait for another fall before it melted, and I found by the reſult that the obſervation was true.

On Sunday the fourth of January 1789, at the phaſis of the firſt quarter of the moon, the cold was again renewed, and continued ſevere on the Monday and Tueſday. At eight in the morning I firſt obſerved an immenſe maſs of clouds towering upwards, and extending from north-eaſt to ſouth, which roſe towards the zenith, by advancing to the weſtward. Accuſtomed as I am to conſider theſe clouds according to the ſyſtem of my old Profeſſor of Natural Philoſophy, Father Beccaria*, I concluded that they would proceed to deſolate our fields, the fruits of which, particularly the oranges and lemons, had already periſhed by the antecedent cold. And as a ſtrong wind then prevailed over the face of the ſea, I foretold to my two eldeſt ſons, that it was very probable we might diſcover ſome water-ſpout in the courſe of the day. In fact, about five minutes after ten in the morning, I obſerved on the ſea, at the diſtance of not more than a muſket-ſhot from the ſhore, a round ſpace of ten or twelve toiſes in diameter, in which the water did not really boil, but ſeemed ready to boil. Plate XXIV. For there appeared all round, and ſometimes within the circle, vapours in the form of miſts, eight toiſes and more in height, having the appearance, though on a ſcale incomparably larger, of thoſe vapours which riſe from the ſurface of water beginning to ſimmer. I ſaw clearly that this was, if I may ſo expreſs myſelf, the embryo of the foot of a water-ſpout driven along by the wind, while the clouds were not ſufficiently advanced to afford the ſtem a body. It continued therefore to move before the wind from eaſt to weſt, keeping, to my very great ſurpriſe, its ſurrounding vapours, elevated like ſails, notwithſtanding the extreme force of impulſion which drove it towards the ſhore. As ſoon as it came near the land, the circle was contracted, the maſs of vapour became of leſs dimenſions, and at the moment it touched the land it was at once overſet by the wind, under the appearance of a long train of miſt, fig. 1, b, which was ſpeedily diſſipated. I then perceived that the hope I had formed of ſeeing water-ſpouts during the day, was on the point of being realized: but as my occupations demanded my attendance elſewhere, I charged my two eldeſt ſons to watch alternately at the window, in order that the phenomenon might not paſs unobſerved.

At laſt, about eight minutes before noon, my ſecond ſon came to me, exclaiming, "Father, here is a very ſuperb water-ſpout." His earneſtneſs was equal to that of a ſailor,

---

[*] Electriciſme Atmoſpherique. We have a tranſlation in Engliſh.

who, after a long and tedious voyage, firſt diſcovers the land. I followed him to the window, and beheld an immenſe water-ſpout paſſing majeſtically before Nice, as repreſented in fig. 2. The clouds had already occupied not only the upper and ſouthern part of the atmoſphere, but they had proceeded towards the weſt, ſo as to cover the whole extent within my view; with this circumſtance nevertheleſs, that they had left uncovered beneath and towards the ſouth a part in the form of a ſegment of a circle, through which, at an extreme diſtance, ſome clouds were diſcerned, upon which the ſun threw the colours of the morning.

The foot of this water-ſpout, which was in every reſpect infinitely ſuperior to that which Meſſrs. Papacin, Renaud, and myſelf, obſerved in 1780, was ſo ample, that a man of war of 100 guns, with all its ſails, might have been enveloped and even concealed in it. For we are, from daily practice, ſufficiently accuſtomed to judge of the ſize of objects at different diſtances by that of the image formed upon the retina, aſſiſted by our habitual reaſoning. And hence, from the circular form of the foot of this water-ſpout, ſome judgment may be made of the volume of vapour it afforded.

Inſtead of the tranquillity it exhibited at its firſt appearance, this lower part aſſumed the reſemblance of the crater of a volcano, with this exception, that it threw out nothing but large ſtreams of cloud and ſpouts of ſea-water. But it threw theſe in parabolic ſtreams from the centre to the circumference, and all around, with ſuch impetuoſity and violence as to render it very evident to us, that an inexpreſſible effervescence muſt have prevailed in the interior baſon, though the great diſtance and the opacity of the ſurrounding vapour prevented us from ſeeing the phenomena as diſtinctly as we ſaw the ebullition of the water-ſpout of 1780*.

The diameter of the water-ſpout, and that of its expanded upper part, were large in proportion. Its colour was a very deep indigo, the ſame as that of the clouds, which extended from eaſt to weſt. It was impoſſible for us to obſerve the aſcent of the vapours of freſh water; but the obſervation of 1780, in which this was clearly ſeen, ſupplies this defect. It will alſo be ſhewn that the aſcenſion was again ſeen in a moſt complete manner.

While we were looking at this extraordinary appearance, which my ſons beheld for the firſt time, and which ſeemed to have concentrated all their ſenſes in one, on a ſudden an impetuous ſhower of hail diſcharged itſelf againſt the windows in grains of the ſize of piſtol and muſket balls. We immediately ſuſpended our obſervations, in order to cloſe the ſhades of both ſtories of the houſe, in which the whole family aſſiſted, for fear of having the windows abſolutely broken to pieces, as happened a few years before. But I ſoon perceived that this precaution was abſolutely uſeleſs, or at leaſt unneceſſary. For the hail, though in a few minutes it covered the ground to the height of four inches, did not in the leaſt damage the trees in the garden behind our houſe. It conſiſted merely of large flakes of ſnow rounded by the wind in their fall, and poſſeſſing neither the weight nor the hardneſs of hail. Upon opening ſome of the pieces I found them to conſiſt of a thin compact ſhell, nearly empty within, excepting a few rays from the centre to the circumference.

---

* See the Journal de Phyſique, XXX. Part I. 284. for Obſervation ſur une Trombe de Mer, faite à Nice de Provence en 1780, et addreſſée à M. Faujas de St. Fond, par M. Michaud. The facts are leſs minutely ſtated, but nearly agree with the preſent memoir. N.

The degree of congelation in theſe balls was ſo ſlight that they began to melt the moment they touched the ground, and accelerated the fuſion of the ſnow which had fallen before.

This frozen ſnow, which during its fall had obſcured the air ſufficiently to prevent our ſeeing the water-ſpout through the blinds, having ceaſed, we reſumed our obſervations with all poſſible diligence, and beheld another water-ſpout ſomewhat inferior in magnitude to the former which had diſappeared. It followed nearly the ſame courſe as the other. By the account of time employed by each in its ſucceſſive paſſage, I eſtimated that the one before us muſt have been the third; nevertheleſs, by confining my narration to what I really ſaw, it muſt be conſidered as the ſecond only. This water-ſpout having continued its courſe towards Antibes, we obſerved that it began to contract in all its dimenſions, ſome time before it arrived at the ſhore, and that the foot was reduced to nothing when it touched the ground. It contracted inſenſibly upwards, the expanded conical part became broader and more rare, and the whole joined the maſs of clouds in the ſame manner as one miſt incorporates with another. There muſt conſequently be ſome error in the account of the obſervation of 1780, where I ſay that the ſpout withdrew upwards as quick as lightning. This expreſſion gives too precipitate an idea of the diſſolution of the water-ſpout. Having thus kept ſight of it until its total extinction, I returned towards the place where I had diſcovered the firſt water-ſpout, and was greatly ſurpriſed at diſcovering a new foot ready formed, without any deſcending ſpout a, fig. 3, Plate XXV. My aſtoniſhment was founded on theſe three circumſtances:

1. The exiſtence of the foot of the water-ſpout without its ſtem or body. For before this obſervation, and from the facts in 1780, I conſidered it as indubitable that the enveloping matter of the foot or recipient was a production of the body of the water-ſpout itſelf, or an expanſion of its proper ſubſtance. Now I ſaw clearly enough in this phenomenon the identity of the ſubſtance which compoſes clouds and miſts, and that it was not ſupplied by the water-ſpout. The embryo of the ſpout which I had ſeen at ten o'clock, appeared to ſhow that it was probably produced by the ſea.

2. I was ſurpriſed to ſee that this foot was ſtationary at the place of its formation, whereas thoſe which I had before ſeen were carried ſwiftly along by the wind. For though it was not impoſſible but that this foot might be carried by a motion along the line of ſight, and conſequently not perceptible by me; it was at leaſt certain that it gained nothing from eaſt to weſt, that is to ſay, from my left to my right, the direction in which the ſea, the clouds, and the other water-ſpouts which had travelled ſo far in ſo ſhort a ſpace of time, were carried.

3. I was aſtoniſhed, that the body of the water-ſpout being wanting, which, according to my notions, might increaſe the intenſity of the power by which this appearance is produced, it was nevertheleſs poſſible that this envelope ſhould be capable of remaining upright and ſtationary. In this uncertainty I ſuſpended my reflexions to obſerve the reſult. I remarked a kind of teat or protuberance, b, fig. 3, projecting obliquely from the lower part of the clouds which arrived from the eaſt. The foot continued motionleſs, and the protuberance preſerved its oblique direction, till the moment when by the action of the wind it arrived at the foot; at which inſtant we all three obſerved the protuberance direct itſelf perpendicularly towards the foot, and like an immenſe ſack of gauze unroll itſelf

from

from the extremity c, fig. 3; when the folds of this ſack diſappeared, and the body of the water-ſpout, which was grey and tranſparent, fixed itſelf in the bottom of the foot, aſſumed the vertical poſition, and became larger in diameter. My ſecond ſon, who, as well as his elder brother, poſſeſſes a very clear ſight, immediately exclaimed, "See, father, how rapidly the vapours fly up through the bag." I ſaw, in fact, that they ſeemed to expand it with a kind of tenſion, at the ſame time giving it a deep indigo colour, which was communicated to the cloud. At the ſame inſtant the colour of the whole water-ſpout became ſo deep that we could diſtinguiſh no motion in its expanded part. We obſerved only that the whole phenomenon moved from eaſt to weſt, and was deſtroyed on the coaſt of Provence. Laſtly, a fourth was formed, which was deſtroyed in the ſame manner, without any ſuch reproduction, beyond the hills of Antibes, as I obſerved in 1780, becauſe, their courſe being more oblique towards the north, they could not meet the gulph Jean, and the prolongation of their track was altogether over land. A fall of ſnow ſucceeded immediately afterwards, which was of the uſual denſity and configuration. It laſted all the reſt of the afternoon and the following night, ſo that on the following day there was as much ſnow on the ground as before. It afterwards rained for a long time, which cleared the country of the ſnow that had accumulated. As the impetuous wind of the preceding day continued with undiminiſhed force through the whole night, and the other acceſſary circumſtances were likewiſe preſent, I think there is reaſon to conclude that new water-ſpouts muſt have been formed in the afternoon, and perhaps in the night of the 6th; but the obſcurity of the atmoſphere, from the fall of ſnow, did not permit me to obſerve them. I ſhall therefore proceed to make ſome remarks on the wind which cauſed this phenomenon.

Though the velocity of this wind was nearly equal to that of the greateſt ſtorms in our ſeas, the waves were not proportionally deep. Two circumſtances appeared to concur in producing this effect; the firſt, that by the form of our coaſt an eaſt wind cannot have paſſed over ſo great an extent of ſea as a wind from the ſouth-weſt, from which quarter our greateſt ſtorms come. This cauſe is conſtant with regard to our local ſituation. The other circumſtance was, that the wind did not blow obliquely downwards, but moved parallel to the ſurface of the ſea. This ſuppoſition, which is the only one that requires proof, was confirmed at the time of obſervation by the appearance of a ſmall Catalan veſſel, which the wind of the 6th of January blew aſhore near Nice. I ſaw her paſs before my windows, driven by a force ſhe was incapable of reſiſting. She did not labour much, but came to an anchor at a little diſtance, from which, however, the violence of the wind drove her on ſhore, though without conſiderable damage, ſince ſhe was got off a few days after, and purſued her voyage. The force of this wind was ſeen not only in the inſtance of this veſſel, but in a conſiderable number of others which were loſt on the neighbouring ſhores.

It is an obſervation very agreeable to the opinion of Profeſſor Toaldo, and at the ſame time well eſtabliſhed by every obſervation I have made ſince I directed my attention to the phenomena of water-ſpouts, from the beginning of 1789 to the 19th of March in the ſame year, on which day we obſerved thoſe which I ſhall preſently deſcribe: it is a confirmed obſervation, I ſay, that the phaſes of the moon are accompanied with a change of weather.

It is certain that the cold weather which ſuddenly came on with the new moon of the 27th of December, recurred again exactly at each new phaſis. I cannot be deceived in this reſpect, becauſe the chimney of my apartment ſmoked each time, and did not ſmoke but during this accidental cold. Two days afterwards, when the weather became milder, this chimney acted as uſual, and did not ſmoke again until the following change. I could urge many other circumſtances in favour of the opinion of Mr. Toaldo; but I forbear, becauſe they are foreign to my preſent ſubject.

Laſtly, on the 19th of March the wind, which had begun the preceding evening, blew with a degree of impetuoſity leſs than that of the 6th of January. The clouds were accumulated from the eaſt towards the weſt, but they were much leſs condenſed than at that time. At forty minutes after eleven in the morning, we obſerved two water-ſpouts, a, b, Plate XXV. fig. 4, which moved at the ſame time, the one after the other. The moſt remarkable circumſtances in theſe water-ſpouts were: 1. The prodigious enlargement of the protuberance d, fig. 4, from the extremity of which hung the effective ſpout b, which was incomparably thinner; but the wonder diſappears, when we reflect that the following ſpout, which maintained itſelf in the ſame ſtate as thoſe we had before obſerved, robbed the preceding one in ſome meaſure of its ſupport; ſo that this enlargement was, as it were, a commencement of diſſolution, and the thinneſs of b was a proof of the little intenſity of electric power then acting; a concluſion which is alſo confirmed by the following circumſtances. 2. The incapacity in the feet of theſe two water-ſpouts to elevate their ſurrounding plumes. It is ſeen, a b, fig. 4, that they were reverſed by a force which prevented their riſing in a perpendicular direction, like thoſe of the preceding water-ſpouts. At the extremities of the plumes here deſcribed, as well as at the centre of the circle near the ſurface of the ſea, there was formed a ſmall atmoſphere: but as it was not extenſive, the vapours were ſo few that we had very little ſnow, which continued for about half an hour; when the weather cleared up. During the tranſition of theſe water-ſpouts very diſtant thunder was heard five or ſix times.

In the interval between the obſervations of the 6th of January and the 19th of March, other water-ſpouts muſt have been formed on the coaſts of Provence. It is certain, at leaſt, that I ſaw the appendices projecting from the clouds, and that the product of frozen ſnow reached our firſt hills on this ſide of the Var; but as my proſpect, being limited by the mountains of Provence, did not allow me to ſee theſe water-ſpouts in ſuch a manner as to make any drawing or deſcription, I ſhall here conclude my obſervations, and attend to the reſults.

I don't know whether I am miſled by partiality for my own obſervations, when I expreſs my opinion that the facts noted by me on the 12th of April 1780 are of great value in natural philoſophy, as well becauſe of the vicinity of the water-ſpout which appeared that day, as of the tranſparency of the ſurrounding vapours of the foot, which exhibited the interior ebullition with ſcarcely any obſcurity. Whence it follows,

1. That there is a real ebullition in the ſea, at the place circumſcribed by the foot of the water-ſpout.

2. That the vapours of the water which muſt ariſe, are the product of an evaporation

which muſt ſeparate freſh water from the ſalt; it being aſcertained by experiment, that diſtillation is the only method by which ſea-water can be rendered completely freſh.

I ſhall take the liberty in this place to make uſe of a compariſon, by way of explaining more perfectly the phenomenon we at that time beheld. It will not probably be unacceptable to ſuch as have never ſeen a water-ſpout, at leaſt in ſo favourable a poſition. Suppoſe the chimney of a baker's oven, ſuch as we have at Nice, in which the fuel commonly burned conſiſts of branches of pine recently broken from the tree, and ſufficient to bake large quantities of bread; let this chimney be imagined to be in the ſtate of throwing out immenſe clouds of vapour and ſmoke. Suppoſe a funnel of glaſs to be adapted to the aperture at the lower part, and to have its diameter enlarged upwards, until it terminates in a very extended veſſel. It may eaſily be conceived that the aſcending vapours will be ſo preſſed together in the narrow neck of the glaſs, as to render it opake, without permitting the ſucceſſive undulations of the vapour to be diſcerned. But in proportion as they proceed further from the cauſe of motion, which is the fire of the oven, and arrive at a part of the glaſs where they have room to expand, the vapours, being leſs condenſed, will exhibit their peculiar motions, and the ſucceſſive eruptions of the ſmoke will indubitably be ſeen. In this imaginary experiment, let the foot be ſuppoſed to be abſtracted; let the aſcending vapour be ſimply that of boiling water, and the conical tube of glaſs here contemplated, will afford a natural repreſentation of the phenomenon obſerved in 1780, and confirmed on the 6th of January 1789.

It may perhaps be objected, that theſe facts do not at all agree with Profeſſor Muſchenbroek's theory of water-ſpouts, given in his Eſſai de Phyſique. This objection was ſtarted by all my philoſophical acquaintance againſt the report of our obſervation in 1780, and engaged me to ſuſpend the publication of my memoir for ſeveral years, becauſe it was not poſſible to reconcile the means employed by nature in the production of water-ſpouts, nor their uſe, with the ideas of the celebrated Dutch Profeſſor. Beſides which, having only one obſervation to offer, though well ſupported by the teſtimony of two reſpectable perſons, I flattered myſelf that I might remove all the difficulties by repreſenting a water-ſpout in the ſame manner as lightning and thunder are imitated by the electrical machine. For it ſeemed to me at that time, and ſtill appears to be, a thing very practicable. But at preſent, by a new ſeries of obſervations in confirmation of that of 1780, I ſee clearly that the proceſſes of nature are very different from thoſe pointed out by Profeſſor Muſchenbroek. That enlightened and moſt accurate obſerver had no opportunity of examining this phenomenon in a favourable poſition himſelf, and has been equally unfortunate in the explanation of the ſuppoſed deſcent of water, which really aſcends in water-ſpouts, as well as in the formation of the foot, which according to his theory is a maſs of ſea-water in its natural ſtate. I can affirm without fear of contradiction by experience, that this foot or atmoſphere as it may be called is nothing but the matter of clouds and miſts.

It muſt moreover be remarked, that in the time of Muſchenbroek the theory of electricity had made ſo little progreſs, that he did not avail himſelf of it in the account of fiery meteors. It is therefore leſs wonderful that he ſhould not have recurred to it in his theory of water-ſpouts.

What then is the agent, it may be aſked, which cauſes this ebullition in the ſea, and raiſes the vapours through the water-ſpout to the cloud? Simply to affirm that this agent is electricity, without further proof, is in fact to ſay nothing. To this queſtion I muſt anſwer, that I have exhibited the products of obſervation: I have related what I have moſt clearly ſeen. I think I can diſcern the cauſe without being able to exhibit proof: but I ſhall be happy to be anticipated by philoſophers of greater ſkill in this theory, and think it better to ſuſpend my judgment than yield to the ſeductive pleaſure of explaining every thing by adding to the maſs of error in natural philoſophy.

The ſecond fact which preſents itſelf in our obſervations is, that two cauſes unite in the formation of water-ſpouts, or rather two different modifications of the ſame cauſe. When the foot appears without the water-ſpout, it is not the productive cauſe, but rather an effervеſcence which prevails in the ſea at that place. But how many intereſting queſtions might be aſked reſpecting this part of the phenomenon! What cauſe is it ſo powerful as to retain the foot a, fig. 3, and keep it motionleſs, notwithſtanding an impetuous eaſterly wind, until the projection in the cloud which is to form the water-ſpout ſhall arrive directly over it? Was the apparent bag which developed itſelf from the cloud, pre-exiſtent in the projecting part? As I can make no ſatisfactory reply to theſe and other queſtions which might be propoſed, I ſhall proceed to the third remarkable fact.

3. When the foot of a water-ſpout begins to approach the earth, its diameter contracts, its height is diminiſhed, and its volume becomes leſs and leſs; ſo that the foot is reduced to nothing at the inſtant it touches the ſhore. From the attentive examination I have made, it has appeared that the foot even of the greateſt water-ſpouts began to diminiſh when the depth of the ſea beneath became leſs than the elevation of the foot itſelf above the ſurface. If this be true, as I think it is, it may be concluded that the effervеſcence which ſupplies the ſpout with water, and forms the ſurrounding vapours of the foot, extends itſelf in depth nearly as much as the foot itſelf riſes above the ſea, and that materials for the ſupply of vapour become defective in quantity in proportion to the ſhallowneſs of the water.

Explanation of the Drawing.

FIG. 1. Plate XXIV. repreſents the imperfect foot a of the water-ſpout ſeen on the 6th of January 1789, at five minutes after ten in the morning. On the left hand are ſeen the clouds which riſe towards the zenith, but ſtill conſiderably diſtant. This foot had plumes elevated nearly like ſails, and was driven towards the ſhore by the wind. In proportion as it came near the land it contracted, and was reduced into a column of miſt, which the wind overſet on the land the moment the ſupply of water was wanting.

Fig. 2 repreſents, letter a, the enormous water-ſpout obſerved on the ſame day at eight minutes before noon. Nothing could more nearly reſemble a ſhip of war on fire than this phenomenon, excepting that no flames appeared. I have endeavoured to ſhew the continual jets of its ſurrounding vapour, and of the water which iſſued from the centre. At b are ſeen the remains of a water-ſpout after it has been deſtroyed by the foot having touched the ground.

Fig. 3. Plate XXV. a repreſents the foot of the ſecond water-ſpout ready formed, which was probably the third. It has yet no ſpout. At b is ſeen a protuberance tending obliquely towards the eaſt, and advancing to the weſt with the cloud to which it is ſuſpended. At

the letter c it may be obſerved how the protuberance b having arrived over the foot a became vertical, and unrolled itſelf inſtantly in a kind of large bag, of the figure of an inverted cone, nearly tranſparent, like gauze. As ſoon as this bag compoſed of the matter of the cloud had developed itſelf, which occupied the time of three or four ſeconds, and had fixed its ſmall extremity in the bottom of the foot d, it became ſtraight, without folds. A vapour like that ſeen in 1780 immediately roſe up the tube, extended it in the form of a water-ſpout, deprived it of its tranſparency, gave it a deep indigo-colour, like that of the clouds, and at the ſame inſtant the foot and the ſpout were moved from eaſt to weſt, and followed the courſe which the impulſe of the wind gave to the clouds to which the ſpout was attached. This ſpout being deſtroyed, the following ſpout exhibited the ſame phenomena as the others. It muſt be remarked, that the diſtance of the protuberance b from the foot already formed at a could not be augmented in the figure; but this protuberance, when firſt obſerved, was at more than a league diſtant from a, which remained motionleſs till its arrival. It is probable that it may have arrived from a ſtill greater diſtance. It is alſo to be noted, that the ſpouts 2 and 3 were ſomewhat leſs in all their dimenſions than the firſt.

Fig. 4. exhibits two water-ſpouts, which were ſeen following each other on the 19th of March. The wind was leſs ſtrong than on the 6th of January, the ſea leſs agitated, the clouds leſs accumulated, and leſs deep in colour. The intenſity of the phenomenon was alſo proportionally leſs. It may be ſeen at a and b that the ſurrounding plumes of the foot had not the power to raiſe themſelves up, as in the preceding figures, but were kept down by the wind. The enlargement of the upper part of the ſpout d appeared to be a commencement of diſſolution, ſuch as was obſerved at b fig. 2. Theſe two water-ſpouts proceeded to the point of N. Dame de la Garde, beyond Antibes. Thoſe of the 6th of January reached the ſhore between the town of Antibes and the mouth of the Var. The line they deſcribed from the time we firſt ſaw them to that of reaching the ſhore may be eſtimated at five or ſix common leagues.

Advertiſement reſpecting the Figures.

THE magnitude of fig. 1. in theſe drawings muſt not be conſidered as proportional to thoſe of the figures 2, 3, 4. The firſt water-ſpout paſſed at no greater diſtance than a muſket-ſhot from our windows; the others were at the diſtance of two or three leagues. It is neceſſary to make allowance for what the firſt gained by its nearneſs, and the others loſt by their remote ſituation, in order to form a judgment of the reſpective ſize of each ſuppoſed to be at the ſame diſtance.

II.

Experiments and Obſervations on the Inflexion, Reflexion, and Colours of Light.

By HENRY BROUGHAM, *Jun. Eſq.*

[Concluded from page 563.]

HAVING endeavoured to unfold the property of flexibility, as varied in inflexion, deflexion and reflexion; and alſo the phyſical cauſe of this property; and having indulged in a ſpeculation depending on this cauſe, I flatter myſelf neither altogether uſeleſs nor unimportant; I haſten now to the natural phenomena, the explanation of which depends on the property whoſe exiſtence and nature we have juſt now been inveſtigating; and that we may treat this part of the ſubject with conciſeneſs and order, we ſhall rank the phenomena under a diviſion ſimilar to that under which we laid down the principles, beginning with thoſe appearances which are explicable on the principles of flexion.

1. It is obſervable, that when a body is expoſed in the ſun's light ſo as to caſt a ſhadow, and another body is approached to it, either between the ſun and it, or the ſhadow and it, or in the ſame line with it, the ſhadow of the one body comes out a conſiderable way, and meets that of the other. Now it is evident, that when the bodies are held at a ſufficient diſtance from one another, a penumbra is formed round the ſhadow of each, making it leſs than it ſhould be were there no inflexion; but when the bodies are brought ſo cloſe to one another that the edge of the one is within the ſphere of the other's inflexion, the light being already bent by this laſt, the former can have none to bend, and conſequently no penumbra in the part of the ſhadow correſponding to that part of the body which is within the other's ſphere of inflexion; and the reſt of the ſhadow having a penumbra, this part that has none will be larger than it, and increaſe as the bodies approach, till at laſt it meets the other ſhadow: the like appearance happening when the ſhadows are thrown on the eye. Mr. Melvill has endeavoured to ſhew that it belongs ſimply to a caſe of viſion[*]. However, we have now ſeen that it has no reference to the ſtructure or poſition of the eye, but only to the common nature of all ſhadows[†].

2. If we ſhut out all the light coming into a room from external objects, except what may paſs through a ſmall hole of $\frac{1}{2}$ or $\frac{1}{4}$th of an inch in diameter, the images of the external objects, as clouds, houſes, trees, will be painted on the oppoſite wall by the rays of light croſſing at the hole; but if a piece of rough glaſs or of very fine paper be held ſo as to cover it all over, the light does not paſs through; then if the paper be wetted with oil, or the glaſs with water, ſo as to give either a ſmall degree of tranſparency, the firſt rays that come through are thoſe from red and orange objects, and laſt from blue and violet. Now it is evident that tranſparency in general, and this particular fact, are explicable by what was before laid down. It was found by Newton, that a body tranſmits the light incident on it more or leſs according to the continuity of its particles; and that a ſtrong reflexion takes place on the confines of a vacuum[‡]: How does this happen? The initial velocity of light is ſufficient to carry it through the firſt ſurface or ſet of particles; but it is ſo much diminiſhed that it is reflected by the repulſive power of the back ſide of theſe particles, unleſs there be others behind at a certain diſtance, namely, that at which inflexion or attraction acts, that is, apparent contact: this attraction renews the impetus of light, and tranſmits it to another ſet, and ſo on. Now this action being ſtrongeſt on the largeſt and red particles, and weakeſt on the blue and violet, if the continuity be diminiſhed, the former will be tranſmitted, and not the latter; which is conformable to the experiment juſt now mentioned.

3. The doctrine of flexibility furniſhes an eaſy and ſatisfactory explanation of the different colours which are aſſumed by flame. Whether we ſuppoſe the light to come from the burning body or the oxygenous gas, the largeſt or red particles have the ſtrongeſt attrac-

---

[*] Edinburgh Literary Eſſays, Vol. II.

[†] See, however, Philoſ. Journ. I. 431.

[‡] Optics, Book ii. Part III. Prop. 3.

tion for bodies, the violet the weakeſt; when therefore the gas and the body combine, the precipitation of light muſt be in the reverſe order of the affinity between the particles of light and thoſe of the bodies. If then the combination take place ſlowly, the violet and-blue particles will be firſt emitted, and laſt of all the red: and this is conſiſtent with fact; for any inflammable body whatever, on being lighted, burns at firſt with a blue or violet flame, and afterwards has its flame of two or three diſtinct colours, blue, white, red, &c. as is ſeen remarkably in the caſe of a candle. Nay, I have obſerved in the flame of a blow-pipe, all the ſeven primary colours at once. When indeed a body is burnt in pure oxygenous gas, the combination is ſo rapid, that white light alone is precipitated undecompoſed; but in common air, where the azotic gas impedes the combuſtion, the above phenomena are obvious.

4. A curious phenomenon has often ſurpriſed philoſophers, namely, blue ſhadows. Theſe I have obſerved at all times when the paper on which I received them was illuminated by the ſky and any other light; and the reaſon of them I take to be this: that the ſhadow made by one light is illuminated by the blue rays from the ſky; for I have often obſerved purple, and even reddiſh ones, when the ſky or clouds happened to be of thoſe colours; and this account of the matter is confirmed by an experiment. Having received the coloured ſpectrum made by a priſm with a large refracting angle, on a ſheet of rough white paper, and held above it another ſheet; I ſtopped all the rays that illuminated the firſt, except the blue, and violet, and red; and if I held a body between the blue and the ſecond paper, its ſhadow was red; and if I held a body between the red and the paper, its ſhadow was blue; and ſo of other colours. This I take to amount to a demonſtration of the thing[*].

5. Paſſing over other phenomena of leſs note, I come now to one that has divided opticians more than any other; I mean the coloured fringes that ſurround the ſhadows of bodies. I made ſeveral obſervations on theſe, which enable me to conclude that each fringe is an image of the luminous body; for, holding between my eye and a candle two knife-blades, as I approached the one to the other, the edge of the candle ſeemed multiplied, and ſoon became coloured, coming wholly away from the candle; and, as the knives approached ſtill nearer, became diſtinct dilated images, highly tinctured with the priſmatic colours; and juſt before the knives met, the candle, whoſe edges had been all along coloured with red and yellow, became much diſtended, till at laſt it was divided in the middle, one half ſeeming to be drawn away by each knife, and then it wholly diſappeared. I have obſerved three kinds of theſe images, two without, and one within the ſhadow: the firſt had its colours in the order from the ſhadow, red outermoſt, and violet innermoſt; the ſecond and third had the colours in the contrary order; but the ſecond was ſo very faint that I could never perceive it, unleſs when let fall on my eye. All this is eaſily explained by the different flexibility of the rays. In fig. 9, Plate XXIII. let AD be a body by which the rays SDT and S′D′T′ paſs, and let SD be within AD's ſphere of inflexion, and S′D′ within its ſphere of deflexion; then SD will be bent into DG: but becauſe of the different inflexibility of its parts, the red will be bent into DR, and the violet into DV, and the intermediate rays will fall between R and V; the whole forming an image RGV, ſeparated into the ſeven primary

---

[*] Since writing the above, I find the ſame explanation of the matter given by Mr. Melvill and ſome of the French academicians, particularly Meſſrs. Buffon and Biguelin; alſo Count Rumford (Philoſ. Journal I. 101); but I have thought fit to keep it in, on account of the experiment that occurred to me in illuſtration of it.

colours; and in like manner by the different deflexibility of the parts whereof S'D' conſiſts, an image without the ſhadow, as V'G'R', will be formed, ſimilar to VGR, R' being red, and V' violet, all which is both theory and experience; and the ſame explanation may be extended to the other caſes. Now in all theſe, the bending power ſtretching to a very ſmall definite diſtance, and being of different degrees of ſtrength at different diſtances from the body, ſeveral pencils or ſmall beams, paſſing through different parts of the ſpheres, will be acted upon by the power in its different ſtates of ſtrength, and each beam will be diſpoſed into an image in the way before deſcribed. Of theſe images I have ſometimes obſerved four, and even, by uſing great care, the faint lineaments of a fifth. In forming them, the power acts ſtrongeſt at the ſmalleſt diſtances, and of conſequence bends the mean flexible rays that paſs near, farther inwards or outwards than thoſe that paſs farther off; ſo that the extreme rays will in the former caſe be more ſeparated from the mean than in the latter; and the nearer image will always be the largeſt and moſt highly coloured; which is conſiſtent with fact. This explains fully the celebrated experiment of Sir Iſaac Newton with the knives, and the explanation is confirmed by the experiments which I related above on flexibility, where the bending force acted moſt ſtrongly on thoſe images formed out of red light, and leaſt ſtrongly on thoſe formed out of violet and blue light. A number of other phenomena are explicable on the ſame principles, being only particular caſes, as it were, of the coloured fringes or images: I ſhall here mention a few of the moſt remarkable.

6. When making ſome of the experiments which I have related in the courſe of this paper, I obſerved, that when the ſun was ſurrounded, but not covered, by clear white clouds, the white image on the chart (the hole being 1½ inch in diameter) was ſurrounded by two rainbows pretty broad and bright; in the colours were red on the outſide, and violet next the white of the image. Theſe bows muſt not be confounded with one which ſometimes appears wholly of a dull red and yellow, when the ſun or moon ſhines through a cloud, and which is owing to the direct tranſmiſſion of the red rays, and reflexion of the others; for not only are the colours different in ſpecies, in brightneſs, and in number, in the phenomena under diſcuſſion, but likewiſe they are formed by the hole in the window, as I knew by altering its ſhape into an oblong; and the colours now were not diſpoſed in circles, but in broad lines of the ſame breadth as the bows had been, running along the ſhadow of the hole's ſides, and in the ſame poſition of colours as before. It is evident that their cauſe is the inflexion of the light which comes from the clouds by the ſides of the hole (for if the ſky have no clouds the colours do not appear) which ſeparate the white light into the parts of which it is compoſed.

7. It is obſervable, that when we look at any luminous body at a diſtance greater than one or two feet, its flame appears ſurrounded by two bows of faint colours, the innermoſt of them terminating in a white which continues to the flame; and the colours are red outermoſt, and green and blue innermoſt: the appearance is moſt remarkable if we look at a ſmall hole in the window-ſhut, the room being otherwiſe dark; and if the eye be preſſed upon, and then opened, the colours are more lively than before, as Deſcartes obſerved [*]; from which both he and Newton concluded that the appearance was owing entirely to wrinkles formed on the ſurface of the eye by the preſſure [†]. But this could neither form

---

[*] De Meteoribus.

[†] Lect. Opticæ. Sect. III. ad finem.

the bows with the regularity in which they always appear, nor could the colours be in the order above mentioned from the different refrangibility of the rays: it will alſo be obvious to any one who tries the thing, that the preſſure only increaſes the brightneſs and breadth of the bows, but does not form them. The true ſolution of the difficulty ſeems to be this: The rays which enter the pupil are inflected in their paſſage through the fibres which extend over the cornea, and which are very minute, but opake; by theſe they are decompounded into fringes, having the red outermoſt and the violet innermoſt; and the fringes formed by each fibre, being joined together, form the bow. How then does the preſſure enlarge and vivify them? The fibres are naturally extended over the ſurface of a ſpherical ſegment: when this ſurface is compreſſed into a plane circle, they are condenſed into a much leſs ſpace, and conſequently brought nearer to one another; the rays are therefore more inflected and ſeparated than before. If this explanation be true, it will follow that the like bows may be produced by ſmall hairs like fibres placed near one another; and this I found perfectly conſiſtent with fact: the bows are in this caſe brighter than in the other; and the ſmall hairs on a hat or the hand made them brighter than any other I have tried. A circumſtance which I obſerved in both caſes ſeems to ſhew clearly the identity of the cauſes; the white ſpace which reached from the interior bow to the flame, was ſpeckled or mottled in a manner which cannot be eaſily deſcribed, but which any one will perceive upon trying the experiment.

8. The laſt of theſe phenomena which I ſhall mention is the celebrated one obſerved by Sir Iſaac Newton, namely the rings of colours with which the focus of a concave glaſs mirror is ſurrounded. Sir Iſaac made ſeveral moſt ingenious and accurate experiments to inveſtigate their nature[*]; and finding their breadth to be in the inverſe ſubduplicate ratio of the mirror's thickneſs, he concluded that they were of the ſame nature and original with thoſe of thin plates deſcribed by him[†]. The Duc de Chaulnes purſued theſe experiments with conſiderable ſucceſs: he found that the rings were brighter, the nearer to the perpendicular the rays were incident; and that if, inſtead of a concave glaſs mirror, a metal one was uſed, with a ſmall piece of fine cambric, or reticulated ſilver wire, ſtretched before it, the colours were no longer diſpoſed in rings, but in ſtreaks of the ſame ſhape with the intervals between the threads: hence he concludes that they are owing to inflexion; that in paſſing through the firſt ſurface they are inflected, and condenſed by the ſecond[‡]. I am not, I own, quite ſatisfied with this account of the matter: that they are produced by inflexion the Duke's experiments put beyond doubt; but that they ſhould be formed in paſſing through the firſt ſurface, and reflected by the ſecond, is quite inconſiſtent with the ratio obſerved by their breadth, this being greater in the thinneſt glaſs, and alſo with the order of the colours. Beſides, all the coloured images, which fall on the back ſide of the mirror, will be (by what we before found, when ſpeaking of flexibility [§]) reflected into a white focus; ſo that, upon the whole, there appears every reaſon to believe that the rings are formed by the firſt ſurface, out of the light which, after reflexion from the ſecond ſurface, is ſcattered, and paſſes on to the chart. It will follow, 1ſt, that a plane mirror makes them not; for the regularly reflected light, not being thrown to a focus, mixes with the

---

[*] Optics, Book ii. Part IV.

[†] Book ii. Parts I. and II.

[‡] Mem. de l'Academie pour l'Année 1755.

[§] Part II. Obſ. 6. of this paper.

decompounded ſcattered light, and dilutes it. 2d, That the nearer to the perpendicular the rays are incident, the more light will be reflected to the focus, and conſequently the leſs will dilute and weaken the rings. 3d, That the thinner the mirror is, or the nearer the two ſurfaces are, the broader will the rings be. 4th, That the rings farther from the focus will be broader. And laſtly, that when homogeneous light is reflected the fringes or images will be larger, and farther from one another in red than in any other primary colour. All which is perfectly conſiſtent with the experiments of Newton and Chaulnes. There is only one difficulty that may be ſtarted to this explanation: How happens it that the colours (made by the mirror) are always circular? We anſwer, It is owing to the manner of poliſhing the concave mirror, which is laid between a convex and concave plate, and then turned round (with putty, or melted pitch) in the very direction in which the rings are. If it ſhould be aſked, Why does the thickneſs of the mirror influence the breadth of the rings exactly in the inverſe ſubduplicate ratio? we anſwer, That to a certain diſtance from the point of incidence (and the rays are never ſcattered far from it) this is demonſtrable to hold as a property of mathematical lines in general.

Having found that the fringes by flexion are images of the luminous body [*], I thought that from this conſideration a method of determining the different degrees of flexibility of the different rays might be deduced, ſimilar to that which I had formerly uſed for determining their degrees of reflexibility [†]. I therefore made the following experiment:

Obſ. 12.[‡] Having let into my darkened chamber a ſtrong beam of the ſun's light, through a hole $\frac{1}{40}$th of an inch in diameter, I held a hair at four feet from the hole; and receiving the ſhadow at two feet from the hair, I drew a line acroſs the middle of the coloured images, and pointed off in each the diviſions of the colours, as nearly as I could obſerve; and repeating the obſervation ſeveral times, and at different diſtances, I found, by the ſame way I had formerly done in my experiment on reflexibility, that the axis, or line drawn through the middle of each, was divided inverſely according to the intervals of the chords which ſound the notes in an octave, *ut*, *re*, *mi*, *ſol*, *la*, *fa*, *ſi*, *ut*. But as the meaſures in theſe experiments were very minute, and the operations of conſequence liable to inaccuracy, I thought proper to try the thing by another teſt.

Obſ. 13. The ſun ſhining into the room, as before, I placed at the hole an hollow priſm made of fine plate glaſs and filled with pure water, its refracting angle being 55 degrees. The ſpectrum was thrown on an horizontal chart eight feet from the window; and at four feet from the priſm there was placed in the rays a rough black pin $\frac{1}{20}$th of an inch in diameter. The ſhadow in the ſpectrum was bounded by hyperbolic ſides, as before deſcribed; and drawing a line, which might be the axis of the ſhadow, and paſs preciſely through its middle, I marked on one ſide ſix or eight points of the ſhadow's outline in each ſet of rays; and this being often repeated at different diſtances and in different ſhadows, the poſition of the axis remaining the ſame, the curves formed by joining the points were all parallel; which ſhows that each ſine of inflexion taken apart has a given ratio to the ſine of incidence. I afterwards divided the axis according to the muſical intervals, and thus found where each colour of the ſpectrum had terminated, in what colour each part of the ſhadows had been, and by what rays formed. Then I joined the parts that I had marked, and obtained a curve, which I took to be, either nearly or accurately, an hyperbola of the fourth order. I next meaſured the ordinates (the axis of the ſpectrum and ſhadow being the axis of the curve) at the confines of each colour; firſt, the ordinate at the extremity of the rectilinear red, then that at the confine of the red and orange, and ſo on to that at the extreme rectilinear violet. To each of theſe ordinates I added the greateſt one, or that in the violet, which (in fig. 10) is VV′; that is, I produced vV to V′, ſo that vV′ is equal to vV; and through V′ I drew V′R′ parallel to the axis VR, and produced gG to G′, and rR to R′; then from V′ I ſet off V′g′ equal to G′g, and V′r′ equal to R′r, and the other ordinates in like manner; and I found, according to the method before deſcribed*, that VV′ was divided inverſely, after the manner of the muſical intervals. It is therefore evident that the inflexibilities of the rays are directly as their deflexibilities and reflexibilities, but inverſely as their refrangibilities. The ſame may be proved by meaſuring and dividing the images made in the inſide of the ſhadows. Theſe I have found to be at equal incidences and diſtances, equal to the images on the outſide, both in breadth, in diſtance from the edge of the ſhadow, and in the relation which their diviſions bear to one another; wherefore whatever be the ratio of the angle of inflexion to that of incidence, the ſame is the ratio of the angle of deflexion to that of incidence; ſo that the angle of deflexion is equal to the angle of inflexion. If farther proof of this propoſition be deſired, the following experiment and obſervations, which from the importance of the thing I do not ſcruple to add, may be ſufficient.

---

[*] Page 588.

[†] Page 560.

[‡] So numbered in the original. N.

Obſ. 14. When two knife blades were placed by one another in a beam of light which entered the dark room, ſo that the one might form and the other diſtend the images, I made in one of the blades (with a file) a ſmall dent, which, on the chart, caſt an elliptic or ſemicircular outline; then I obſerved that the images of both blades were diſturbed by it, and wound round the edges of the ſemicircle; and they were all affected in preciſely the ſame manner and degree. So then the firſt knife deflected the images formed by the ſecond in preciſely the ſame degree that it inflected thoſe images which itſelf formed, and ſo of the other knife; otherwiſe the effect of the dent would have been different upon the two ſets of images. We may therefore conclude that the angles, or ſines of inflexion and deflexion, bear the ſame ratio to the angle or ſine of incidence, and that they are equal to one another. My next object was to determine this ratio in one of theſe caſes, and conſequently in both; and it was very agreeable to find data for the ſolution of this problem in Newton's meaſurement of the images and ſhadow; ſince this philoſopher's well-known accuracy in ſuch matters, beſides the ſingular ingenuity of the methods he employed, made me more ſatisfied with theſe than any experiment I could make on the ſubject. In fig. 11. CS is the line perpendicular to the chart SU, and paſſing through the centre of the body, whoſe half is CD or SE. EB is parallel to CS, and AI a ray incident at D. ADB or EDI is the angle of incidence, EDR that of the red's deflexion, EDV that of the violet's, and EDG that of the intermediates. According to Newton †, CD was $\frac{1}{350}$th of an inch, DE ſix inches, SI $\frac{1}{108}$th of an inch, RV $\frac{1}{170}$th, and conſequently RG $\frac{1}{340}$th; GS was $\frac{1}{75}$th; whence the angles IDE, EDV, EDG, and EDR, will be found to be 4′, 30″; 5′; 7′, and

---

* Page 560.

† Optics, Book III. Obſ. 3.

9′, reſpectively. Now the natural ſines of 4′, 30″; 5′; 7′ and 9′, are as the numbers 1309, 1454, $2035\frac{1}{2}$, and 2617, which are as the ſines of incidence, deflexion, and inflexion, of the violet, green, and red. Thus the angles of flexion of the extreme and mean rays being given, thoſe of the other rays are found by dividing the difference between 1454 and 2617 in the harmonical ratio; for then the red will be equal to $145\frac{3}{8}$, the orange $87\frac{9}{40}$, the yellow $155\frac{1}{15}$, the green $193\frac{5}{6}$, the blue $193\frac{5}{6}$, the indigo $129\frac{2}{9}$, and the violet $258\frac{4}{9}$; and by adding to the number 1454 the violet, and to their ſum the indigo, and ſo on, we get the flexibility of the red from 2617 to $2471\frac{5}{8}$, of the orange from $2471\frac{5}{8}$ to $2384\frac{2}{5}$, of the yellow from $2384\frac{2}{5}$ to $2229\frac{1}{3}$, of the green from $2229\frac{1}{3}$ to $2035\frac{1}{2}$, of the blue from $2035\frac{1}{2}$ to $1841\frac{2}{3}$, of the indigo from $1841\frac{2}{3}$ to $1712\frac{4}{9}$, and of the violet from $1712\frac{4}{9}$ to 1454, the common ſine of incidence being 1309. It is therefore evident that the flexibility of the red is not to that of the violet as the refrangibility of the violet to that of the red; and a little attention will convince us that we had no reaſon to expect the analogy ſhould be kept up in this reſpect; for the refrangibility of the rays depends on the ſpecies of the refracting medium, and follows no general rule; whereas our calculation has been made concerning the action of the bending power at a certain diſtance, greater than that whereat the particles of media act on the rays in refracting them. It was obſerved in the mathematical propoſitions prefixed to this paper, that the angle of flexion is leſs than that of incidence, when in the caſe of inflexion the angle made by the ray and the body is acute, and when in the caſe of deflexion that angle is obtuſe; and when the ray is perpendicular, or parallel, the angle of incidence vaniſhes in both caſes. It is evident therefore, that in both theſe ſituations of things the ratio of 1309 to 2036 being that of a leſs to a greater, will not enable us to find the angle of flexion, although it ſerves very well when the ray before inflexion makes an obtuſe, and before the deflexion an acute angle. I have therefore mentioned the angle made by the bent ray with the incident, which gives a general formula; for let the angle of incidence be I, and that which the bent ray makes with the incident B, then F being the angle of flexion, we have $F = B \pm I$; ſo that if $I = O$, $F = B$, or if the incident makes an obtuſe angle with the body in the caſe of deflexion, and an acute in that of inflexion, then $F = I - B$, and in the remaining caſe $F = I + B$.

Theſe obſervations enable us to give a very ſhort ſummary of optical ſcience. When particles of light paſs at a certain diſtance from any body, a repulſive power drives them off; at a diſtance a little leſs this power becomes attractive: at a ſtill leſs diſtance it again becomes repulſive; and at the leaſt diſtance it becomes attractive, as before, always acting in the ſame direction. Theſe things hold, whatever be the direction of the particles; but if, when produced, it paſſes through the body, then the neareſt repulſive force drives the particles back, and the neareſt attractive force either tranſmits them or turns them out of their courſe during tranſmiſſion. Farther, the particles differ in their diſpoſitions to be acted upon by this power in all theſe varieties of exertion; and thoſe which are moſt ſtrongly affected by its exertion in one caſe, are alſo moſt ſtrongly affected by that exertion when varied, except in the caſes of refraction, of which we before ſpoke; and theſe diſpoſitions of the parts are in all the caſes in the ſame harmonical ratio. Laſtly, the cauſe of theſe different diſpoſitions is the magnitude of the particles being various.

All that remains now to be done on this part of the ſubject, is to explain one or two phenomena relating to reflexibility.

1. It has been remarked, that if we look at a candle, or other luminous body, with our eyes almoſt ſhut, bright ſtreaks ſeem to dart upwards and downwards from it. Newton[*] explains this by refraction through the humours adhering to the eyelids; Rohault[†] and Mr. Young[‡] aſcribe them to reflexions; Deſcartes makes them ariſe from wrinkles on the eye's ſurface; De la Hire from refraction through the moiſture on the eyelids, as through a concave lens; and Prieſtley[‖] from inflexion through the laſhes. The truth of Sir Iſaac's obſervation is obvious, becauſe the ſtreaks which dart from the top of the luminous body are formed by the under eyelid, or at leaſt by the moiſture adhering to the under ciliary proceſs, and thoſe which appear from the bottom of the body by the upper eyelid; which could not be, either if they were formed by reflexion from the proceſſes, or by inflexion through the laſhes.

I have however obſerved another kind of ſtreaks, mottled with broken colours of all kinds, and formed by reflexion from the moiſture on the proceſſes. In theſe the under ſtreak correſponds to the under proceſs, and *vice verſa*. They may be formed by any poliſhed body held in the proper poſition between the pupil and luminous body. The colours are very beautiful when made by the ſun, and reſemble, in form and irregularity of arrangement, ſome of the ſtreaks made by large half-poliſhed bodies, as deſcribed in Part II. of this paper.

2. The next object of attention is one of the greateſt importance to our theory, namely, the formation of images by reflexion. Three things here require explanation: the number of the images, their colours, and their variations in point of ſize.

Obſ. 15. I have uniformly found that no reflecting ſurface forms them except it be curve, and (its ſurface) of a ſtructure ſomewhat fibrous. A plain mirror, nor a concave, nor a convex one, do not make them, unleſs they are of that ſtructure; and for the ſame reaſon quickſilver, when held ſo as to reflect the light incident upon it, forms them not; but by triturating it, ſo as to divide it into ſmall particles, and by placing theſe in the beam of the ſun's light, each particle formed an image with the colours in the regular order, and very bright. On holding a cylinder in the rays, and obſerving the lengths of the images, I found that if the curvature was increaſed, the images were alſo increaſed in ſize, being more diſtended and highly coloured. Theſe things immediately ſuggeſt the explanation. Each of the ſmall fibres forms an image, which, from the different reflexibility of the rays, is divided into the ſeven primary colours. But why does not a plain mirror form one of theſe upon the ſame principles? In fig. 12. let A E be the curve ſurface of a very convex mirror, that is of a ſmall fibre, GC a ray reflected by the ſmall ſurface DC. It will be ſeparated into CI red, and CK violet, by the unequal action of FC on its parts. But if DC is continued to L in a ſtraight line, then LC's ſphere of reflexion extending a little way beyond it to KC, the part neareſt to C, and not to IC, will drive KC, and alſo the indigo and part of the blue, nearer to the perpendicular; then IC being within LC's ſphere of inflexion, will, together with the orange, yellow, and part of the green, be brought nearer to KC; ſo that IC and KC will both be brought to an angle equal to that of incidence, and will be reflected in a parallel white beam. If LC is removed a little, or the ſurface becomes more convex,

---

[*] Lect. Opt. Sect. III. ad finem.

[†] Phyſica, p. 249. Clark's edit.

[‡] Phil. Tranſ. 1793.

[‖] On Viſion, vol. ii.

IC is attracted, and KC repelled, but not ſo much as to reduce them to paralleliſm and whiteneſs; an image being formed narrower, and leſs coloured, than when LC is moved ſo far round that KC is attracted, and IC deflected or repelled. If LC is moved round, ſo that the mirror is concave, then KC is repelled and IC attracted, as before, unleſs the curvature be conſiderable; and then KC and IC are both repelled, and an image formed in the *cauſtic* by reflexion. In *Obſ.* 3. we found that certain irregularities in the ſurface of the reflector cauſed the images to be in the inverted order of colours. How does this happen? In fig. 13. let *gf*, *fe*, *er*, *ri*, and *ih*, repreſent the ſections of the convex fibres on the ſurface of the reflector, and let the ray AB be reflected from *ef*, ſeparated into B*r* red and B*v* violet; then if AB was ſo inclined to *ef*, that B*r* and B*v* fell upon *er*, the ſide of the fibre next to *ef*, and a little larger than *ef*, it is evident that B*v* will be reflected into *v*V, and B*r* into *r*R, and an image VR will be formed, having the violet outermoſt, and the red innermoſt, the intermediate colours being in their order from V to R. Laſtly, it is evident that the greater the angle of incidence is, the longer will be the image, and the farther ſeparated its colours; for which reaſon the farther the images are from the ſhadow, the leſs dilated and coloured will they be. Nor will they have the ſame appearance at all diſtances from the point of incidence. Very near it they will be all in the form of fringes acroſs the ſtreak, the breadth being greater than the length (if I may uſe the expreſſion); but as we recede from it they will become diſtended, as before deſcribed, the length increaſing faſter than the breadth, and at one point or diſtance they will be juſt as long as broad; all which agrees with experiment. And it is needleſs to ſhow by particular demonſtration, the manner in which one image is divided from another, the reaſon obviouſly being the manner in which the fibres on the reflecting ſurface are arranged and inclined to one another.

3. A number of phenomena involved in that of the images are explicable by what has been ſaid on them. If a piece of metal be ſcratched, and then expoſed in the ſunſhine, a number of broken colours will be formed by the ſcratches, as may be ſeen either by letting them fall on the eye, or by receiving them on a white object. This is evidently owing to the different reflexibility of the rays incident on the ſcratches, which are ſo many irregular ſpecula, of great curvature; the images are therefore diſtorted and broken, juſt as a candle, &c. appears broken and coloured when viewed through a piece of irregular cryſtal, ſuch as the bottom of a wine glaſs. If we look attentively at any object expoſed in the light of the ſun, provided it be not poliſhed, we ſhall ſee its ſurface mottled with various points of colours from the ſpecular nature of its minute particles. If we look towards the ſun with a hat on our head, held down, ſo that the ſun's direct light may not fall on our eyes, but on the hairs of the hat, and be reflected, we ſhall ſee a variety of lively colours darting in all directions from thoſe hairs; and we may eaſily ſatisfy ourſelves that they are not the conſequence of flexion, by trying the ſame thing with unpoliſhed threads; in which caſe they do not appear, provided the threads be not very ſmall. In the ſame manner we may account for the colours of ſpider's webs, of different cloths which change their colours when their poſition is altered, and of ſome foſſils which appear of different ſtreaks of colours when held in the light; ſuch as the fire marble of Saxony, &c. All theſe bodies having ſurfaces of a fibrous ſtructure, each fibre reflects and decompounds the rays.

4. The conſideration of the foregoing phenomena inclined me to think that, upon the

principles which have been laid down, the colours of natural bodies may be explained. The celebrated diſcovery of Newton, that theſe depend on the thickneſs of their parts, is degraded by a compariſon with his hypotheſis of the fits of rays and waves of ether. Delighted and aſtoniſhed by the former, we gladly turn from the latter; and unwilling to involve in the ſmoke of unintelligible theory ſo fair a fabric, founded on ſtrict induction, we wiſh to find ſome continuation of experiments and obſervations, which may relieve us from the neceſſity of the ſuppoſition. My ſpeculations on this ſubject have by no means been completed, as I have not yet finiſhed the demonſtrations and experiments into which it has engaged me to enter; but in order to complete my plan I ſhall offer a few hints on the ſubject:—The parts of light are affirmed in Prop. III. Book I. Part I. of the Optics to be different in reflexibility; that is, according to the author's definition, in diſpoſition to be turned back, and not tranſmitted at the confines of two tranſparent media. That the demonſtration involves a logical error, appears pretty evident. When the rays, by refraction through the baſe of the priſm uſed in the experiment, are ſeparated into their parts, theſe become divergent, the violet and red emerging at very different angles, and theſe were alſo incident on the baſe, at different angles from the refraction of the ſide at which they entered: when therefore the priſm is moved round on its axis, as deſcribed in the propoſition, the baſe is neareſt the violet, from the poſition of the rays by refraction, and meets it firſt; ſo that the violet being reflected as ſoon as it meets the baſe, it is reflected before any of the other rays, not from a different diſpoſition to be ſo, but merely from its different refrangibility. Although then this experiment is a complete proof of the different refrangibility of the rays, it proves nothing elſe; and indeed an experiment will convince us that the rays all have the ſame diſpoſition to be reflected, provided the angle of incidence be the ſame. For I held a priſm vertically, and let the ſpectrum of another priſm be reflected by the baſe of the former, ſo that the rays had all the ſame angle of incidence; then turning round the vertical priſm on its axis when one ſort of rays was tranſmitted or reflected, all were tranſmitted or reflected. We cannot therefore apply the different reflexibility of light to the explanation of the colours of bodies, ſince this property has no exiſtence. But we have ſhewn that the rays differ in reflexibility, taking the word in the new ſenſe, as explained above. Let us ſee whether this principle will not ſolve the important problem. It is evident that the particles of bodies are ſpecular. Now, I take the colours of bodies to depend not on the ſize, but on the poſition, of theſe particles, or at leaſt on only the ſize in as far as it influences their poſition; an idea perfectly familiar to mathematicians.

Obſ. 16. In making ſome of the experiments which I related above, on the reflexibility of light, I obſerved, among the regular images made by moſt of the pins which I uſed, one or two all of the ſame colour, as red, blue, &c.; and when the pin was moved theſe moved alſo, becoming of other colours in regular order like the reſt; which ſhows plainly that their being of one colour at firſt was owing to ſome fibre in the ſurface jutting out, or rather to ſeveral of theſe, which ſtopped the red and all the reſt but the blue of ſeveral images, or the blue and all the reſt but the red. Farther, I produced ſeveral regular images by two or three very ſmall pins, and with conſiderable trouble. I at laſt contrived to place them in ſuch a poſition, as that one blue colour of conſiderable ſize might be produced, then a red, and ſo on, by altering the poſture of the pins. Now whether the poſture or the ſize be altered it matters not, for the one affects the other. Is it not evident that this experiment,

and

and the concluſion to which it evidently leads, may be transferred to the colours of natural bodies, as ſeen by reflexion? For, the parts being ſpecular and ſpherical, each will form an image of the luminous body; and by the poſition of the ſides of the neighbouring ones, any ſix of the colours may be ſtopped while the ſeventh emerges; and if this happens in one part it will happen in all, ſince, that the texture and ſize of the parts is the ſame throughout has never been called in queſtion. But it will be aſked, How are the particles to reflect a mixture of different colours? We anſwer, That a particle having its ſide concave and front convex will produce the effect; for the colours will be thus mixed in a proportion determined by the poſition of the others. How can whiteneſs and blackneſs be produced? If the particles be large, then the whole light incident on each will be reflected and ſeparated; and all the images being compounded and mixed together, a confuſed ſenſation, or a ſenſation of white, will be the reſult. For, the parts being tranſparent, and the images formed by the convex ſurface of the ſecond row of particles, theſe will be larger in proportion to the thickneſs of the particles or plates through which they have to paſs before they meet with obſtruction, and conſequently will not be ſtopped by other particles; and in like manner the colour will be red if the particles are a little leſs, and ſo on. If the particles be very ſmall, the light will be ſeparated into images alſo ſmall; with which and with one another the particles interfering, the light by many reflexions and obſtructions will be totally loſt. How do bodies appear of their proper colours, though no luminous body be ſhining whoſe image may be formed by a reflexion? They reflect images of the clouds which reflect the ſun's white light; for if we hold between our eye and a hole in the window illuminated by the light of the clouds a reflecting body, as a pin, &c. coloured images are formed of the hole diſtended, like thoſe of the ſun, as I have often found; and the ſame holds of inflexion. Why does cutting a body to pieces not alter its colours? This only changes the poſition of maſſes of particles, not of the particles themſelves; but if by bruiſing them we change their magnitude and poſition, we change alſo their colonr: thus the leaves of vegetables bruiſed in a mortar, many paints powdered, &c. Why do many bodies change colours when viewed in different poſitions? Becauſe they reflect two colours or more of each image to different quarters; and it matters not whether their poſition with reſpect to us, or our poſition with reſpect to them, be changed. How do bodies become coloured by tranſmitted light? Becauſe the foregoing reaſonings apply alſo to the flexion of the rays in their paſſage through the parts of bodies. Theſe obſervations appear to me to furniſh a very ſimple ſolution of the problem. I ſhall endeavour in a future communication to confirm what has been ſaid by other remarks and experiments; for it would be tedious, and perhaps ſuperfluous, to illuſtrate what has been ſaid by figures and demonſtrarions [*].

Purſuant to theſe remarks, it will not be difficult to account for the rings of colours of thin plates by reflexion, as we before did thoſe of thick plates by flexion [†]. Indeed, thoſe formed in the experiment of the two lenſes, ſuppoſed by Newton to be owing to the plates of air between them, appear to have a different cauſe, as may be without much reaſoning gathered from the curious experiments of the Abbé Mazeas *, and even from one or two of Sir Iſaac's own, in which he ſuppoſes ſome medium more ſubtle than air to be between the glaſſes †. But at preſent I forbear entering into the ſubject any farther. This paper has already been extended to a greater length than was at firſt intended. And I haſten to conclude by a ſhort ſummary of propoſitions, containing the principal things which have been demonſtrated in the courſe of it.

---

[*] It is obvious that the different refrangibility of the rays will not account for the bright and diſtiuct colour of bodies. If the refracting angle of a priſm be continually diminiſhed, till, for example, it is equal to one of a minute, the refraction will produce no ſenſible colour. Indeed, almoſt every piece of plane glaſs has its ſides in a ſmall degree inclined to one another, and yet no colours are formed. Much leſs then will refraction through the infinitely ſmaller parts of bodies produce ſeparation of the rays.

[†] Page 589.

Prop. I. The angles of inflexion and deflexion are equal at equal incidences.

Prop. II. The ſine of inflexion is to that of incidence in a given ratio (which is determined in the paper).

Prop. III. The ſun's light conſiſts of parts which differ in degree of inflexibility and deflexibility, thoſe which are moſt refrangible being leaſt flexible.

Prop. IV. The flexibilities of the rays are inverſely as their refrangibilities; and the ſpectrum by flexion is divided by the harmonical ratio, like the ſpectrum by refraction.

Prop. V. The angle of reflexion is not equal to that of incidence, except in particular (though common) combinations of circumſtances, and in the mean rays of the ſpectrum.

Prop. VI. The rays which are moſt refrangible are leaſt reflexible, or make the leaſt angle of reflexion.

Prop. VII. The reflexibilities of the different rays are inverſely as their refrangibilities, and the ſpectrum by reflexion is divided in the harmonical ratio, like that by refraction.

Prop. VIII. The ſines of reflexion of the different rays are in given ratios to thoſe of incidence (which are determined in the paper).

Prop. IX. The ratio of the ſizes of the different parts of light are found.

Prop. X. The colours of natural bodies are found to depend on the different reflexibilities of the rays, and ſometimes on their flexibilities.

Prop. XI. The rays of light are reflected, refracted, inflected, and deflected, by one and the ſame power variouſly exerted in different circumſtances.

III.

On certain Points of Nomenclature. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

IT appears to me, that the new name *tartarin*, propoſed to deſignate the ſubſtance commonly called vegetable alkali, is more objectionable than ſome of the number of names which are in uſe.

1. Becauſe the word *tartar* is familiarly known to always denote a ſalt of acid properties, which conſiſts of nearly three parts of tartareous acid, and one of vegetable alkali.

2. The word *tartareous acid*, which is already employed to denote the acid contained in tartar (and for which ſubſtance there is no other name), ſo nearly reſembles the word *tartarin* as to make this name an improper one for the alkali of tartar and of vegetables in general.

---

* Mem. de l'Academie pour l'Année 1738.

† Optics, Book ii. Part I. Obſ. x. and xi.

3. But a very ſmall proportion of alkali is obtained from tartar comparatively with that got from other ſources; and it ſeems at leaſt as improper to call it *tartarin* becauſe it is gotten from tartar, as it would be to call it fern alkali, wormwood alkali, beech alkali, &c.

I agree that the name *pot-aſh* is very objectionable, and I wiſh that another could be thought of that is appropriate, eſpecially becauſe this word is commonly underſtood in commerce and in the arts to denote the vegetable alkali in its ordinary mild but impure ſtate, as uſed in manufactories; in which ſtate the alkali is only partially ſaturated with carbonic acid, and which therefore ſhould be called, according to the new nomenclature, *ſub-carbonate of pot-aſh*; whereas, in the new ſyſtem, the name pot-aſh is intended to ſignify the alkali in its pure or cauſtic ſtate.

Conſidering, 1ſt. That the alkali called pot-aſh is very generally diffuſed through the whole vegetable kingdom, which neither of the other ſpecies of alkali are—2. That the other fixed alkali, commonly called foſſil alkali, or ſoda, exiſts principally in the mineral kingdom—3. That the volatile alkali or ammoniac is obtained almoſt entirely from the animal kingdom—4. That the denomination *vegetable alkali* is ſtill very generally employed, and has been ſo for a very long time—5. That the three alkaline ſalts reſemble one another in ſo many properties as to form a natural genus which may continue to be denominated *alkali*:—I ſay, from theſe conſiderations it ſeems beſt to retain the former ſpecific terms vegetable, foſſil, and volatile alkali; but abbreviated, as propoſed ſome years ago by Mr. Chriſtie, and accordingly to employ the names *vegalkali*, *foſkali*, and *volkali*, till more appoſite names be ſuggeſted.

Give me leave to remark, that Engliſh writers are not uniform in their orthography of ſeveral terms of the new ſyſtem, and I apprehend ſome of them write improperly; for inſtance:

Ox*i*gen and oxygen*e* for *oxygen*.
H*i*drogen and hidrogen*e* for *hydrogen*.
Ox*i*d and ox*y*de for *oxide*.
Sulph*at* and ſulfate for *ſulphate*.
Sulph*it* and ſulfite for *ſulphite*.
Muri*at*, &c. &c. &c. for *muriate*.
Calor*ique* for *caloric*.

We hear alſo ſometimes erroneous proſody, for inſtance *cālŏric* for *călōric*.

The French writers uniformly write gaz, and the Engliſh generally ga*s*. In the Engliſh, either mode may be employed; but the French probably prefer their orthography *gaz* on account of the word *gas* ſounding too much like another word in their language which excites an indelicate idea.

I am, Sir, your ſincere friend,

A VERY HUMBLE PHILOLOGER.

Feb. 17, 1798.

NEW

NEW PUBLICATIONS.

Eſſai ſur les Ouvrages Phyſico-mathématiques de Léonard de Vinci, avec de Fragmens tirés de ſes Manuſcrits apportés de l'Italie. Lu à la première Claſſe de l'Inſtitut National des Sciences et Arts. Par J. B. Venturi, Profeſſeur de Phyſique à Modène, de l'Inſtitut de Bologne, &c A Paris, chez Duprat, 1797.—Or, An Eſſay on the Phyſico-mathematical Works of Leonardo da Vinci, with Extracts from his Manuſcripts brought from Italy. Read before the Firſt Claſs of the National Inſtitute of Sciences and Arts. By J. B. Venturi, Profeſſor of Natural Philoſophy at Modena, Member of the Inſtitute at Bologna, &c. Quarto, 56 pages, with one plate.

AMONG the treaſures of ſcience and art which the French have lately brought from Italy, are thirteen manuſcript volumes written by Leonardo da Vinci, whoſe extraordinary powers as a painter, ſculptor, muſician, geometer, philoſopher, and engineer, are well known. Citizen Venturi, who reſided in France during the war in his own country, has obtained the communication of theſe manuſcripts, and has ſelected ſuch parts as appear deſerving of publication, which he purpoſes to print in three ſeparate and complete treatiſes, on mechanics, hydraulics, and optics. The fragments in the preſent work are for the moſt part diſtinct and ſeparate from thoſe principal matters, and are enriched with notes by the editor.

Leonardo was born in 1452. He was the natural ſon of a notary, whoſe family poſſeſſes at this day at Vinci in Tuſcany the ſituation of reſpectable mediocrity. Nature, which, as C. Venturi obſerves, is not actuated by conſiderations of birth, was prodigal of her gifts to this man. His perſon was beautiful; his diſpoſition animated and lively, and the powers of his mind wonderful. He applied to geometry, muſic, and painting, and in each of theſe purſuits he ſoon excelled his teachers. He was called to Milan to caſt an equeſtrian ſtatue in bronze, which Louis Sforza conſecrated to the memory of his father. He offered his ſervices to the Duke in every undertaking relating to military machines, water-works, ſculpture, mechanics, and painting, with a defiance to any one who ſhould pretend to excel him; a boaſt which indeed he had the means of ſupporting. When France took poſſeſſion of the Milaneſe at the end of the 15th century, he paſſed ſeveral years at Florence; not becauſe he was in any reſpect prejudiced againſt the French, as is reported; on the contrary, Louis the XIIth gave him a penſion, together with certain duties on the Milaneſe canals, where Leonardo was employed under the French government. While he was at Florence he ſelected two of the moſt beautiful ladies of the country, with the intention of drawing their portraits, and preſented them to Louis the XIIth. He left Milan, and repaired to Rome in 1513, after Sforza had again entered the Milaneſe; after which he went to France on the invitation of Francis I. where he died, as it is ſaid, in the arms of that prince.

I ſhall forbear at preſent to enter into any detail reſpecting the contents of this intereſting work, in which this great genius, who preceded Chancellor Bacon in the true method of philoſophiſing near a century, has the peculiar felicity of an editor and commentator, whoſe valuable annotations beſpeak him a complete maſter of the ſubjects of ſcience and reſearch he has undertaken to publiſh and elucidate. On a future occaſion I ſhall preſent my readers with ſome extracts of the moſt curious parts.

Phyſiology; or, An Attempt to explain the Functions and Laws of the Nervous Syſtem; the Contraction of Muſcular Fibres, and the conſtant and involuntary Actions of the Heart, the Stomach and Organs of Reſpiration, by Means of ſimple, univerſal, and unvarying Principles. To which are added, Obſervations on the intellectual Operations of the Brain; and on the Diverſity of Senſations; with Remarks on the Effects of Poiſons, and an Explanation of the Experiments of Galvani and others on Animal Electricity. By E. Peart, M.D. &c. 8vo. 327 pages. W. Miller, 1798.

I have juſt received this work; for which reaſon I can at preſent only announce its publication, and ſay that the author's explanation of the phenomena mentioned in the title appears to be founded upon the ſyſtem which he offered to the world in his Elementary Principles of Nature publiſhed in 1789.

Travels in the Two Sicilies, and ſome Parts of the Apennines. Tranſlated from the original Italian of the Abbé Lazzaro Spallanzani, Profeſſor Royal of Natural Hiſtory in the Univerſity of Pavia, F. R. S. &c. &c. In Four Volumes 8vo. 1500 pages, with 11 Plates. Robinſons, 1798.

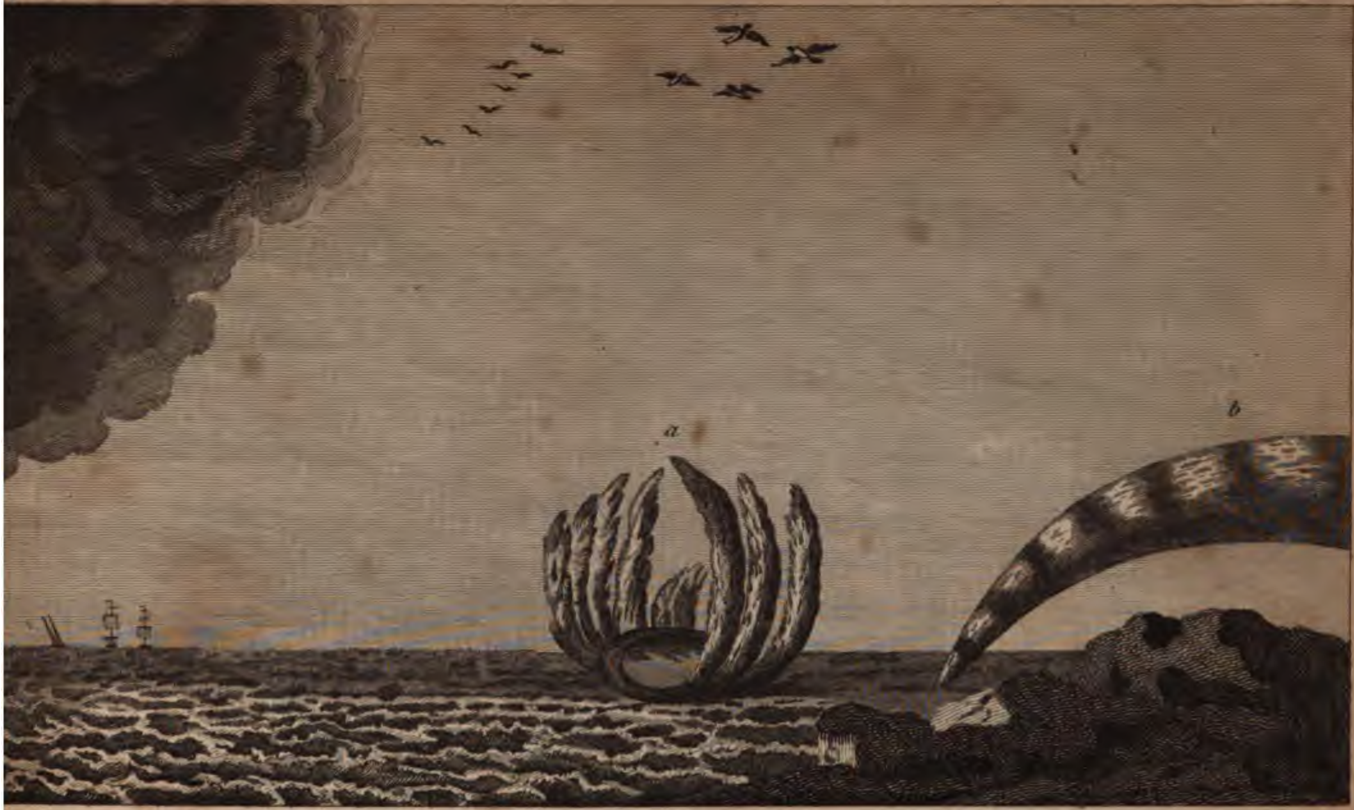
This celebrated philoſopher had the good fortune to obtain a clear and diſtinct view of three out of the four craters which are ſtill burning in the volcanized countries through which he travelled, namely, Etna, Stromboli, and Vulcano, of which he has given deſigns; but at Veſuvius his wiſh could not be gratified. The Lipari Iſlands have been the ſubject of his aſſiduous examination, as well with regard to their igneous productions, as the ſtate, character, and occupations of the inhabitants; and the environs of the unfortunate city of Meſſina afforded much information, from the variety of natural objects they preſented. Scylla and Charybdis; the fiſheries in the Straits of Meſſina for the ſword-fiſh, the ravenous ſhark, and for coral, with various other intereſting objects, alſo engaged his attention during his excurſion in the year 1788-89.

To ſay that theſe volumes contain a treaſure of geological and chemical information, is ſcarcely neceſſary to thoſe who are acquainted with the former labours of the Profeſſor. I muſt therefore more particularly obſerve, that he has traverſed theſe intereſting diſtricts, contemplated and compared the volcanic products with a particular regard to local circumſtances, and the inferences to which thoſe circumſtances lead; examined the ſeveral ſubſtances with every precaution which the difficulties of fixing the external characters and obvious properties could ſuggeſt; ſubmitted them to the action of fire before the blow-pipe, and in the furnace, with the pyrometer of Wedgwood and pneumatic apparatus; and laſtly, that in ſuch ſpecimens as could not from their external characters be referred to known earths already analyſed by chemiſts of reputation, he has had recourſe to the methods of humid analyſis. Where his experimental enquiries have proved conciſe, he has frequently incorporated them with the account of his journeys; but where otherwiſe, he has placed them with ſuch ſubjects of general diſcuſſion as could not form a part of his narrative in the order of time.

From this rapid ſketch of his method of proceeding, the reader, who may be acquainted with the advantages which the modern improvements of Chemiſtry muſt afford in the hands of an enlightened obſerver, will be able to form ſome eſtimate of the value of the work.

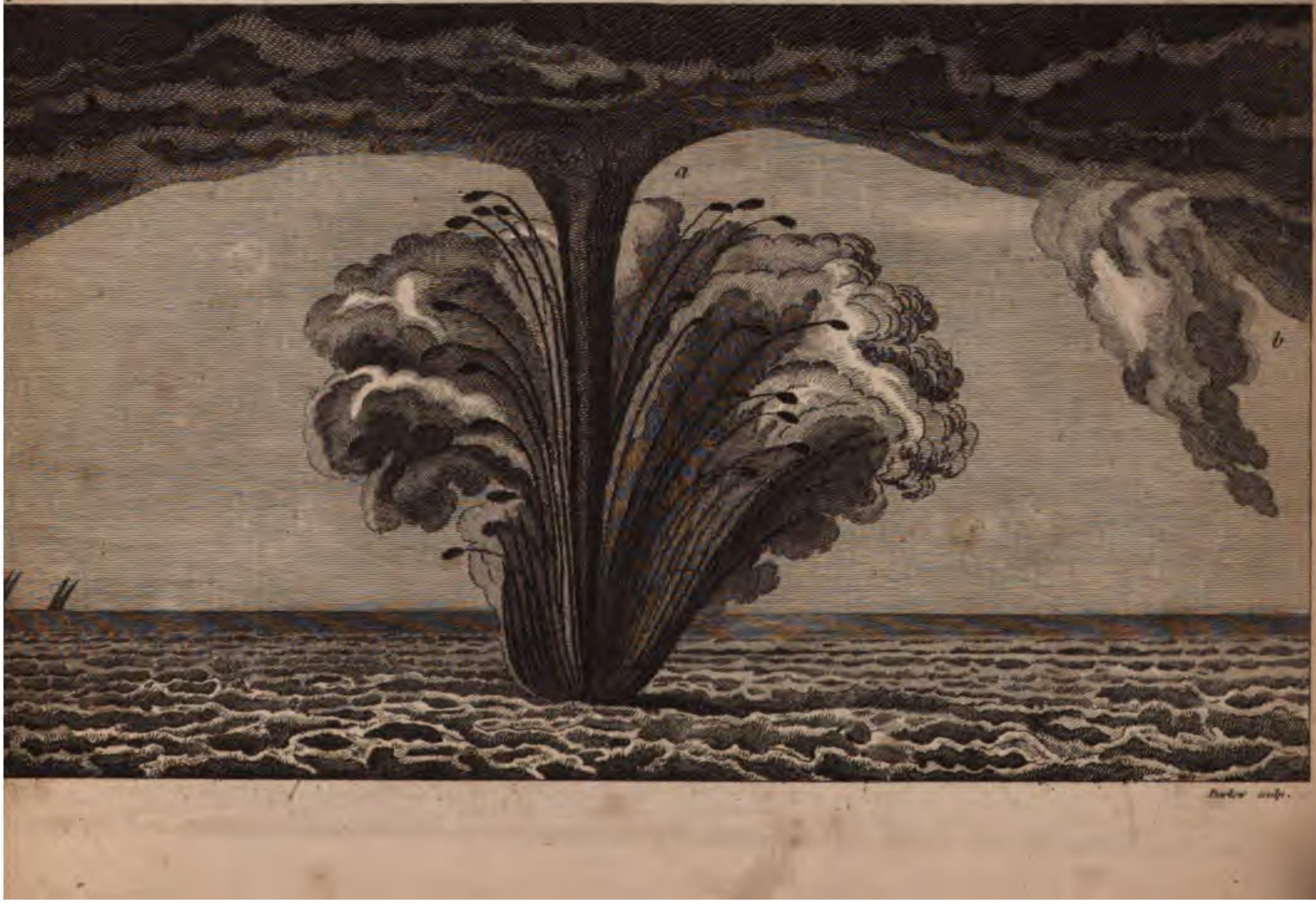
END OF THE FIRST VOLUME.

ig. 1.

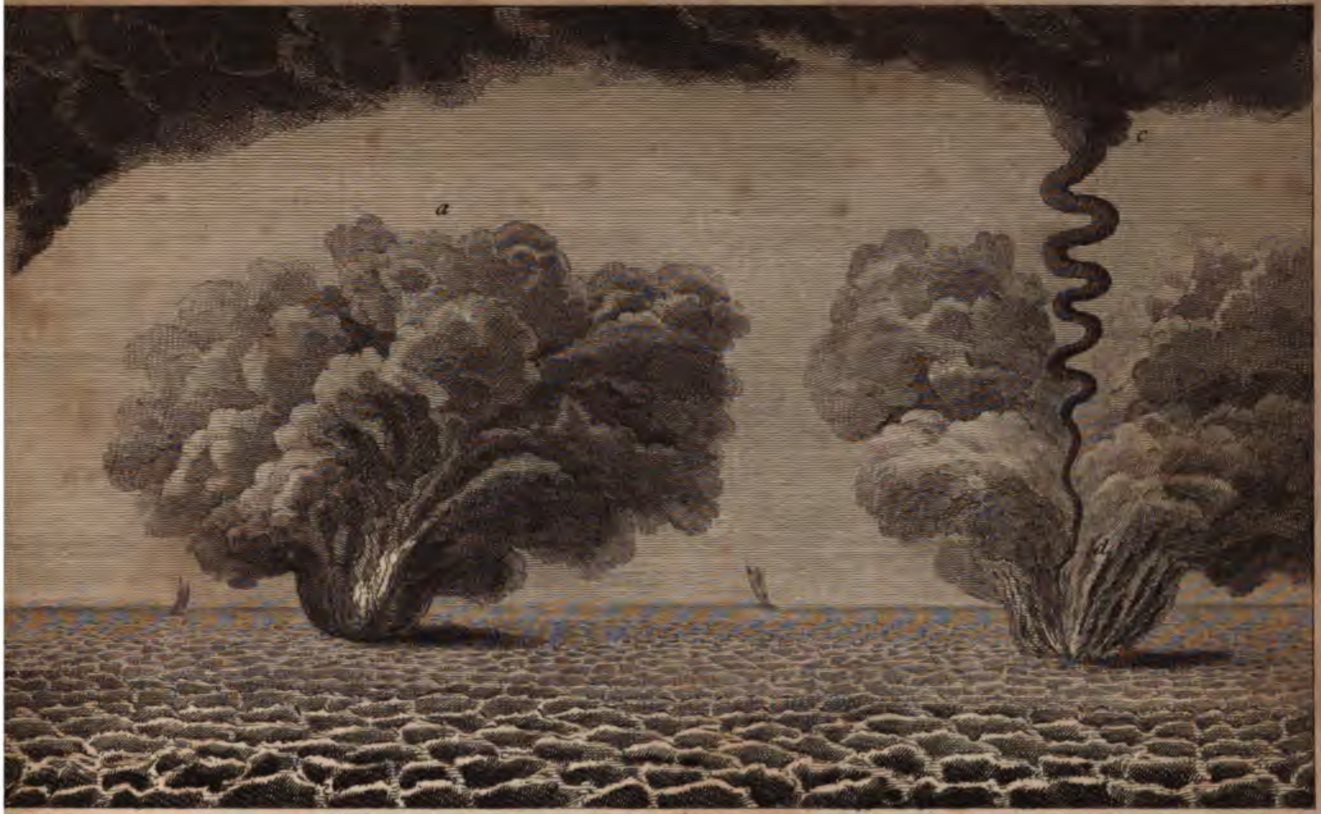


The Phenomena of Water Spouts.

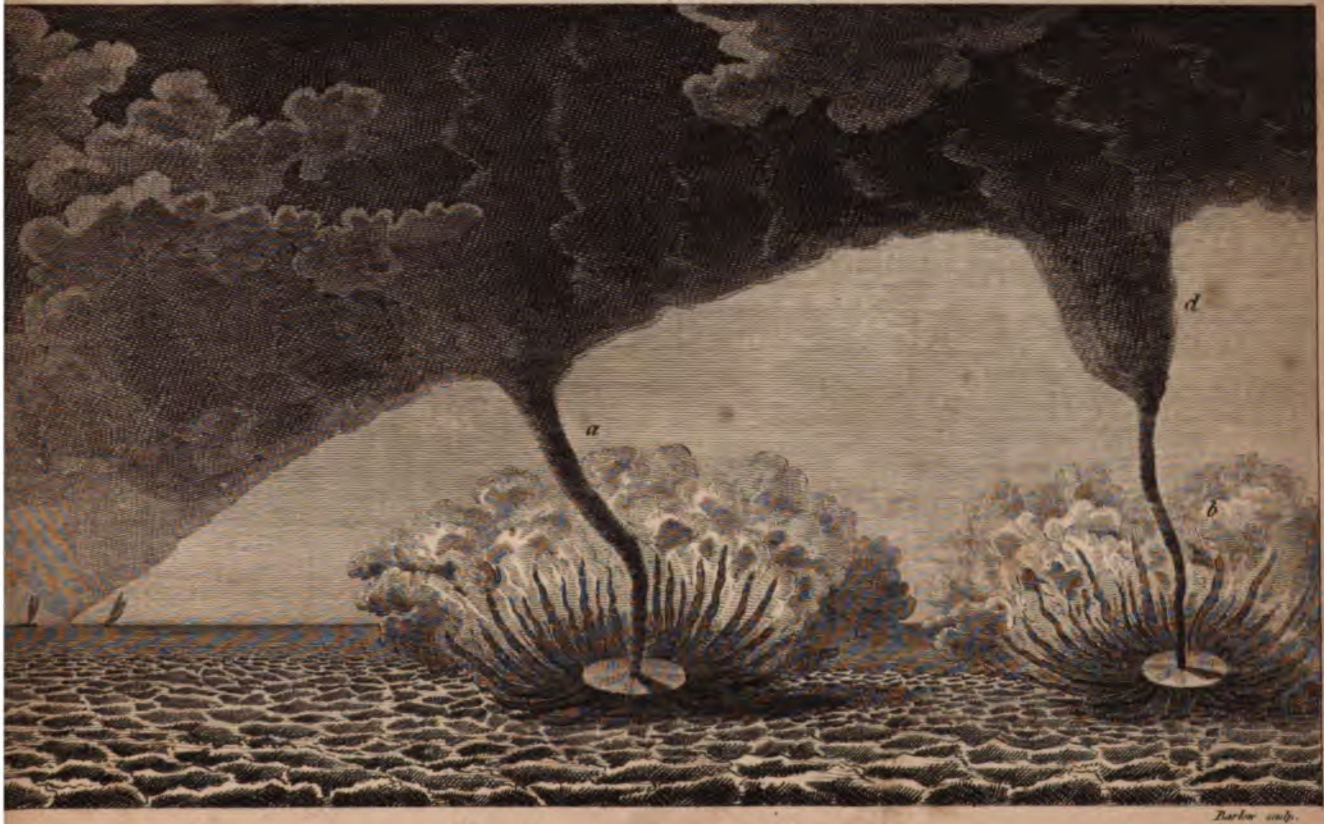
ig. 2.



Barlow sculp.



The Phenomena of Water Spouts.



Barlow sculp.

ADVERTISEMENT.

London, March 1, 1798.

A YEAR has nearly elapſed ſince this Journal firſt appeared. A complete Volume is now before the Public; on which occaſion it ſeems neceſſary to mention the nature of its contents. Enough has already been ſaid of the utility of ſuch a Work and the duties of the Editor; among which fidelity and accuracy are undoubtedly the chief. Whatever defects of ability may appear in the ſhare I have had in this collection, I can with confidence aſſert my claim to thoſe moral requiſites. I have deſcended to none of the arts of Book-makers. No commendatory letters have flowed from my pen: no imaginary congratulations are echoed: no pretended ſucceſs forms the ſubject of my acknowledgements. I have confided in the ſincere performance of my engagement with the Public; and have ſolicited the approbation of good men by ſuch means only as my heart could thoroughly approve. I truſt it will give pleaſure to many of my Readers to hear that I have not been diſappointed. The friendſhip and correſpondence of men whoſe talents and virtues I revere, men whoſe approval conſtitutes the only eſtimable part of fame, have amply overpaid my exertions: and in a commercial view, though I have found the ſale of my book unequal to what might have been expected in times of leſs general diſtreſs, yet it has been progreſſively increaſing, and ſufficient to encourage my perſeverance.

The copious Table of Contents will render it unneceſſary to recapitulate any of the excellent works I have received or collected. I have adhered to the principles of ſelection which are expreſſed in the Preface publiſhed laſt year, and have been more ſolicitous to offer productions of real merit and utility than ſuch as were chiefly remarkable for their novelty. It will be ſeen, however, that nearly half the papers in this volume are original and intereſting; that above a third conſiſts of new and important works which have never yet appeared in our language; and that the remaining part conſiſts either of digeſted reports and abridgments of excellent, but voluminous papers diſperſed in academical collections, or ſuch as from other circumſtances deſerved to be copied intire.

Men of information will hence perceive that this work is not an indiſcriminate compilation of things neareſt at hand, nor a looſe temporary record of tranſactions which a few years muſt render worthy of the oblivion they will experience. It is reaſonable to hope that it will every year become of more value, as the Repoſitory of Diſcoveries in Science and the Arts; and that it will tend to accelerate the progreſs of both.

ERRATA.

Page 13. l. 4. r. *other* engagements.
15. l. 27. r. as in the figure; theſe lines were parallel to the threads of the cloth.
17. l. 26. r. Inſtrument. And in the plate certain letters are omitted, which however may be eaſily ſupplied from the text.
64. l. 15. r. 0.0166.
66. l. 39. r. one-third *more.*
68. l. 10. r. Figure 11.
73. l. 15. r. Market.
87. l. 18. r. $\overline{31}|^{2}.7854-\overline{31-18}|^{2}.7854=522$.
107. l. 8. of the note, r. which *are* of the ſame nature.
124. l. 2. r. *be* divided the whole.
139. —— throughout the mathematical part, for ÷ r. *
144. l. 24. r. *Analyſes.*
174. bottom line, r. *eighteenth* Volume.

Page 186. l. 5. of the Mathematical queſtion, r. Then *D*'s chance.
207. l. 31. r. *meteorological* apparatus.
266. l. 41. dele *and repelled.*
304. l. 10. r. *or* the water.
313. l. 24. for *communication* r. *paper.*
381. to the note add 1, page 252.
382. l. 37. r. *Surfaces.*
399. l. 5. r. *as* it would have otherwiſe.
418. l. 22. r. *place* of percuſſion.
422. l. 36. read five *feet* wide.
432. l. 24. for ſine H × ſine h; or putting radius = unity, Coſ. a = Coſ. a, &c. read ſine H × ſine b × radius; or putting radius = unity, Coſ. A = coſ. a, &c.
—— l. 26. read Mr. William Caſtieau.

TABLE

TABLE OF CONTENTS

TO THIS FIRST VOLUME.

APRIL 1797.

I. THE Principles and Application of a New Method of conſtructing Achromatic Teleſcopes. By Robert Blair, M. D. — — page 1

Imperfections of achromatic lenſes—ariſe chiefly from the glaſs. Propoſed remedy by the uſe of fluids. Priſmatic apparatus for meaſuring the refractive and diſperſive qualities of fluids. Hadley's quadrant applied to meaſure the abſolute refractive denſities. Enumeration of the optical properties of various fluids. Conſtruction of a lens including a fluid. Obſervations. New and ſingular caſes of refrangibility—Explanation. Coloured ſpectra formed by different mediums are not divided into proportional intervals—Conſequence of this, and its remedies. The doctrine of optical aberration from figure, and method of obviating it; from Huyghens—Application to teleſcopes. Imperfection of compound lenſes from the want of perfect compenſation in the contrary powers of diſperſion through the whole ſpectrum. Diſcovery of an adequate remedy. Conſtruction of lenſes, in which the aberrations from figure and diſperſion of colour are both removed. Preſent ſtate of the invention.

II. A remarkable Effect of the Inflection of Light paſſing through Wire Cloth, not yet clearly explained — — — — p. 13

Singular appearance of a diſtant lamp ſeen through an handkerchief. Variation of the experiment—Inferences—Remarks on thoſe inferences, and new experiments. Deductions. Experiments with wire cloth. Extremely brilliant ſolar ſpectrum. Obſervations.

III. Deſcription of an Inſtrument which renders the Electricity of the Atmoſphere and other weak Charges very perceptible, without the Poſſibility of an equivocal Reſult — — — — p. 16

Hiſtory of the invention. Deſcription of a ſpinning inſtrument—Its power.

IV. Obſervations on the Art of printing Books and Piece Goods by the Action of Cylinders — — — — — p. 18

Three methods of writing—by the pen or bruſh—by the type or block—and by the engraved plate.

Letter-preſs deſcribed. Block printing. Diſtinctions between letter-preſs and block-printing. Copperplate printing. Cylinder printing. General view of the commercial impediments to the ſucceſs of inventions—Phyſical difficulties—particularly with regard to cylinders—Remedies. How far paper ſtainers and callico-printers are prevented from uſing machinery by the operations of the Exciſe Laws.

V. An Account of the Diamonds of Brazil. By M. D'Andrada — p. 24

Deſcription of the province of Brazil, which affords diamonds. Hiſtory of the diſcovery. Natural hiſtory of the diamond. Method of exploring.

VI. Abſtract of the Specification of Mr. William Deſmond's new Method of Tanning, with Obſervations relative to that Subject — — p. 26

Art of tanning. Method of obtaining the tanning principle, and the principle of aſtringency, from oak bark ſeparately—Criterions for diſtinguiſhing them. Proceſs with ſtrong hides—with lighter ſkins. Tanning propoſed inſtead of tar for ropes, and ſalt for fleſh meat. Reflections. Reſults of actual enquiry. Preſent ſtate of the invention.

VII. Deſcription and Account of a new Preſs operating by the Action of Water on the Principle of the Hydroſtatic Paradox. Invented by Joſeph Bramah, Engineer — — — — — 29

Deſcription of the inſtrument, and its mode of action. Computation of its force.—Compared with a ſcrew; in theory—and in practice.—Experiments or trials.

VIII. The Proceſs for giving a beautiful White Colour to Raw Silk, without Scouring. By M. Baumé — — — p. 32

Hiſtorical facts. Quality of Chineſe ſilk. Baumé's proceſs—1. Killing the chryſalis—2. winding off the ſilk on a reel. Apparatus for bleaching with marine acid and ſpirit of wine.—The proceſs.—Precautions in drying.—Chineſe ſilk probably bleached in this way.

IX. On the Hydrometer of Baumé — — — p. 37

Hydrometer for ſalts;—and for ſpirits. Deductions of the ſpecific gravities indicated by theſe inſtruments, diſpoſed in tables.

X. Obſervations on the Soap of Wool, and its Uſes in the Arts. By J. A. Chaptal, Inſtitutor of the Polytechnic School — — p. 40

Preliminary obſervations. Proceſs for making ſoap of alkali and wool. Enumeration of the facts. Choice and preparation of materials. Management of the operation. Singular effect of this ſoap on cottons.

XI. Extract of a Memoir concerning three different Species of Carbonated Hydrogenous Gas obtained from Ether and Alcohol by different Proceſſes, forwarded to the National Inſtitute of France by the Society of Dutch Chemiſts; being Part of a Report read to the Firſt Claſs of the Inſtitute by Citizen Fourcroy at the Sitting of the 26th Frimaire, 16th December 1796 — p. 44

General account of the olefiant gas. Proceſs for obtaining it. Phenomena.

Mathematical Correſpondence — — — p. 45

Scientific News — — — — p. 46

Revival of the Journal des Savans and the Annales de Chimie. Sitting of the French National Inſtitute. Accounts of foreign books.

MAY 1797.

I. Extract of a Memoir concerning three different Species of Carbonated Hydrogenous Gas obtained from Ether and Alcohol by different Proceſſes, forwarded to the National Inſtitute of France by the Society of Dutch Chemiſts; being Part of a Report read to the Firſt Claſs of the Inſtitute by Citizen Fourcroy, at the Sitting of the 26th Frimaire, 16th December 1796 [concluded] p. 49

Purification of olefiant gas—Phyſical properties—negative—affords an oil when mixed with oxygenated muriatic acid gas—Properties of this oil. Compoſition of the olefiant gas. It contains hydrogene and carbone. Appropriate name. Olefiant gas from ether treated with ſulphuric acid, or from alchol and ether paſſed through an ignited tube of pipe clay. Gas afforded by paſſing ether and alcohol through an ignited glaſs tube—is not olefiant, &c. Various experiments. Two other elaſtic fluids obtained from ether and alcohol. Recapitulation. Queſtions and enquiries relating to theory.

II. On the Methods of obviating the Effects of Heat and Cold in Time-pieces — — — — — p. 56

Inſtruments for meaſuring time. Hour-glaſs—Sand-clock—Water-clock—Common clock and watch—Trains of wheels—move uniformly when acted upon by a weight, becauſe friction prevents the accelerated motion. Of clocks regulated firſt by a fly—then by a balance—and laſt of all by a pendulum. Detached eſcapements and compenſations for temperature. Effect of temperature on clocks. Deal pendulum. Graham's quickſilver pendulum—improved by Troughton. Harriſon's gridiron pendulum. Elicot's lever pendulum—improved by Cumming. Fixed compenſations. Project for a gridiron balance. Curious contrivance of a compound bar which bends when its temperature is changed. Theoretical and practical conſiderations. Experiments with a bar of this kind. The expanſion balance deſcribed. Application of the ſame principle to pendulums. Inſtruction to actual workmen.

III. Obſervations and Experiments on the Light, Expence and Conſtruction of Lamps and Candles, and the Probability of rendering Tallow a Subſtitute for Wax — — — — — p. 67

Artificial light is an object of the firſt neceſſity—means of producing it—meaſure of its intenſity. Obſervations on lamps—purification of oil—acceſs of air—inconvenience of lamps. Obſervations on the candle.—Fuſibility, or freezing points of tallow, ſpermaceti, fatty matter of fleſh, pela, bees-wax and bleached wax. A lamp for tallow, very deſirable. Experiment.—Conſiderations reſpecting the ſnuffing of candles. Chineſe candle with wax on the outſide. Imitation. Experiments for rendering tallow leſs fuſible.

IV. A Memoir upon the Diſcovery of America. By Mr. Otto — p. 73

Introductory remarks. Hiſtorical documents reſpecting Martin Behem—who ſailed in 1484 from Portugal,

Portugal, eight years before Columbus, and diſcovered Brazil and other parts of South America Various collateral proofs.

V. Analyſis of the Oriental Lapis Lazuli. By M. Klaproth — p. 77

Former experiments enumerated. Analyſis. Blue colour not changed by ignition ;—but it becomes blueiſh grey with enamel. Other experiments in the dry and humid methods. Component parts of the ſtone.

VI. Uſeful Notices reſpecting various Objects—Roſe Water—Eau de Luce—Soap of Wool—Sea Sickneſs — — — p. 80

Roſe-water, how to be procured freſh at all times. Eau de luce, experiments for making. Repetition of Chaptal's operation for making ſoap of wool. Ether ſaid to be a cure for ſea ſickneſs.

VII. A Compariſon between Electrical Machines with a Cylinder, and thoſe which produce their Effect by Means of a circular Plate of Glaſs. With a Deſcription of a Machine of great Simplicity and Power, invented by Dr. Martinus Van Marum — — — — p. 83

Obſervations on the action of machines with a cylinder. Undulation of electricity. Deſcription of a machine with a plate of glaſs exhibiting both powers. Numerical eſtimates of the force of different electrical machines.

VIII. The Proceſs for giving a beautiful White Colour to Raw Silk, without Scouring. By M. Baumé. (Concluded from page 32.) — p. 88

Methods of recovering the alcohol after uſing it in bleaching—by adding alkali and diſtilling—or by diſtillation alone. Inſtructions. Difficulties of the new method of bleaching. Purification of vitriolic acid. Proceſs to obtain the marine acid.

Mathematical Correſpondence — — — p. 92

Scientific News — — — — p. 93

Analyſis of the four firſt cahiers of the Journal of the Polytechnic School.

New Publications — — — — p. 95

JUNE 1797.

I. A Letter from Mr. de Humboldt to M. Pictet, on the Magnetic Polarity of a Mountain of Serpentine — — — p. 97

Situation and appearance of the mountain. Experiments. Obſervations on the ſtone, which was forwarded to Sir Joſeph Banks with the memoir.

II. An Account of ſome Experiments upon Coloured Shadows. By Lieutenant General Sir Benjamin Thompſon, Count of Rumford, F. R. S. In a Letter to Sir Joſeph Banks, Bart. P. R. S. — — — p. 101

Difference in colour between candle-light and day-light.—Obſervations. Experiments with coloured glaſſes, candles and lamps. Senſations of colour afforded by mere contraſt. Intereſting effects.

III. A Memoir upon the Diſcovery of America. By Mr. Otto. (Concluded from page 77.) — — — — p. 107

Death of Chevalier Behem—Critical remarks and obſervations to prove that he was the diſcoverer of America.

IV. Deſcription of a Gravimeter, or Inſtrument for meaſuring the Specific Gravity of Solids and Fluids. By Citizen Guyton — — p. 110

On ſpecific gravities—and hydrometers. Obſervations of the hydrometer of Nicholſon. Deſcription of an improved inſtrument—Additional remarks. Formula for finding ſpecific gravities without diſtilled water, or the thermometer or barometer. Uſeful application of the inſtrument. Table of ſpecific gravities of alcohol. Specific gravities of alloys of tin and lead. Account of Gilpin's tables of ardent ſpirit.

V. Deſcription of the Improved Air-Pumps of Prince and Cuthbertſon. With Obſervations — — — — — p. 119

Familiar elucidation of the principles of the air-pump. Air-pumps with ſtop-cocks and with valves. Smeaton's pump. Brooks's experiments. Deſcription of Prince's air-pump, which has no lower valve or ſtop-cock. Advantages. Remarks on condenſing engines. Deſcription of Cuthbertſon's air-pump, in which the valves are all metallic—Its great power. Compariſon between theſe two pumps. Project for a pump which may unite the advantages of both.

VI. Uſeful Notices reſpecting various Objects.—A Method of preventing Heat in grinding. Concerning Gold, Silver, and other Metals reduced into very thin Leaves by the Hammer. Globules for Microſcopes. On the Plumb-Line and Spirit-Level — — — p. 131

Heat developed in grinding. A German grindſtone which does not heat. Experiments to inveſtigate this ſubject. Succeſs and advantages. Thickneſs of leaf gold, ſilver, braſs or Dutch gold, tin or Dutch ſilver, and tin-foil. Method of forming very clear ſpherules of glaſs. Breadth of the fineſt wire in ſeconds of meaſure. Spirit-level deſcribed. Its adjuſtment and degree of accuracy. How to procure good tubes for levels. Method of grinding the inſide of the tube. Imperfections of the ſpirit-level.

Mathematical Correſpondence — — — p. 137

Scientific News — — — — — p. 141

Memoirs of the Polytechnic School.

JULY, 1797.

I. Obſervations on Horizontal Refractions which affect the Appearance of Terreſtrial Objects, and the Dip or Depreſſion of the Horizon of the Sea. By Joſeph Huddart, Eſq. F. R. S. — — — p. 145

Elevation of points of land by refraction. Theory. Inverted appearance of a veſſel at ſea. Explanation. Remarkable difference in the intenſity of Portland Lights dependent on this theory. The dip of the viſible horizon is rendered uncertain by this refraction—Remedy. General annotation.

III. Re-

II. Remarkable Effect of Terreſtrial Refraction on a diſtant Headland.—Extract of a Letter from Andrew Ellicot to David Rittenhouſe, Eſq. dated at Pittſburg, Nov. 5, 1787, concerning Obſervations made at Lake Erie p. 152

Phenomenon called Looming. Preſqu' Iſle ſeen double, with frequent and ſingular changes.

III. Extract of a Memoir of Mr. Benedict Provoſt, of Geneva, on the Emanations of Odorant Bodies. By Citizen Fourcroy — — p. 153

Facts ſuppoſed to depend on odorant effluvia. Motions of camphor upon water. Inferences tending to eſtabliſh a theory.

IV. A Method of Meaſuring the Force of an Electric Battery during the Time of its being charged. By Lieutenant Colonel Haldane — p. 156

The charge meaſured by the number of exploſions of a jar connected with the external ſurface of a battery. Experiments.

V. On the Mechanical Conſtruction and Uſes of the Screw — p. 158

Short deſcription and theory of the ſcrew. Computation of its effect. Loſs by friction. Scientific uſe for meaſuring lines or ſpaces. How far it may be relied on. Account of the method of making ſcrews—by hand—by the tap and ſcrew-tool—and dies. Principal ſources of error in tapping:—1. Wave from change of hands—2. Drunk from preſſure downwards—3. Undulation from the firſt action of the dies—4. Bad fitting or unequal hardneſs of material—5. Setting up of the dies not being in the plane of the thread; and 6. Their cutting on one ſide only cauſes irregular depths and crookedneſs—7. Uncertain effects, by an action of the nature of wire-drawing. A ſcrew of forty threads in the inch will meaſure to greater preciſion than the hundred thouſandth part of an inch. Stock with four pair of dies. Its advantages.

VI. The Method of obtaining the Fixed Alkalis in Cryſtals of the greateſt Purity. By Lowitz, Profeſſor, &c. at Peterſburg — — p. 164

Deſcription of the proceſs, with numerous obſervations. Solution in alcohol does not afford pure alkali.

VII. Experiments on Eau de Luce. By a Correſpondent — p. 166

Trial of various ſubſtances.

VIII. An Account of Experiments deſcribed, and in part repeated, at the Sitting of the National Inſtitute of France on the 15th Germinal, in the Year 4. By Citizens Fourcroy and Vauquelin—On Detonations produced by Concuſſion — — — — — p. 168

Detonations afforded by the ſuper-oxygenated muriate of pot-aſh mixed—with ſulphur—with ſulphur and charcoal—crude antimony, zinc, metallic antimony, ſulphuret of iron, cinnabar, ſugar, gum, oil, alcohol, ether.

IX. A Memoir on the Combination of Oils with Earths, Volatile Alkali, and Metallic Subſtances. By M. Berthollet — — p. 170

Combinations of fat oil with lime—with ammoniac—with magneſia—with clay—with barytes—with mercury, zinc, tin, iron, copper, lead, ſilver, gold, magneſia, volatile oils, and copper. General facts and remarks.

X. Analyſis of a Memoir of Citizen Bonhomme on the Nature and Treatment of Rachitis, or the Rickets — — — — p. 174

Nature of Rachitis. Treatment with alkaline lotions—and phoſphate of lime internally. Obſervations on urine and its depoſitions—particularly of rachitic ſubjects.

XI. On the Nature of the Diamond. By Smithſon Tennant, Eſq. F.R.S. p. 177

Hiſtorical remarks on the combuſtibility of the diamond. Combuſtion with nitre in a veſſel of gold. Eſtimates of the quantity of fixed air or carbonic acid afforded by the diamond—It affords nothing but the fluid called fixed air.

XII. Uſeful Notices reſpecting various Objects—Improvement of Teleſcopes—Imperfections of Optical Glaſs—Purification of Mercury p. 180

Advantages of an iris, or changeable aperture to teleſcopes—Mechanical contrivances for that purpoſe. Method of examining glaſs by the microſcope. Experiments. Mercury purified by agitation.

XIII. A Memoir containing ſome Reſults ariſing from the Action of Cold on the Volatile Oils, and an Examination of the Concretions found in ſeveral of thoſe Oils. By Citizen Margueron, Member of the Societé des Pharmaciens at Paris — — — — p. 182

Effect of cold on the pure oils of peppermint, orange-flower, lemon-peel, bergamot, lavender, thyme, turpentine, and cinnamon. Alſo on thoſe oils mixed with water.

Mathematical Correſpondence — — — p. 186

Scientific News — — — — p. 188

Letter from Sir Benjamin Thompſon, Count of Rumford, F. R. S. to the Right Hon. Sir Joſeph Banks, Bart. K. B. P. R. S. announcing a Donation to the Royal Society for the Purpoſe of inſtituting a Prize Medal p. 188

Biennial premium of Count Rumford for the beſt improvement or diſcoveries reſpecting heat or light.

New Publications — — — — p. 190

Table of ſpecific gravities of water with common ſalt.

AUGUST 1797.

I. An Account of the New Syſtem of Meaſures eſtabliſhed in France. By Ch. Coquebert — — — — p. 193

Origin of the ſyſtem. Two univerſal meaſures, length of the pendulum, and magnitude of the earth. Preference given by the French Republic to the latter. Data, as at preſent known. Quadrant of the meridian from the equator to the pole, taken as the fundamental unity. Decimal ſubdiviſion. Nomenclature. Lines. Surfaces. Solids. Weights—monetary. Tables—1. Of the nomenclature of the new meaſures, and their relation to the old French and Engliſh. 2. Proportions of the new and old French meaſures. Annotation reſpecting the limits of error.

II. Analyſis of a Memoir of Citizen Bonhomme on the Nature and Treatment of Rachitis or the Rickets. (Concluded from p. 177.) — — p. 200

Singular experiments, in which oſſification was promoted by calcareous phoſphate. Application of this reſult to the caſe of rachitis. Account of cures. Inferences and reflections.

III. Abſtract of a Memoir read at the Sitting of the French National Inſtitute on the 26th Pluvioſe, containing an Account of ſome Experiments concerning the Section which Cylinders of Camphor undergo at the Surface of Water; with Reflections on the Motions which accompany this Section. By J. B. Venturi, Profeſſor of Natural Philoſophy at Modena, Member of the Inſtitute of Bologna, &c. — — — p. 205

Motions of camphor upon water. Section at the ſurface of water. Explanation of this fact—and of the rotation. Apparent caprice or uncertainty of the effects accounted for. Several other motions reſembling the preceding. Additional facts and reflections.

IV. Analyſis of four Specimens of Steel; with Reflections on the new Methods employed in this Analyſis. By Citizen Vauquelin — — p. 210

Account of the ſteel—and report concerning its qualities when made into tools. Excellence of the hook-tool. Preſent ſtate of the analyſis of ſteel—Its uncertainty and difficulties. New proceſſes with ſteel, to ſeparate phoſphorus, carbone, and mangane ſe. Inſufficiency of Bergman's method of ſeparating mangane ſe.

V. Deſcription of an Artificial Rock-Cryſtal produced in the Humid Way. By Mr. Trommſdorff, Profeſſor of Chemiſtry at the Univerſity of Erfurt p. 217

Previous remarks. Hiſtory of the fact. Rock-cryſtal ſeparated from liquor of flints during five years repoſe.

VI. Geological Obſervations on North-Wales. By Mr. Arthur Aikin — p. 220

No proper volcanic productions in North-Wales. Gradation of the ridges of lime, ſlate, ſtratified rocks, and granite in the greater chains of mountains. Figure of primitive, ſecondary, and derivative mountains. Beds of rounded pebbles on the tops of the ſlate mountains. Theoretical remarks. Metals. Coal. Primitive ſtate and ſubſequent changes in the face of the earth in North-Wales.

VII. An Account of the Fata Morgana; or the Optical Appearance of Figures in the Sea and the Air in the Faro of Meſſina. With an Engraving — p. 225

Deſcription of the phenomenon. Strange figures in the ſea—and in the air—ſometimes vividly coloured with priſmatic fringes. Remarks and obſervations.

VIII. A Memoir containing ſome Reſults ariſing from the Action of Cold on the Volatile Oils, and an Examination of the Concretions found in ſeveral of thoſe Oils. By Citizen Marguerón, Member of the Societé des Pharmaciens at Paris. (Concluded from p. 186.) — — p. 227

Nature of the concretions in volatile oils.

IX. On the Cold Winds which iſſue out of the Earth. By Profeſſor De Sauſſure and others; with Obſervations — — p. 229

Accounts of the cold caves of Mont Teſtaceo, Iſchia, St. Marino, Ceſi, Chiavenna, Caprino, and Hergiſweil; together with thoſe of Roquefort. Theory of the author; from ſuppoſed reſervoirs. Objections. Another explanation from the imperfect conducting powers of bodies. Similar facts in common buildings.

X. The Combuſtion of Phoſphorus in the Vacuum of the Air-Pump. By Dr. Martinus Van Marum — — — p. 236

Combuſtion of phoſphorus in oxygene. Unexpected ſpontaneous combuſtion in vacuo.

Mathematical Correſpondence — — — p. 237

New Publications — — — p. 238

SEPTEMBER 1797:

I. Experiments and Obſervations made with the View of aſcertaining the Nature of the Gaz produced by paſſing Electric Diſcharges through Water; with a Deſcription of the Apparatus for theſe Experiments. By George Pearſon, M. D. F. R. S. — — — — p. 241

Short account of the experiment of Paets Van Trooſtwyk and Dieman. Difficulty of repeating it. Inſtructions for ſucceſsfully conducting this proceſs.

II. Analyſis of Four Specimens of Steel, with Reflections on the new Method employed in this Analyſis (continued from p. 217). By Citizen Vauquelin p. 248

Experiments on the ſpecimens. Table of the quantities of carbone, ſilex, phoſphorus, and iron in each ſpecimen. Reflections on the variable quantities of carburet obtained from ſteel—on Bergman's method of diſcovering phoſphorus—and on the methods hitherto propoſed to diſcover and ſeparate manganeſe.

III. Extract of a Letter from Mr. Humboldt to Mr. Blumenbach, containing new Experiments on the Irritation cauſed by the Metals with reſpect to their different Impreſſions on the Organs of Animals — — p. 256

Strange effects of galvaniſm upon wounds occaſioned by a bliſtering plaſter. Obſervations. Other important facts and experiments reſpecting irritability.

IV. Uſeful Notices reſpecting various Objects.—Methods of cloſing wide-mouthed Veſſels—Preſervation of Gun-powder—Granulation of Shot—Precipitation of Magneſia — — — — p. 260

Wide-mouthed veſſels cloſed by tinfoil, oil, &c. Neceſſity of keeping gun-powder very dry. De-

ſcription of the common proceſs for making ſmall ſhot; and alſo of the manufacture of patent ſhot. Experiments and remarks on the means of precipitating magneſia in the moſt light and impalpable ſtate.

V. On the elaſtic Fluid contained in the Air-veſſels of Fiſh — p. 264

Oxygene found in the air-veſſel of the ſword-fiſh. Azote in other fiſh.

VI. Account of certain remarkable Changes of Colour and Direction of the Clouds during a Thunder Storm — — — p. 265

Circumſtances attending the ſtorm of July 30th, 1797. Inferences.

VII. The Method of making excellent Bread without Yeaſt; as practiſed at Debretzin, in Hungary. By Robert Townſon, L.L.D. F.R.S. Edin. p. 267

Account of a leaven which may be kept ſix months. Method of uſing it.

VIII. Deſcription and Uſe of an Eudiometer, with Sulphuret of Potaſh. By Citizen Guyton — — — — p. 268

The ſlowneſs and imperfection of Scheele's proceſs are removed by the application of heat. Very ſimple eudiometer.

IX. Deſcription of an improved Electrometer, in which the Senſibility of the Gold Leaf is conſiderably augmented, and the Intenſities are diſtinguiſhed by numerical Graduation — — — p. 270

The electrometer improved, by making the uninſulated ſide-pieces adjuſtable.

X. The improved Proceſs of Tanning. By Citizen Seguin — p. 271

Art of tanning. Lixiviations of tan. Tanning principle and gallic acid. Inſoluble compound of gelatin and tanin. Other intereſting facts: tanin becomes oxided. Structure and compoſition of ſkin. Fibrous matter is oxygenated glue, and muſt be diſoxygenated before it will tan. Reaſons why the proceſs ought to be gradual; but the time may be as ſhort as not to exceed a fortnight. Important conſiderations reſpecting the new properties ſtuffs acquire by tinctorial proceſſes.

XI. The Combuſtion of Phoſphorus in the Vacuum of the Air-Pump. By Dr. Martinus Van Marum (Concluded from p. 237.) — p. 279

Inflammation of phoſphorus in vacuo, when wrapped in cotton powdered with reſin. Analytical experiments, which ſhew that the cotton clothing is chiefly concerned. Phenomena attending this combuſtion.

Mathematical Correſpondence — — — p. 283

Queſtion reſpecting a glory ſeen in water. Difficulty ſtated relative to the Fata Morgana.

New Publications — — — — p. 285

OCTOBER

OCTOBER 1797.

I. An Account of the Manner in which Heat is propagated in Fluids, and its general Conſequences in the Economy of the Univerſe. By Benjamin Count of Rumford — — — — p. 289

General view of the theory of heat. Equilibrium of temperature—Capacities—in the ſtates of ſolidity, fluidity, and elaſticity—altered by change of combination. Conducting powers. Fluids affirmed to be non-conductors.—Inſtances: with large thermometers—with an apparatus contrived for the ſame purpoſe. Conducting power of fluids impaired by mixture of fibrous or glutinous matter. Clothing is more effectual for preſerving the temperature of bodies in water, than even in air. General application of the experiments to the power which animals and vegetables poſſeſs, of preſerving their temperature.

II. Experiments and Obſervations on the fulminating Preparations of Gold and Silver — — — — p. 296

Fulminating gold—Silver—Proceſs. Great danger from this experiment, with ſtrong ſolution of ammoniac. Prince Rupert's drop. Danger of the powder of oxygenated muriate of potaſh and ſulphur, from ſpontaneous exploſion.

III. Experiments and Obſervations made with the View of aſcertaining the Nature of the Gaz produced by paſſing electric Diſcharges through Water, with a Deſcription of the Apparatus for theſe Experiments. By George Pearſon, M.D. F.R.S. (Continued from p. 248.) — p. 299

Decompoſition of water by interrupted diſcharges—and by complete diſcharges. Mode of action.

IV. Experimental Reſearches to aſcertain the Nature of the Proceſs by which the Eye adapts itſelf to produce diſtinct Viſion — p. 305

Mr. Young's obſervations on the muſcularity of the cornea. Mr. John Hunter's diſcovery of the ſame fibrous ſtructure. Dr. Hoſſack's inveſtigation of the effect of the external muſcles of the eye. Mr. Home's lecture on the eye. Mr. Ramſden's obſervations, tending to ſhew the true purpoſe anſwered by the ſtructure of the cryſtalline. Very ſtriking inſtance of adjuſtment in the eye, after extraction of the cryſtalline. Experiments to ſhew, that the action of the ſtrait muſcles cauſes a variation in the cornea, at the inſtant of adjuſtment, and of the ſame kind as that adjuſtment demands. Recapitulation.

V. Concerning the Properties of the Sulphureous Acid, and its Combinations with Earthy and Alkaline Baſes. By Citizens Fourcroy and Vauquelin p. 313

Phyſical properties of the acid—chemical properties with caloric; oxygene; water; other acids; combuſtible bodies; alkalis.

VI. A Memoir on the Nature of the Alum of Commerce, on the Exiſtence of Potaſh in this Salt, and on various ſimple or triple Combinations of Alumine with the Sulphuric Acid. Read before the National Inſtitute of France. By Citizen Vauquelin — — — — — p. 318

Theory of the effect of alkalis in alum-making, requires elucidation. Pure ſulphate of alumine does not cryſtallize. It cryſtallizes by the addition of a few drops of ſolution of potaſh;—but not by carbonate of ſoda; nor lime; nor barytes. The addition of potaſh, or ammoniac, does not ſerve to ſaturate an exceſs of acid: their ſulphates anſwer the ſame purpoſe, even though they may have

have exceſs of acid. Cryſtallized alum is a triple ſalt, and contains a notable proportion of potaſh, or ammoniac. Analyſis of the alums of commerce. General reſults of much importance to the arts, &c.

VII. Deſcription of an Inſtrument proper to meaſure the Volume of a Body, without plunging it in any Liquid. By H. Say, Capitaine du Genie p. 325

Determination of the bulks of bodies, by immerſion in air inſtead of water or other liquids. Limits of error in this method.

VIII. Uſeful Notices reſpecting various Objects.—Styrian Steel—Elaſtic Strings for Muſical Inſtruments, and other Purpoſes. Wheels without Cogs p. 328

Styrian ſteel is made from any kind of iron. Proceſs. Silk ſubſtituted for catgut. Wheels acting on each other by the end grain of wood.

IX. An economical Proceſs to obtain pure cauſtic Alkali in the large way, with fuſed Potaſh, or the Lapis Cauſticus. By Citizen Bouillon Le Grange — — — — p. 329

Apparatus. Depuration of alkali by ſlaking with lime, percolation of water, and ſubſequent evaporation in an iron veſſel. Berthollet's method with alcohol.

X. A Table for reducing the Unities of the Metre, Litre, and Gramme, into Engliſh Inches, Gallons, and Grains — — p. 332

Mathematical and Philoſophical Correſpondence — — p. 333

Mr. Varley's ſcheme for a perpetual motion.

Publications on Galvaniſm — — — p. 333

NOVEMBER 1797.

I. Experiments and Obſervations on the Nature of Sugar. By William Cruickſhank, Chemiſt to the Ordnance, and Surgeon of Artillery p. 337

Deſtructive diſtillation of ſugar—and of gum arabic. Pneumatic experiments on malting. Oxygene is abſorbed; and is abſolutely neceſſary. Experiments for converting ſugar into mucilage, by abſtracting oxygene. Unſucceſsful attempts to convert gum into ſugar.

II. An Account of the Manner in which Heat is propagated in Fluids, and its general Conſequences in the Economy of the Univerſe. By Benjamin Count of Rumford (Continued from p. 296.) — — p. 341

The motion of a fluid while changing its temperature, ſhewn in a ſtriking manner by floating pieces of amber. The upper part of a fluid may boil without heating the reſt. Experiments with ice and water, by which it is ſhewn, that water at 40 degrees will melt as much ice while ſtanding on its ſurface, as an equal volume of boiling hot water.

III. Experiments and Obſervations made with the View of aſcertaining the Nature of the Gaz produced by paſſing electric Diſcharges through Water; with

with a Deſcription of the Apparatus for theſe Experiments. By George Pearſon, M.D. F.R.S. (Concluded from p. 305) — p. 349

Obſervations and inferences concerning the effect of electricity on water, in the experiments recited; and the general doctrines of chemiſtry which they tend to explain.

IV. Obſervations on the Electrophore, tending to explain the Means by which the Torpedo, and other Fiſh, communicate the electric Shock p. 355

Action of the electrophore. Experiments with electrophores of talc.—Computations.—Figure and dimenſions of the electric organs of the Torpedo. Induction of their capacity or charge. Facts, which ſhew that a mechanical Torpedo might be made, capable of giving innumerable ſhocks, and retaining its power for an unlimited time. Conjectures on the actual means of operation in the Torpedo.

V. A Letter from Mr. Von Humboldt, to Mr. H. Van Mons, on the chemical Proceſs of Vitality; together with the Extract of a Letter from Citizen Fourcroy, to Citizen Van Mons, on the ſame Subject — p. 359

Experiments on the effects of chemical agents upon the irritability of the limbs of animals.—Caution of Citizen Fourcroy againſt multiplying hypotheſes.

VI. Concerning the Properties of the Sulphureous Acid, and its Combinations with Earthy and Alkaline Baſes. By Citizens Fourcroy and Vauquelin (Concluded from p. 318.) — — — — p. 364

Habitudes of the ſulphates of ſoda, of ammoniac, of magneſia, of barytes, and of alumine.

VII. An Account of the Great Copper-Works in the Iſle of Angleſey. By Mr. Arthur Aikin — — — — p. 367

Account of the mines. They are for the moſt part open quarries. Nature of the ores; working proceſſes; number of men employed. Port and town of Amlwch. Aſpect of the ſhore. Village of Cemmaes; eſtimable manners of the inhabitants.

VIII. A Method of diſpoſing Gunter's Line of Numbers, by which the Diviſions are enlarged, and other Advantages obtained — p. 372

Theorems. Conſtruction of a Gunter's line, in which the diviſions are enlarged eight times.—Conſtruction of a ſpiral logarithmic line, ſeven inches in diameter, but of equal power to that of a Gunter's rule 40 feet long.

IX. On the mechanical Projects for affording a perpetual Motion p. 375

What is a perpetual motion? Projects of Biſhop Wilkins—of the Marquis of Worceſter—of Orfyreus—of Dr. Shiviers. Conſiderations on the cauſes of theſe deceptions. Inſtrument of Deſaguliers, and of the author, to ſhew the fallacy of the notion from which projectors of ſuch ſchemes reaſon. Eſſential requiſite to a perpetual motion. Enumeration of ſuch natural agents as may keep a machine in motion, as long as it retains its form, or till it is worn out.

X. Uſeful Notices reſpecting various Objects—Silver alloyed with crude Platina—Tempering of Steel—Rifled Shot — — p. 380

Attempt to unite ſilver and platina. Sudden increaſe of ignition at the inſtant of congealing. On the hardneſs and tenacity of ſteel. Tempering by the colour;—by blazing;—and by oil. Rifled ſhot. Experiments which ſhew that they revolve, but do move with more preciſion than round ſhot.

Mathematical Correſpondence — — — p. 383

DECEMBER 1797.

I. Concerning the ſpontaneous Action of concentrated Sulphuric Acid on Vegetable and Animal Subſtances; its Action upon Alcohol, and the Formation of Ether. By Citizens Fourcroy and Vauquelin — p. 385

Ancient and pneumatic theories erroneous. Action of ſulphuric acid on vegetables—ſuppoſed to deprive the acid of oxygene—but does not. The acid attracts oxygene and hydrogene, and becomes diluted; the vegetable depoſits coal, and forms acetous acid. The diſpoſing affinity. Synthetical inferences. Action of ſulphuric acid on ſome vegetable and animal ſubſtances is more complicated. New path opened for the analyſis of vegetables. Popular theory of ether erroneous. Experiment detailed. Inductions. The true theory, as pointed out by experiment.

II. On the Multiplier of Electricity. By the Inventor, Mr. Cavallo: with Obſervations — — — — p. 394

Quotation reſpecting the ſpinning inſtrument. It differs in principle from the multiplier, and is a condenſer. The multiplier does not demand a ſtock of electricity, but operates by means of an abſolutely ſmall quantity. Hiſtory of inſtruments for ſhewing weak electricity. Compenſations in the jar, the conjugate conductors, the can and chain, the electrical well, ribbands, and ſilk ſtockings, vindicating electricity, and the electrophore. Firſt proceſs of doubling. Volta's condenſer. Bennet's doubler. Mechanical doublers of Darwin and Nicholſon. Their ambiguity. Cavallo's very perfect condenſer,—does not remove the ambiguity of the doubler—neither does the ſpinning inſtrument, which is an improvement on the condenſer. Cavallo's multiplier; operation and power. Uncertainty of the doubler exiſts in the other inſtruments, though the doubler alone can ſhew it.

III. A Memoir on certain Methods of Economy, and Improvement in the Manufacture of Hats. By Citizen Chauſſier — — p. 399

Political conſiderations. Art of the hatter. Operation of felting explained by Monge. Why cock and hen feathers are inferior to thoſe of geeſe. Probable remedy. Sulphuric acid in the bath for felting hats. Advantages. Stiffening.

IV. Doubts concerning the Exiſtence of a new Earth in the Mineral from New South Wales, examined by Wedgwood in the Year 1790 p. 404

Examination of the Sydney earth, by Klaproth—by Wedgwood—Contraſt of the experiments of both. They did not examine the ſame ſubſtance.

V. A philoſophical Memoir, containing, 1. Experiments relative to the Propagation of Sound in different ſolid and fluid Mediums.—And, 2. An experimental Enquiry into the Cauſe of the Reſonance of Muſical Inſtruments. By Mr. Perrole: with Annotations — p. 411

Tranſmiſſion of ſound through wood—metals—ſtrings—and other ſolids;—through fluids. Reſonant property of various bodies. Undulations of the air, and of ſonorous bodies. Whether air be the medium of ſound. Why ſound is beſt heard in the night. Obſervations relating to an acouſtic tube. Sonorous bodies vibrate. On the external ear, and the tympanum. General requiſites of an inſtrument for magnifying ſound.

VI. Concerning the Steam Engine, as originally invented by the Marquis of Worceſter, and the Improvements ſince made in Steam Engines without the Piſton, or Lever. With a Deſcription of an Engine of this Kind conſtructed by Mr. Peter Keir, of Kentiſh Town — — p. 419

Marquis of Worceſter's deſcription—Savery's engine. Hiſtory and adjuſtment of the claims of the Marquis of Worceſter and Savery as to the invention of the ſteam engine. Papin's ſteam engine. The engine with a piſton. Compariſon of Savery's and Newcomen's engines. Keir's engine on Savery's principle. Deſcription. Peculiarities. Steam engine with lifts.

VII. On the Mechaniſm by which the Mariner's Compaſs is ſuſpended p. 426

Introduction. Suſpenſion of the magnetic needle on a cap—requiſites for ſteadineſs. Compaſs-box ſupported on a point—or by gimbals. Adjuſtment of the gimbals for ſteadineſs. Eaſy trial of a good compaſs. Lorimer's ſuſpenſion of the dipping needle by gimbals.

VIII. On the maintaining Power in Clocks and Watches — p. 429

Vibrations of pendulous bodies—Train of wheels.

Mathematical and Philoſophical Correſpondence — — p. 430

Optical queſtion concerning the glory in water anſwered. Viſible emanation from rectified animal oil. Anſwers to mathematical queſtions.

JANUARY 1798.

I. Experiments made with a View to aſcertain the Cauſe of Buildings, which have metallic Conductors belonging to them, being ſtruck by Lightning. By Lieutenant-Colonel Haldane — — p. 433

General remarks. Deſcription of the apparatus. Experiments. Concluſion.

II. New Conſtruction of the Air Pump. By James Sadler, Eſq. Chemiſt to the Admiralty — — — — — p. 441

Two conſtructions of air-pumps, in which the vacuum is rendered more perfect by the interpoſition of a fluid.

III. Obſervations on Phoſphorus. By Citizen Brugnatelli, Profeſſor of Chemiſtry, &c. at Pavia — — — p. 444

Solution of phoſphorus in oxygene gas becomes luminous by the addition either of the oxygenated muriatic acid gas or nitrous gas. Phoſphorus diſſolved in hydrogene gas—Appearances of phoſphorus in oxygenated muriatic acid gas—in carbonic acid gas—and atmoſpheric air. Solution in oil of turpentine, and alcohol; effects of water on the latter ſolution—and of other fluids. Phoſphorated ether. Recapitulation.

IV. On the Advantage of inverting the Slider in many Operations on the common Sliding Rule. By the Rev. W. Pearſon, of Lincoln p. 450

Methods of working arithmetical queſtions on Gunter's rule with the inverted ſlider.

V. Abſtract of a Memoir entitled "Enquiries concerning the Nature of Pruſſian Blue." By Mr. Prouſt — — — — p. 453

Iron is not oxidable at the intermediate terms between the extreme proportions of oxygene. It affords two ſulphates only; the green cryſtallizable, and the red not cryſtallizable; and there is no intermediate ſalt. Properties of theſe. The green ſulphate affords a white pruſſiate; which rapidly abſorbs oxygene and becomes blue. The red ſulphate affords the moſt lively blue pruſſiate. The yellow oxide of iron is as completely ſaturated with oxygene as the red. Pruſſian blue is diſoxygenated, and converted into the white pruſſiate, by keeping in a cloſe veſſel with water and plates of iron or tin. Other facts concerning the definite oxidation of metals. Uſeful obſervations on ink and the black dye.

VI. An Account of ſome Experiments to determine the Force of fired Gunpowder. By Benjamin Count of Rumford, F.R.S. M.R.I.A. p. 459

Gunpowder fired in a cloſe veſſel. Attempts to meaſure its elaſtic force. Detail of the apparatus and experiments. An heavy weight was applied to the mouth of a metallic barrel, in order that the exploſion might tend to raiſe it. When the force was inſufficient to raiſe the weight, it did not continue to exert the ſame action after the exploſion as at the inſtant of that event. Singular hard black reſidue, of a pungent alkaline taſte and hepatic ſmell—concluded to have been in the elaſtic ſtate during the exploſion. A barrel ¼ inch bore, and 2¼ inches external diameter, burſt by 28 grains of gunpowder. Experiments and deductions from which it is inferred that a force of 54750 atmoſpheres was exerted in producing this effect. Other experiments. Table of reſults.

VII. Ob-

VII. Obſervations and Experiments on Steel reſembling that of Damaſcus; with an eaſy Teſt for determining the uniform Quality of Steel before it is employed in Works of Delicacy or Expence — — p. 468

Deſcription of a ſabre of Damaſcus. Its reputed properties. Suppoſed proceſs for making Damaſcus ſteel. Experiments by which a ſimilar ſteel was made. Inferences which explain the properties of this ſteel. Examination of all kinds of ſteel by the application of a weak acid. Great utility of ſuch an examination.

VIII. On the Irritability of the Pollen of Plants. With an Account of a Compoſition for cloſing wide-mouthed Veſſels (continued from page 313) p. 471

Irritation of pollen by ardent ſpirit. Wide-mouthed veſſels cloſed by ſpermaceti and caoutchouc.

IX. Experimental Reſearches to aſcertain the Nature of the Proceſs by which the Eye adapts itſelf to produce diſtinct Viſion — — p. 472

Imbricated texture of the ſclerotica in the eyes of birds. Obſervations. Probability that ſome birds are directed in their flight by the re-action of the air rather than by ſight. Inſtance of bats deprived of their eyes, and ſuppoſed to have a ſixth ſenſe. Mr. Home's lecture. 1. Examination of the variable convexity of the cornea, by obſerving the image in its virtual focus. 2. Attempts to diſcover whether the axis be elongated. 3. Probability that the eye is adjuſted by changes in the curvature of the cornea and in the axis of viſion, together with a motion of the cryſtalline lens. 4. Peculiarities of the eyes of quadrupeds—are ſuch as increaſe their power of ſeeing near objects. 5. Peculiarities in birds;—tend to increaſe the ſteadineſs of adjuſtment, and adapt the eye to very near objects. 6. Peculiarities in the eyes of fiſh.

New Publications — — — — p. 479

FEBRUARY 1798.

I. An Attempt to accommodate the Diſputes among the Chemiſts concerning Phlogiſton. In a Letter from Dr. Mitchill of New York, to Joſeph Prieſtley, LL.D. F.R.S. &c. &c. Dated November 20, 1797 — p. 481

Ignited charcoal urged by the eolipile. Suppoſed combuſtion of water. Objections to the term hydrogene—inſtead of which the word phlogiſton is propoſed. Nomenclature. Preſumption that hydrogene exiſts in ſulphur, phoſphorus, zinc and iron;—but not as a conſtituent part.—Concluſion. Annotations. On the action of ſteam from the eolipile. The word phlogiſton. Whether the appearance of flame neceſſarily indicates the preſence of hydrogene. Remarks on the difference between the functions of Dr. Mitchill's phlogiſton and that formerly maintained by Kirwan.

II. Experiments on the Compoſition and Proportion of Carbon in Bitumens and Mineral Coals. By Richard Kirwan, Eſq. F.R.S. L. & E. M.R.I.A. &c. p. 487

Uſes of mineral coal—On the combuſtion of coal—and its decompoſition by nitre. The carbon in mineral coals is proportioned to the nitre decompoſed by equal quantities of each. Analytic courſe grounded upon this fact. Kilkenny coal. Table of Kirwan's Numbers uſed to diſtinguiſh the luſtre, tranſparency, fracture and hardneſs of minerals. Maltha. Aſphalt. Cannel coal. Slaty Cannel coal. Whitehaven coal. Wigan coal. Swanſey coal. Leitrim coal. Newcaſtle coal. The reſults tabulated.

III. Obſervations on the beſt Methods of producing artificial Cold. By Mr. Richard Walker — — — — p. 497

Preparation of ice in powder. Congelation of mercury in a few minutes where the air is not hotter than 85°. Cheaper proceſs. Table of freezing mixtures, and the cold produced by them. Various repetitions of the experiments. Freezing by ether. Deſcriptions of the apparatus uſed in the preceding experiments.

IV. The Deſcription of a new portable Electrical Machine. Invented by the Rev. W. Pearſon of Lincoln — — — p. 506

Deſcription and uſe of the machine. Eſtimate of its power, &c.

V. A Memoir concerning a remarkable Phenomenon in Meteorology. Read to the Society of Naturaliſts of Geneva. By M. de Sauſſure, October 1797. p. 511

Times of greateſt dryneſs are the precurſors of rain. Explanation of the cauſe.

VI. On the various Denominations given to the Alkali of Tartar. By a Correſpondent — — — — p. 513

Remarks on the terms kali, fixed vegetable alkali, potaſh, ſpodium and tartarin. Preference given to the laſt.

VII. An Account of ſome Experiments to determine the Force of fired Gunpowder. By Benjamin Count of Rumford, F.R.S. M.R.I.A. (concluded from p. 468) — — — — p. 515

Why fire-arms do not burſt by the extreme force of gunpowder. Much of the gunpowder is blown out of fire-arms unburned. This was probably on fire, but extinguiſhed by rapid motion through the air. Direct experiments by firing gunpowder againſt a number of paper ſcreens. Remedies for the ſlow combuſtion of gun-powder. Windage. Probability that the force of gunpowder is owing to ſteam.

VIII. Obſervations on Strontian. By Citizen Pelletier. Read to the National Inſtitute, April 30, 1796 — — — p. 158

Diſcovery of ſtrontian. Klaproth's account of the diſtinctive character of carbonate of ſtrontian. Compariſon of ſtrontian and barytes. Proceſs for ſeparating the carbonic acid from the carbonates of barytes and ſtrontian. Habitudes of the carbonates of barytes and ſtrontian.

IX. Philoſophical News, and Accounts of Books — — p. 523

Garnerin's deſcent by a parachute from an elevation of 600 feet. Theory of the parachute.

MARCH

MARCH 1798.

I. Obſervations on Strontian. By Citizen Pelletier. Read to the National Inſtitute, 30th April 1796 (concluded from p. 522) — p. 529

Carbonates of barytes and ſtrontian treated with acids. The nitrates and the muriates of ſtrontian and of lime afford a red flame with alcohol.—Strontian contains no lime—It is not precipitated by pruſſiates, like barytes—Conſtituent parts of the native carbonates. General concluſions. Additional comparative experiments.

II. Abſtract of two Memoirs on a Method of obtaining Barytes pure, and on the Properties of this Earth compared with thoſe of Strontian. By Citizens Fourcroy and Vauquelin. Read to the National Inſtitute, 30th April and 21ſt September 1796 — — — p. 535

Properties of the pure barytes, obtained by igniting the nitrate. It is fuſible, effloreſcent, ſoluble in water, cryſtallizable, &c. Properties of ſtrontian chiefly with reſpect to its difference from barytes.

III. Extract of a Letter from Count Muſſin Puſchkin, Vice-Preſident of the Department of Mines at Peterſburg. On the Salts, Precipitates and Amalgam of Platina; on Cobalt; on Antimonial Soap; and on the Decompoſition of Soap by the acid Extracts of colouring Matters — — p. 537

Brick-coloured precipitate by ſal ammoniac is ſoluble in water. Other precipitates. Amalgam. The ſingular properties. Cobalt. Antimonial ſoap. Soap decompoſed and charged with vegetable colour.

IV. Obſervations on the fundamental Property of the Lever. With a Proof of the Principle aſſumed by Archimedes in his Demonſtration. By the Rev. S. Vince, A.M. F.R.S. — — — — p. 541

Former demonſtrations enumerated and examined. New demonſtration.

V. Obſervations on the Acid of Tin, and the Analyſis of its Ores. Read at the Sitting of the Claſs of Mathematical and Philoſophical Sciences of the National Inſtitute of France, the firſt of Meſſidor, in the Year 5. By Citizen Guyton — — — — — p. 543

Acidification of tin. State of this metal in certain ores. Analyſis and obſervations.

VI. On Fairy Rings — — — — p. 546

Figure and cauſe of fairy rings. Circles of burnt graſs produced by lightning.

VII. Experimental Reſearches to aſcertain the Nature of the Proceſs by which the Eye adapts itſelf to produce diſtinct Viſion — — p. 547

Peculiarities of the eyes of fiſhes. How the eye is adjuſted. Its diſeaſes. Indiſtinctneſs. Double viſion. Squinting. Diſeaſes of the cornea treated by ſtimulating applications. Caſe of Tobit in the Apocrypha confirmed by modern practice.

VIII. Experiments and Obſervations on the Inflexion, Reflexion, and Colours of Light. By Henry Brougham, jun. Eſq. — — p. 551

Propoſitions reſpecting the means by which light is made to alter its direction. Experiments and obſervations. The leaſt refrangible rays are ſuch as deviate moſt by inflexion—and alſo by deflexion. Flexion, refraction and reflexion are performed by a force acting at a definite diſtance. Coloured ſpectrum by reflexion—Order of the colours. They are not produced by any new modification, but by the ordinary decompoſition of light. The colour-making rays thus ſeparated are homogeneous and unchangeable. The leaſt refrangible rays are the moſt reflexible, or rebound neareſt the perpendicular. Spectrum by reflexion is divided in the ſame proportions as the Newtonian ſpectrum by refraction. Meaſurement of the colours. On the phyſical cauſe of reflexibility.

IX. An Account of the Manner in which Heat is propagated in Fluids, and its general Conſequences in the Economy of the Univerſe. By Benjamin Count of Rumford. (Concluded from p. 348) — — p. 563

Experiments of the fuſion of ice by water ſuffered to repoſe upon it. Precautions. Reſults with boiling hot water, exhibiting the quantities of ice melted in a given time. Experiments with cold water at 41 degrees, by which a greater quantity of ice was melted than by boiling water. General conſequences applied, at length, to the diſtribution of heat over the ſurface of the globe, by virtue of the internal motions of water, and currents which take place in the ſeas, ſimilar to the trade winds in the atmoſphere.

X. Uſeful Notices reſpecting various Objects.—Welding of Caſt-Steel—Flexure of Compound Metallic Bars by Change of Temperature — p. 575

Sir Thomas Frankland's method of welding caſt-ſteel to iron. Experiments to determine the proportion between the thickneſs of compound metallic bars and their flexure by heat.

SUPPLEMENT.

I. Obſervations on Water-ſpouts ſeen from Nice. By Mr. Michaud, Correſpondent with the Royal Academy of Sciences at Turin — p. 577

State of the weather previous to the appearance of water-ſpouts. Vaporous foot of a water-ſpout, unaccompanied by the ſpout itſelf. Deſcription of another water-ſpout of extreme magnitude, the lower part of which threw out large ſtreams of vapour and jets of ſea water. Termination of water-ſpouts on their approach to the ſhore. Inſtance of the foot and ſtem being formed at ſeveral miles diſtance from each other; the former of which remained ſtationary on the ſea until the winds had brought the cloud from which the latter was to iſſue, immediately over the maſs of vapour. Additional facts. Obſervations; and explanation of the drawings.

II. Experiments and Obſervations on the Inflexion, Reflexion, and Colour of Light. By Henry Brougham, jun. Eſq. (Concluded from p. 563) p. 585

Explanation of phenomena dependent on the flexibility of light. Interference of the penumbræ of bodies. Colours of light through mediums partially tranſparent. Colours of a flame. Blue ſhadows. Coloured fringes. Colours ſurrounding the ſun's image—and the flame of a candle. Sir Iſaac Newton's coloured rings by reflexion from a concave glaſs mirror. Admeaſurement of the flexibilities of the ſeveral primary rays. Summary of optical ſcience. Explanation of various phenomena dependent on reflexibility. Bright ſtreaks from a candle when the eyes are nearly ſhut. Other ſtreaks variouſly coloured formed by reflexion from ſurfaces of a fibrous ſtructure. Colours from ſcratches in a piece of metal;—from the minute particles of unpoliſhed bodies;—from hairs, ſpiders' webs, certain foſſils, &c. Whether theſe principles be ſufficient to explain the natural colours of bodies. The rays of light are not variouſly reflexible in the manner taught by Newton in his celebrated experiment of internal reflexion by a priſm. In that experiment the rays were variouſly reflected, for no other reaſon than becauſe their incidences were different. Images formed by reflexion of one uniform colour. Experiment and obſervations to ſhew that the natural colours of bodies may be produced in this way. Colours of thin plates by reflexion. Summary of propoſitions.

III. On certain Points of Nomenclature. By a Correſpondent — p. 597

New Publications — — — — p. 599

JUL 5 - 1939